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THE FUNCTION OF CLOTHING AND OF BODILY ADORNMENT[1]

By Herbert C. Sanborn, Vanderbilt University

One of the popular questions of the hour concerns the style of woman's dress. For woman herself this is said to have furnished themes of perennial interest well beyond the ken of psychologist and philosopher. But for the latter, too, the general problem of clothing has been at various times a puzzling subject of reflection. At the present moment it may have for them a more especial attraction, inasmuch as the short skirt and the short hair of the present generation have come to be confidently linked by reformers with certain other alleged short-comings said to have developed *pari passu*. Proceeding on the usual assumption that clothing and modesty stand in a one-to-one correspondence, it would be indeed easy to arrive at the conclusion that the recent shortening of the skirt is the explanation for an alleged moral turpitude, or vice versa—an opinion widely held by the man in the street.

If we may credit newspaper reports, this view of the matter has been the ground for attempted regulation of dress and head-dress of employees in certain American business houses, the theme for occasional sermons from the pulpit concerning "the sacredness of the body," while women have been forbidden to enter certain churches unless attired to suit the regulations of those in charge. Moreover, we are informed that far from the influence of Puritanism, near the region where Spartan youths without moral jeopardy wrestled naked with naked Spartan girls and Greeks in general were lightly clad, the maid of Athens has had the proper length of her skirt prescribed for her by some modern Draco. On the other hand, however, it is perhaps not absolutely unworthy of mention in passing that other reformers in Europe, reacting against what they call effete civilization, are seriously developing cults for the abolition of clothing altogether except for outdoor protection against the weather. Apparently there is a clothing problem that is not settled and will hardly be settled by the present article.

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[1] Read at the annual meeting of the Southern Society for Philosophy and Psychology, held at the University of Kentucky, April 3, 1926.

The view that clothing is a sign of modesty is at least as old as the ancient account of the predicament of our first parents, which is paralleled by the traditions of certain Indian tribes of a period when no clothing was worn by their ancestors.[2] Such accounts seem to suggest that the feeling of shame is an acquired habit; but together with other data they at any rate bear witness to the fact that man has long wondered why he began to wear clothes. As a result of such speculation, especially in recent times, attempts have been made to find some plausible explanation for the custom that shall rest on a basis of scientific fact. When we turn, however, to the conclusions drawn from a consideration of the data furnished by anthropological research we find here as elsewhere varying and even antithetical interpretations.

With investigators like Westermarck, Hirn, and Ernst Grosse, theory has even swung to the opposite extreme of the view which finds in natural shame the impulse to clothe the body, arriving then at an interpretation which imputes the first use of clothing to coquetry or exhibitionism. At first sight such a startling view may seem all of a piece with the perversity that has succeeded, as Kuno Fischer said, in producing "a merciful Nero, a wise Caligula and a virtuous Henry VIII," but the mere fact that it is subscribed to by men of the type indicated makes it in any case worthy of careful consideration.

The genetic basis for the theory is found in an alleged sexual apathy on the part of the male and a certain coquetry on the part of the female, claimed to be practically a universal phenomenon of wild animal life. These are held to be an explanation for the secondary sex characteristics and the courting behavior of some animals, which indeed also led Darwin to his theory of animal aesthetics and the auxiliary hypothesis of sexual selection. Moreover the theory seems to bring these facts into line genetically with the use of various kinds of ornamentation in savage and civilized life.

The function of jewelry is said to be the attraction of attention to the beautiful hand, arm, or head, rather than to the ornament itself; and this is so much the case that the effect of excessive ornamentation is to defeat its purpose by holding the attention too exclusively; a large diamond, as Hirn says, prevents the beautiful fingers and hand from being noticed, so that women of taste have a natural repugnance for large rings, stones, bracelets, and ear-rings. In the case of primitive peoples, accustomed to being naked and with whom clothing, at times at least, is indistinguishable in its function from ornament, it is alleged that the effect of certain articles worn is to call attention emphatically to the pudenda that might otherwise remain unnoticed or at least sexually ineffective.[3]

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[2] Cf. E. Westermarck, *The history of human marriage*, 5th ed., I, 540.

[3] Y. Hirn, *Der Ursprung der Kunst*, 1904, 205; cf. also E. Selenka, *Der Schmuck des Menschen*, 1900, 20; and Heinrich Schurtz, *Grundzüge einer Philosophie der Tracht*, 1891, 141

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Westermarck utilizes in his exposition vast resources of material gleaned from the accounts of explorers and anthropologists in all parts of the world concerning the self-decoration of savages; and he lays especial stress on the claim that there is no people so rude as not to take pleasure in ornament though destitute of almost everything that civilized people regard as the necessities of life. Primitive man attaches to his body all the ornaments he can secure, many of which, on the testimony of some savages, function at present as a means of sexual allurement.

In order to make themselves agreeable to the opposite sex, men and women paint and tattoo themselves, submitting themselves to many painful operations in order to fasten rings, sticks, shells, pieces of fat meat and other objects in the nose, lips, ears, and the pudenda, the ornamental value of which is alleged to increase with the number and size of the objects worn. Sometimes the teeth are knocked out and sometimes they are blackened, browned, or sharpened, the purpose being, according to some of the savages, to make the mouth of the possessor as different as possible from that of an animal; and only the possessors of such teeth are considered marriageable. Similarly painting, dressing, and ornamenting the hair in various ways is said to serve as a sexual stimulant, whereas in many races short hair, especially with women, is a symbol of chastity. Self-decoration is in many tribes more prevalent with the men than with the women, a fact which is supposed to parallel the secondary sex characteristics of the male with birds and other animals. The contrast between this situation and civilized life of the present day is explained in the theory by the fact that in savage life males predominate, whereas in civilized life the opposite obtains. The intimate relationship between self-decoration and clothes brings about a mutual substitution of function, such that clothes readily become decorations while mere decorations have also developed into clothes.

The origin of clothing may, perhaps, according to Westermarck, be traced to several different sources, the most obvious of which is the desire to protect the body from frost and damp. The practice of veiling the face in Mohammedan countries he takes to be a form of protection against the evil eye and he presents other evidence for his contention that the covering of the pudenda with peoples otherwise naked is due in some cases to this same motive; in others it may come from the desire to secure protection against the attacks of insects or to prevent injuries to the organs. Still another motive seems to be furnished, according to Havelock Ellis, Lombroso, and Ferrero, in the desire to forestall the arousal of a feeling of disgust at certain secretions, which is, according to Lombroso, the sole form of modesty among prostitutes to-day.

While admitting, however, that a thing may be invented for one purpose and afterwards serve for another and that savages who now look upon clothing as ornament may not perhaps have done so from the very beginning, Westermarck believes nevertheless that, in many cases at least, the pubic covering was originally adopted as a sexual lure. He quotes in support of this the affirmative statements of some savages, and calls attention to certain practices among primitive races that seem to bear out his contention. Among the Saliras, on the authority of Lohman, only the harlots clothe themselves. Furthermore, clothing is not the only way of attracting attention in the way desired, since in many of the Pacific islands the pudenda are tattooed and mutilated by subcision and introcision ostensibly with the same intent. Westermarck diverges from Ellis in his interpretation of the apathy of the male, regarding coyness of the female not as indispensable to tumescence but rather as promoting by delay more complete and abundant secretions of the sexual glands. Furthermore, he does not claim that peoples who attach no idea of shame to the exposure of the entire body are devoid of modesty, quoting on the contrary Kerr Gross to the effect that "as a general rule, in Africa modesty is in reverse proportion to clothing."[4]

In contrast with this interpretation of clothing stands that of Schurtz, Ratzel, and Wundt which tends to confirm the popular view that clothing is originally due to a certain natural sense of shame. The essay of Schurtz, upon which Wundt draws and to which Westermarck gives some consideration, is worked out in opposition to the position taken by Von Schleinitz and Rohrbach. Von Schleinitz, from his study of the natives of New Britain, where the women though practically nude are decent in their behavior, had come to the conclusion that the impulse to clothe the body had originated in the need for protection against the exigencies of climate, and perhaps in the impulse to adorn the body. He believed that modesty had been developed by the custom of wearing clothes through a long period of time. Rohrbach had already noted the fact that the first coverings are for the middle of the body, claiming that this was due, not alone to the feeling of modesty but rather to the desire to adorn the body; hence women are more often thus adorned than men. Peschel had also traced the origin of clothing to aesthetic motives.

In the opinion of Schurtz, the purely inductive method of many investigators leads to no results and must be supplemented by a deductive ethnology based upon the results of introspection. His study is concerned chiefly with the African races, but he also draws on material contributed by ethnologists from other parts of the world. With the majority of investigators he is in agreement that clothing proper, as distinguished from clothing used for ornament, begins as a covering for the pudenda, apparent exceptions to this being found among races representing declines in civilization. Clothing proper is known to every people; and even where reports at first sight seem to indicate that there are races living in a condition of absolute nudity, a closer investigation reveals that a small group among them—usually the married women—have some sort of clothing. Moreover, such races have a latent sense of shame that manifests itself with respect to strangers, even though it may be quiescent within the circle of the tribe itself, just as in civilized life the sense of modesty is stronger in its manifestation toward those outside the family circle than toward members of the same family. With Ratzel he is in fundamental agreement that modesty is natural and universal, and finds that the covering for the pudenda remains as the last vestige of clothing with the very lowest declines of civilization.[5]

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[4] Westermarck, *op. cit.*, I, 419-571. *Sexual shame* is defined by him as "shame caused by idea of the sexual function or anything that is apt to lead to such an idea;" *sexual modesty* is "the fear of or the tendency to avoid anything which would arouse sexual shame."

The view that clothing originates in a desire to protect the body from cold will not hold for races living beneath the tropics and those who wear the articles in question at present do not usually regard them as protection. Moreover, we have drawings in the caves of Perigord which show that man was not forced even by the cold of the Diluvial Period to put on warm clothing. While a report of Von den Steinen seems to show that the garments in question may have served secondarily as a protection against the bushes in roaming through the thickets or against the attacks of insects, the theories which take these as primary functions are refuted by the fact that the articles themselves are quite unsuitable for the purposes alleged. In parts of Africa in the temperate zone the factor of climate may, however, have led to an *increase* in the amount of clothing worn.[6]

The close relationship between clothing and sex is revealed most distinctly through the fact that all important events of sex life go regularly hand in hand with change in dress. In the case of most savage tribes clothing is not assumed until the beginning of puberty; and then a distinction is made in the dress of the sexes, prescribed strictly by custom. With the advance of civilization, however, and the advent of marriage this stage comes to be considered of less importance, the emphasis being then laid on the distinction through dress of the married and single female members of the given group. According to Schurtz, this fact throws a flood of light on the psychological significance of clothing, which in many cases is plainly seen to be a symbol of the married state. With some races the expression, "he gave her a dress" means that a man has married a maiden and not a widow,[7] while on the island of Tahiti the chief ceremony in the marriage rite consists in the groom throwing the birde a piece of cloth. With other races the woman who has borne a child wears an additional apron. The feeling of modesty is a necessary accompaniment of social organization and develops concomitantly with the development of marriage, that is, with exclusive sexual possession, clothing being the outward sign of this process.[8]

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[5] Schurtz, *op. cit.*, 1 ff., 42, 45, 48.

[6] *Op. cit.*, 11 f., 39, 111, 112, 116, 120; cf. also Selenka, *op. cit.*, 58.

[7] The suggestion of something similar is perhaps to be found in the antithetical expression, "donner une cotte verte à une fille," the origin of which is obscure.

[8] *Op. cit.*, 13 ff., 31, 33, 44, 54, 66, 72, 75 ff., 95, 103, 137.

the males in the mating season; and if this situation were continued for more than a brief season the herd would be defenseless before its enemies. The fact that for human beings there is no especial mating season would make the existence of human society likewise impossible, unless man in his development had been able to succeed in uniting the gregariousness of the herbivora with the monogamy of the carnivora. The primary cause for the initial stages of clothing is the jealousy of the male, who imposes his will upon the woman owned by him. The religious rites used to sanctify marriage might easily seem to precede this, but religious commands are regularly customs already sanctioned by the needs of practical life. It is even possible to admit that clothing proper may have been used first of all as adornment or as protection from cold during sleep, and also that the sense of modesty has been subject to a gradual intensification without committing oneself to the thought that it has been created—even in the case of races living under the tropics—by the use of clothing for some other purpose. Modesty in its development extends normally from woman to man, which explains why woman regularly wears more clothing than man in primitive society. When exceptions to this rule have been advanced, it has been shown that in some races man has taken advantage of his superior position to allow the woman only the scantier apparel, so that in some cases leaves and foliage constitute her sole attire. With the development of civilization the use of clothing gradually extends to other parts of the body; and in the case of these the feeling of modesty may well arise as a secondary phenomenon.

Thus clothing may come to be worn behind instead of in front by tribes living in huts into which they must crawl, inhabitants of the desert may come to transfer the feeling of modesty to the exposure of the face which was veiled originally in the desert to prevent thirst. Furthermore, among many degenerate peoples that appear at first sight to be absolutely naked, there are various forms of symbolical clothing, due to poverty, laziness, and negligence. Thus a mere string of leather, cloth, or pearls, about the waist or about the breast may symbolize a real garment previously worn, while the true meaning of the symbol is unknown to the wearer. With such symbolization there goes hand in hand in many races the tattooing of the body with designs that simulate the garments in question. Closely allied with this, we often find oiling and greasing of the body so that it becomes covered with a layer of dirt or ashes that serves secondarily as a protection against the weather and insects. Even with this poorest of garments there is a differentiation for the various classes in the tribe. Painting the body also serves to symbolize the painted clothing common among certain races, and is sometimes resorted to for medicinal effects.[9]

Under the impulse to adorn the body, more and more clothing comes to be worn and thus develops into money, or becomes a sign of the wealth of the wearer. In connection with this arise many customs later not understood, such as the removal of clothing in the presence of a superior, the meaning being that the inferior (slave or subject) must not in any way conceal himself or his possessions from his master; the master or potentate, on the other hand, retains all his clothing, or is concealed behind a screen, or the subject is obliged to prostrate himself with his face to the ground and must not look directly at his superior.

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[9] *Op. cit.*, 20-83, 110, 121.

Out of this grew the practice of secluding the monarch, as in the case of puppet kings. As a development of the practice of removing all the clothing, there comes later a merely symbolical removal of it in the taking off of certain articles, such as the shoes or the head-dress; from the latter has come our present custom of removing the hat in greeting a person (found also among those negroes in Africa who wear head-dress) and even, as Schurtz believes, the decolleté dress which was first worn on solemn occasions at court.[10]

The view of Peschel that the veiling of the pudenda occurs from aesthetic motives is rejected by Schurtz because *naturalia non sunt turpia,* and he attempts to reënforce this by showing that while it is difficult for Northern races to assume a purely aesthetic point of view with respect to sculpture, the Greeks would not tolerate clothing in their statuary *except* as ornament.[11]

Wundt, relying on the work of Schurtz and Waitz and tacitly assuming with most investigators that human life originated in some tropic region, finds that clothing is unnecessary for protection in a warm climate; he believes that coverings for the pudenda are to be explained by the "specifically human feeling of modesty." Tattooing and mutilations have originally a religious significance, but later came to be transferred to clothing worn because of climate. One of the chief differences between the savage and the civilized man to-day is to be found in this distinction between direct and indirect ornamentation of the body; but there are transitional stages from savagery to civilization where both forms of ornamentation exist side by side. Even among civilized races traces of the direct ornamentation are found in the artistic adornment of head and beard with the Assyrians, in the painting of the hands and face with the Egyptians, the tattooing of the hands and face with the Arabs, the braids once worn by European men, and finally in the earrings, finger-rings, and hairdress of the modern woman. The first step toward the abolishment of barbaric and Oriental ornamentation was taken by the Greeks, but Oriental influence made itself felt again in the picturesque dress of the mediaeval period to be succeeded by a return to Grecian simplicity during the Renaissance. This was followed by the wigs and braids of the Barocco and more recently, in the modern period, by practical utilitarian clothing, which has little or no regard for aesthetic values.

Two transitions in this development according to Wundt are of ethical import: first the passing of ornamentation from man to woman, which symbolizes her elevation from a condition of slavery (where she needed only the simplest possible working costume) to a position of comparative equality with man; and, secondly, the transference of ornamentation from the body to the immediate household environment. The latter symbolizes the comparative equalization of classes, so far as bodily appearance is concerned, except in the case of various kinds of officials. The evolution of cloths mirrors the evolution of morality, and the aesthetic and ethical interpenetrate each other, revealing at one and the same time the aesthetic taste and the ethical character of a given people.[12]

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[10] *Op. cit.*, 107-142. [11] *Op. cit.*, 12 f., 142.

Ernst Grosse comes to the support of Westermarck from the point of view which affirms that ornament antedates clothing. He emphasizes the prevalence of red as a secondary sex characteristic and as a color preferred by primitive races, claiming in opposition to Gerland's religious interpretation of tattooing and scarification that these have an identical aethetic function, tattooing being confined to the yellow races and scarification to the black. Following Lippert he finds that the location of bodily ornament is structually determined by parts of the body like the neck and especially the hips which offer natural support to collars and girdles for tools and weapons. The fact that the covering for the pudenda is not universal and not adequate, together with the further fact that our children have no natural shame, seems to him proof that the theory commonly accepted by anthropologists is false.[13]

The significance of clothing in its strictly ornamental function was first emphasized, so far as I am aware, by Lotze in the *Mikrokosmus*, where he develops a line of thought, worked out later by Friedrich Theodor Vischer and Lipps in the doctrine of *Einfühlung*, and especially by Lipps and Selenka with respect to the significance of bodily adornment. After discussing the intimate manner in which we feel ourselves to be present immediately in the various tools and instruments used by us, so that we seem immediately to touch the paper where the pen is in contact with it, to cut the meat where the knife passes through it, etc., Lotze takes up for consideration the many kinds of bodily ornamentation both from the point of view of the effect on the wearer and on the observer. Except perhaps in the case of colors, he observes, the satisfaction in ornament does not reside in mere vanity aroused by the admiration of others. It consists rather in a special enhancement and refinement of self-feeling experienced by the wearer. Head-dress and foot-wear give us a satisfying sense of increased extension in the vertical. With the movements of the head to and fro we experience an extension of our personality into the very top of the high hat. Similarly with the cane which we carry or swing and with which we touch various objects. Tassels, ribbons, and other fluttering forms of bodily adornment give a delightful experience of being in rapid succession at many points of space, making doubtless for a certain animation and vivacity in the wearer. Children sometimes tie onto themselves a tail that they drag along on the ground or wave in the air behind them for the sake of this enhancement of self-feeling. The greater or less tension of our clothing is felt as though it were a quality of our very selves. The wearer of a corset seems to be the possessor of the strength and elasticity of the garment, just as the wearer of armor assumes the qualities of the metal. Veils and light delicate fabrics worn by women produce in them corresponding experiences, that are reflected in their behavior, and all of these are paralleled by equivalent modifications in the consciousness of the observer.[14] Among other things Lotze suggests that it would be possible to infer much concerning the psychic qualities of various races by a detailed study of their dress; and this Selenka has done under the immediate influence of his colleague Lipps. He does not, however, make any mention of Lotze's discussion, although in one or two places he uses almost identical language and illustrations.

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[12] W. Wundt, *Ethik*, 1912, 152-161; *Völkerpsychologie*. Bd. 3, 1908, 176-189, 219, 225.

[13] Ernst Grosse, *Die Anfänge der Kunst*, 1894, 52-110.

Ornamentation is for Selenka a kind of *Bildersprache* adopted unconsciously by all the peoples of the earth and differing from other sorts of language through the fact of its universality; the identity of meaning in clothing (which is paralleled in its universality only by the expressive mimicry of the human face) he explains as conditioned structurally by the identity of the human form adorned. The language of ornament is only one aspect of a more general language of clothing, a *Bekleidungssprache,* whose elements, with the single exception of hair, have all been borrowed from the world external to the human body. In a long trailing costume a person can not run or move about quickly and hence this sort of clothing means dignity of gait and bearing for both wearer and observer. The crest of a helmet pointing forward symbolizes the charging warrior, etc. Not all symbolical articles of clothing are, however, ornamentation; a tall stiff collar hinders the natural movements of the neck and is a symbol of stiffness and reserve, not an ornament. The language of ornament, in the strict use of the term, expresses only favorable qualities of the one adorned, and originates in a desire to be pleasing; but this impulse and that of vanity must not be regarded as the only motives behind ornamentation. Ornamentation—here he is in agreement with Lotze—exercises at the same time a developing effect upon the wearer, enhancing his self-feeling, influencing his moods, and modifying his behavior.

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[14] Hermann Lotze, *Mikrokosmus*, 1869, 199-212.

If it were not for this moral value of adornment, it could never have been developed into the significant language it has become. While other forms of communication are inventions that vary from region to region, ornament is a discovery, characterized by changeless law. The very word 'kosmos' (the foundation of the English word 'cosmetic') means order and law and was consciously used in a double sense by the Greeks, who meant by ornament only lawful ornament as distinguished from decoration or mere finery. True aesthetic ornamentation is always structurally motivated, and this accounts for the universal agreement of the various forms. When the Roman senator chose the long flowing toga, the naked African her rings, the more agile Spanish woman her fluttering ribbons and laces, the Indian the pinions of the eagle, and the Persian the broad tightly-fitting girdle, choice in all these cases was not a matter of chance but was conditioned by an inner necessity.

The clew to an interpretation of all natural ornamentation is to be found in an attentive examination of the human figure, the upright position of which almost exclusively conditions the form of its ornamentation. In contrast with that of other mammals, the body of man is characterized by two directions, the line of the vertical and (in its forward motion) that of the horizontal; these furnish the conditions for the two classes of pendant-ornament and direction-ornament. To these must be added a third kind of ring-ornament, due to the shape of various parts of the body, a fourth kind of augmentative-ornament (epaulettes and wide shoulder collars), a fifth kind of color ornamentation (produced by gems and stones), and a sixth kind of clothing ornamentation in the proper sense. Besides these there are transitional forms. A closely fitting necklace is a ring-ornament, but a necklace large enough to hang down loosely on the breast becomes also pendant-ornament, enhancing at one and the same time the perception of the erect position and the sense of movement and life in the wearer and for the observer, in so far as movements of breathing, etc., become thereby accentuated. Ribbons at rest are pendant-ornament, but with the body in motion they become direction-ornament. A broad shoulder collar provided with fringes is at one and the same time augmentative, pendant, and, with the wearer in motion, direction-ornament. As for Lotze, the ground for satisfaction is found by Selenka in the fact that the wearer identifies himself with his clothes; but he emphasizes also the social aspect of the matter in his statement that there is no tendency to ornament parts of the body covered by clothing and that it would never occur to a person thrown on a desert island to adorn his person any more than it would occur to him normally to talk to himself.[15]

In agreement with Gottfried Semper, Selenka distinguishes two types of ring-ornament: stiff rings for those parts of the body (arms, head, legs) that change their shape little or not at all during the movement of the wearer; and, secondly, linked or chain rings, worn on ankles, wrists, and neck, which express or symbolize the mobility of these parts. Stiff rings serve to symbolize the expansive life of the muscular parts of the body, and, in the case of head-ornamentation (fillets, head-bands, wreaths, and crowns) to emphasize the form of the head and the color of the hair. Some forms of head-dress, like the broad-brimmed hat, belong to the general class of augmentative-ornament. The aesthetic value of the close-fitting girdle is to give to the wearer a sense of greater strength, while emphasizing the mobility of the upper half of the body in contrast with the rigidity of the hips, but the girdle gets its fuller meaning only with the completely clothed body. Finger-rings have aesthetic significance as local color ornament through the stones in them, while the toe-rings, worn by some peoples, have merely practical utility in protecting the feet.[16]

Pendant-ornament should always be, not rigid but mobile, so that it may adjust itself readily to the true vertical. Thus ear-rings, symbolizing the direction of gravity, must be mobile in order to represent continually the direction of gravity in contrast with the delicately curved lines of throat or neck which manifest their independence of the strict vertical. The aesthetic value of heavy pendant-ornament is enhanced through the fact that it influences the behavior of the wearer by preventing him from making hasty movements, thus putting on him the stamp of dignity. The effect of symmetrical pendant-ornament is relative immobility and in the case of Malay brides it symbolizes an absolute subordination to the will of the husband. One illustration is given of such a bride so loaded down with ornament that she was forced to remain practically motionless for the fourteen days of the wedding ceremonies. The Japanese kimono, which allows the wearer merely to toddle about, possesses a similar meaning. In the case of the moving body, for example in the dance, such ornament serves to emphasize the various positions assumed, and shawls, ribbons, and fluffy fabrics in general give the danseuse opportunity for manifold pendant arrangements. A danseuse in a tightly fitting jersey would be at a great disadvantage and, in fact, ridiculous. Necklaces and girdles when worn loosely as pendant-ornaments give a sense of slenderness to the form of the wearer.[17]

In addition to epaulettes, ladies' trains, head-feathers of the Indian and other forms of augmentative-ornament (all of which may lead to extravagant manifestations), the full beard constitutes a natural kind of augmentative-ornament, lending dignity in most cases to the wearer. When this has been reduced through shaving to a moustache or an imperial we have either a symbolical presentation of the full beard or a form of mimicry. In his discussion of the ornamental function of clothing, in which he follows Lipps very closely, Selenka emphasizes the fact that clothing is ornamental only when it falls into the categories of ornament already pointed out. Not all clothing is ornamental, although it may all be expressive or symbolical. Thus the stiff, high collar of the soldier's uniform means or expresses the rigidity of military discipline, while a large, soft collar may be a true ornament. Various materials, such as silk, plush, lace, gold-brocade have also their specific aesthetic significance. Sometimes ornamentation has merely a physical, sometimes also a spiritual significance, as when the crown draws attention to the head, while signifying mental and social power. The flower in the hair tells, similarly, of the taste and disposition of the wearer, while calling attention to the color and arrangement of the hair itself. Climate, manner of living, environment, racial character, the social position of women, tradition, intercourse, and fashion are all mirrored in the style of ornamentation.[18]

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[15] Selenka, *op. cit.*, 1-14. [16] *Op. cit.*, 14-30. [17] *Op. cit.*, 30-44.

In glancing over the material accumulated in the general field of clothing and ornamentation one is impressed by the fact that the data unearthed are not as unambiguous as might be wished. Moreover, some of the phenomena are in apparent contradiction with other phenomena, and there are contradictions among the investigators with respect to fact that seem to the layman susceptible of elimination. Thus Westermarck, Hirn, and Grosse maintain that there are races living to-day in a state of nudity, while Schurtz and others declare that more careful investigation reveals in all such cases members of the group who are clothed at least symbolically. The assertion that this is true at least for the married women is opposed by the claim that there are Australians regularly naked with the exception of the unmarried women. The emphatic statement of Ratzel, Schurtz, and Wundt that no races are entirely lacking in modesty (also admitted by Westermarck) is met by the contention that there are naked races with no natural shame. The claim that woman is less modest than man, alleged to be shown by the existence of races where the women wear less clothing than the men, is opposed by the explanation that in such tribes (because the position of woman is especially inferior and because clothing has there degenerated into ornament) the women are not allowed to have what little clothing is available; even in such cases it is claimed that the women are more modest than the men and that they resort at times to a clothing of foliage. Scarification is alleged to be for the practical purposes of recording the number of enemies slain in battle, for allowing evil spirits to escape from the body, and for adornment. Similarly, tattooing is explained as due to religious and totemic motives, to use as an amulet and to ornamentation.

The view of Westermarck seems difficult to accept but almost as difficult to reject entirely. Among its difficulties is the fact that it invites us to believe that modesty or decency as now understood could have developed out of its very opposite; although the doctrine is put forward with the important reservations above noted. In common with most of the theories, too, it assumes that the primitive races now extant represent adequately the ancestors of present civilized peoples, whereas it is possible that most of these so-called 'contemporary ancestors' are merely degenerate or the relics of psychically defective stocks, whose failure to progress might be due to an inability to secure such control of their sex life as may have been possessed by the ancestors of the present civilized stocks, granted that there is some relationship between continence and the development of civilization. Many accounts seem to show that, although savages do not regard incontinence as an evil, savage life in general is not characterized by incontinence; but this would seem to be a matter especially difficult to determine for any stage of civilization.

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[18] *Op. cit.*, 44-72

So far as the alleged apathy of the male is concerned, nobody familiar with the behavior of domestic animals will maintain that as a rule it is in their case a matter of fact, and Havelock Ellis, recognizing this, attempts to explain the supposed discrepancy as due to the over-nourishment of the domestic animals. The fact, not mentioned by Ellis, that domestic animals, like the dog, breed oftener and have larger litters than the wild animals with which they are most closely related and from which they are supposed to be descended, tends to make this claim plausible; but in general it is extremely doubtful whether we know enough about the sexual behavior of wild animals to draw the conclusion arrived at by him and others with respect to apathy.[19] So far as the coquetry of domestic animals is concerned it is confined in my own observations to the female and is there rather a form of timidity or fear; mating seems to be quite regularly a conquest by the aggressive males, and such males seem to be the only ones with which some females can be mated. The argument of Grosse with respect to the exciting effect of red for savages, which he connects with the prevalence of red as a secondary sex characteristic, is the popular view; but it has been considerably weakened, so far as animals are concerned, by the experiments of Yerkes on the dancing mouse[20] and by Stratton on cattle;[21] recently, too, there has been furnished a plausible explanation of the secondary characters in the case of birds that brings them into line with the phenomena of protective coloring.[22] The prevalence of red in the painting of the body may be due to the wide geographical distribution of red ochre—the material used—as well as to the further fact that red, like white which is also widely used, stands out very distinctly in contrast with the dark skin of the savage. In the case of savages, it seems quite improbable that the alleged sexual apathy should have disappeared or have been transformed quickly into its very opposite with certain savages that have been only recently affected by civilization. Moreover, the alleged coquetry of savages observed in both sexes seems to be in contradiction with any alleged apathy of the male alone; and with both sexes apathetic the coquetry would seem to lack motivation.

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[19] A few animals, birds and squirrels, taken very young and raised by me in captivity have behaved like the domestic animals.

[20] R. M. Yerkes, *The Dancing Mouse*, 1907, 133-177.

[21] G. M. Stratton, The color red, and the anger of cattle, *Psychol. Rev.*, 30, 1923, 321-325; cf. also E. Kittredge, Some experiments on the brightness value of red for the light adapted eye of the calf, *J. Comp. Psychol.*, 3, 1923, 141 ff.

[22] F. M. Chapman, *Handbook of Birds of Eastern North America*, Rev. ed., 1916, 88-97; H. W. Bates, *The Naturalist on the River Amazons*, 1892, 13, 139; G. H. Thayer, *Concealing-Coloration in the Animal Kingdom*, 1909, 24 ff.

There is also some discrepancy in the statements of Westermarck that, with women in a condition of comparative slavery and in the minority, both sexes indulge in coquetry. Such behavior would seem to belong more especially to a stage of development in which men are scarce and woman's position more independent. The suggestion that even under the conditions of savage life woman may choose her lover, if not her husband, seems to be based on the supposition that personal beauty other than that founded on physical strength is the foundation for the alleged choice, whereas it is fair to assume that in primitive life physical strength and dexterity, quite apart from all adornment that is not the symbol of such strength, is the primary motivation. The elimination of the unfit males would in that case be quite similar to the elimination of the unfit males among the lower animals. The further suggestion that under the given conditions of inferiority savage men are not indifferent to the feelings of their mates implies a degree of refinement and susceptibility probably far above the level of the savage. Moreover, the use of clothing by harlots and in the so-called erotic dances might be for the very purpose of preventing sexual excitement, aroused in the latter case by the intimacy of the dance. It is difficult to imagine savages sexually so apathetic that they are obliged to engage in special dances to arouse or develop a desire which seems, indeed, to be the tacit presupposition of the dances in question, although it is a fact that the savage's lack of initiative in certain other undertakings is compensated for by dances which incite him to activity.[23]

With nudity the norm, it is plain that the novelty of clothing would serve to attract the attention; but would it imply anything more than this, so that by means of it a sexually unattractive savage might become more *attractive*? Even granting that it might, it would seem on the same basis that, with the general adoption of clothing, a return to absolute nudity would soon be indicated, unless this were already objectionable on other grounds. After clothing had once been in general use, it is easy to see how coquetry might come to be secondarily effective through the function of some clothing as ornamentation.

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[23] Cf. Hirn, *op. cit.*, 247 ff.

In any case the theory interprets ornamentation in a manner that ignores the function of true ornamentation as developed by Lotze, Lipps, and Selenka. Even though we might come to derive the first use of clothing from the impulse to beautify or adorn the body, it does not seem necessary to think of such adornment exclusively or primarily in terms of sex stimulant, unless we are committed to something like a Freudian platform. An interpretation of ornamental clothing in aesthetic terms would be quite as possible for the savage as for the civilized children who decorate themselves spontaneously in the various ways described by Lotze. In the case of the male savage, even neglecting the subjective function of ornament emphasized by Lotze, self-decoration might be quite as well for the approval of his equal and superior male companions as for the inferior females; although it might prove to be also, secondarily, a sex stimulant for these. Probably, however, more practical motives are primarily operative in the selection of mates in savage life.

It must not, however, be overlooked that a great deal of the scarification, tattooing, and other decoration can be interpreted as being 'ornamental,' only by doing such violence to the meaning of the term as occurs when Grosse suggests that while these things are not ornamental for us they may be so for the savage; and something similar might perhaps be said even with respect to the interpretation as symbolical clothing of certain articles worn. In many cases the only term that seems to be applicable with strict justice to the facts as they lie before us is the rather general designation of differentiation or 'distinction.' Scarification, tattooing, mutilations, and all articles worn serve at least to *distinguish* the wearer or possessor and thus to enhance his self-feeling in contrast with beings not possessing such marks, whether or not they function specifically as clothing or 'ornament,' whether or not they have magical or religious significance, and whether or not they are signs of especial prowess. The lack of consistency in their alleged specific uses might thus in a way be accounted for. Probably the psychic life of the savage is far more complex or less explicitly differentiated than we in our zeal for simplification are wont to regard it, so that in the assumption of clothing many motives were doubtless in concomitant operation.

One such motive to the initial assumption of clothing is suggested by the fact already mentioned that savages knock out, file, blacken, and brown their teeth, on the testimony of many investigators, *in order to distinguish themselves as sharply as possible from certain lower animals*; they even make fun of the teeth of Europeans, saying that the mouths of the latter remind them of dogs, horses, etc., and for this reason they find them sexually unattractive. Is it not possible, then, that they also paint and dress the hair primarily because of a similar impulse to distinguish themselves from certain lower animals, which they despise or regard in any case as their inferiors, rather than from any specific aesthetic impulse to adorn themselves? They would alter the natural condition of the hair, not for the purpose of any equivocal 'ornamentation,' but merely for the sake of distinguishing or differentiating themselves from their inferiors. Similarly with respect to the pubic and axillary hair. The fact that the pudenda are frequently tattooed in a manner that preludes or simulates clothing, would suggest that here, too, as in the case of other parts of the body, painting had preceded tattooing, which would in the sequel perhaps lead to clothing proper. Moreover, all these distinctions, once they had been established, might come to function in various other ways, just as the coloring and deformation of the teeth (possibly also of the head, feet, and other parts of the body) come, with the races practicing such customs, to be indispensable sex stimulants, since they become social norms for the groups involved.

In a paper read before the Southern Society of Philosophy and Psychology in 1917 on "The Significance of Beauty," Professor Dunlap referred to an aversion to the exposure of the axillary hair by women to-day and might also have called attention to the allied practices of shaving, the tonsure of the clergy in the middle ages and at the time of Leonardo da Vinci, to the fashion of removing the eye-brows, which seems to have returned of its own accord only recently. Dunlap suggests that the aversion to exposure of the axillary hair (which would be explained today, I suppose, on the ground of its suggestions of animality) may be a survival of the practice among Greek women of removing the pubic hair that has set an aesthetic standard. His belief that this practice of the Greeks was due to a desire to be rid of pediculi may indeed indicate a partial motivation for the custom, which would, however, instead of beginning there, be operative all the way from savagery to civilization; but why may it not be regarded quite as well as the survival of an older aversion to hair of the body seen among many savages, so that the explanation given for the aversion to-day would be the natural and universal one throughout the ages? The basis for it might well be found in a general tendency to disparage or vilify whatever the individual or the group does not itself possess. In many cases savages seem to manifest such an aversion by plucking out the hairs of the beard and of the eye-brows,[24] and they sometimes, as we have seen, regard short hair as a symbol of chastity. In other cases they alter the natural condition of the hair of the head by elaborate manipulation. Such an inferred primitive aversion to hair is indeed presupposed by Darwin in his theory that hairlessness in the human race is to be accounted for by sexual selection.[25]

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[24] Heinrich Wölfflin, *Die klassische Kunst*, 31, explains the absence of eyebrows in the Mona Lisa (which was restored in copies of a subsequent period) by a reference to Cortigiano, who states that the removal of the eye-brows was a prevailing fashion with the women of the period which disappeared soon after. In a legend of the Cray Fish clan of the Choctaws mention is made of the fact that the Cray Fish had the hair plucked from their bodies before they could become men, cf. J. G. Frazer, Totemism, *Encyclopaedia Brittanica*, 9th ed., 23, 1911, 467; also James Mooney, *Myths of the Cherokee*, 325 (cited by Wundt, *Völkerpsychologie*, Bd. 2, 153.).

[25] Westermarck, *op. cit.*, 2, 20 f.

Whether in lieu of any evidence for the existence of a hairy race of men we accept his account of the matter, or whether, on the assumption that human life began in the tropics, we may infer that the race has developed under the influence of cooler climates more hair than it had originally, in any case it seems clear that man in a condition of comparative hairlessness might well desire to remove from observation or alter in various ways any such characteristic as hair which connects him with beings necessarily regarded by him as his inferiors in many important respects; and this impulse would naturally be stronger in proportion as the group to which the individual belonged was lacking in hair.[26]

Something like this seems easier to believe than the thought that primitive man is naturally ashamed of his naked body, his genitals, or even the sex phenomena to which St. Augustine refers.[27] Indeed we are informed on the contrary that savages are often very proud of their pudenda; so that in this sense *naturalia non sunt turpia.* On the other hand, the principle is manifestly restricted by the savage's treatment of his teeth and by other mutilations and alterations of his *naturalia.* It would seem that, not in themselves, but only when all these things suggest inferiority or identification in some way with a being of lower status does primitive man perhaps regard them differently. For primitive man in his rudest stage of existence (in which "though destitute of almost everything which we regard as the necessities of life" he still possesses 'ornaments') the only inferior being is the lower animal; so that the same impulse that leads to rivalry with his equals, to the attempt to exalt himself at the expense of his inferiors, and furnishes the general psychic foundation for classes in savage and civilized life might lead primitive man to attempts to distinguish himself from lower animals in certain ways which would doubtless be especially pronounced in the case of animals like the anthropoids that resemble him most closely. The sense of shame—the general negative aspect of the impulse in question—might be largely reducible to a fear of limitation or relative annihilation of the feeling of self through degradation to the status of a being whom the individual for various reasons regards as beneath him.[28] Shame for the naked body, together with the mutilation and scorn of the flesh which has played such an important rôle in civilization, might, then, in a measure go back to some such primitive situation. Sexual modesty proper might be due in part to the aversion to being reduced to the status of the animal through the natural appearance of the pudenda, in part to the fear of being reduced to the status of the rejected lover;[29] and clothing might then come to be used partly for the purpose of concealing premature expressions of sexual emotion, which in themselves, apart from the associations indicated, could hardly be the cause for shame St. Augustine supposed them to be. The privacy and secrecy of sex relations with some savages—although there are others who are said to copulate as publicly and unconcernedly as the lower animals—may be explained perhaps partly by this impulse, partly from certain aesthetic motives.[30]

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[26] The artistic treatment of the beard by the Assyrians, to which Wundt refers, might depend similarly on their being a comparatively hairy race.

[27] De civitate Dei, XIV, 17 (Migne, Patrologiae cersus, XLI, 425 ff.); cf. also Westermarck, *op. cit.*, I, 539 f.

[28] In this connection it is suggestive that Latin *pavor* and *pudor* are both derived from he same root *pu*; the original meaning of which is "beat, cut, or strike." Cf. Alois Vanicek, *Griechisch-lateinisches etymologisches Wörterbuch*, 1877, 539.

[29] Anthropologists relate that the shame at being rejected is among some savages so great that it results in suicide.

[30] Such are suggested by Weininger, *Geschlecht und Charakter*, 1905, chap., "Erotik und Aesthetik," 341 ff.

The chief objection to such suggestions is found, perhaps, in the fact that some savages tattoo themselves with animal forms, arrange their hair to represent the horns of animals like the deer, and otherwise endeavor to assimilate themselves to their own totemic animals. To what extent this is a kind of play or dramatization is perhaps debatable. The fact, however, that savages may admire or actually treat as equals certain animals, notably birds, beasts of prey, and the animals upon which they depend for their sustenance, does not preclude the inference that they doubtless despise very many others; and the circumstance that those who have totems worship these, while, on occasion, they also kill and eat them, involves a contradiction for which there is no satisfactory explanation in the accounts given either by the savages themselves or by their interpreters The natural inference would seem to be that while primitive man might admire or reverence a given animal for its courage, cunning and certain other qualities, he would nevertheless consider a being that could be circumvented and killed by him as essentially inferior, even in the case of his own totem. Certainly, in the later periods of savage mythology, the transformation into an animal form by means of witchcraft is regarded as a punishment and degradation. In any case totemism and the mythology of some savages is probably a late development and it is by no means a universal phenomenon of primitive culture; like all religious and social systems, totemism doubtless stands for a rationalization of customs that have long been in operation for other causes. Marks of distinction, such as scarification, painting, tattooing, wearing of trophies, badges, and the like, may well begin with no ornamental value whatever, and then, under the aesthetic impulse and with developed technique, develop later into true ornamentation that should acquire religious significance through its antiquity, and moral value through its social effects. Finally, while in the discussion of Lotze explicit provision is made for the enhancement of self-feeling only through form, the suggestion in question would provide also for the subjective and objective effects of color as well.

Whatever view of clothing we may feel inclined to accept for the savage, it is plain that the significance of clothing and adornment to-day, in cold climates and with Northern races, is different from what it is in Africa. Clothing for the modern man aims to be practical and for the male has largely ceased to be aesthetic. The body of the industrial male is encased in mathematically straight lines and *pour surcroît de malheur* the trousers are creased. Instead of revealing, as did the Greek costumes, what has been called the most beautiful object in all nature, clothing may function to conceal defective bodies produced by the emphasis on mental and specialized physical efficiency to the neglect of physical beauty and perhaps of physical well-being.

The clothing of modern woman has usually been more adorned than that of the modern male, if not always much more aesthetic, both with respect to color and to form; but hand in hand with the development of feminism and the industrial rivalry of the sexes, there has been a change in the direction of a smarter and more business-like garb. This was doubtless accelerated greatly by the late war, partly as a result of the general practical military atmosphere, partly, I suppose, directly as a result of costumes worn by women in camps.

I doubt, however, whether anybody who regards the matter without prejudice will claim that the newer styles represent a descent from a higher plane of morality, or will attempt to make them responsible for any alleged decline in sex morality, any more than he will credit the prophecy that bobbed hair will result in bald-headed women with beards. Without wishing to be identified with any of the European cults mentioned at the beginning of this paper it seems to me personally that the new styles represent in many cases an advance aesthetically that should make for a sounder morality and possibly in some small way for racial improvement. It is of course possible that recent dress may have been responsible for some laxity on the part of the generation used to other styles; but it is hard to believe that any such condition alleged to be present in the younger generation can be ascribed to this. Within limits habit and custom would be the determining factors in matters of this kind.

Granted the fact of laxity, it can be more readily explained as a phenomenon due to the emotional orgies and the informal hurried marriages and matings of the recent war-period, as well as to the scarcity of men following upon war. This last phenomenon has been frequently observed in the past, the period subsequent to the Thirty Years' War having been especially notorious in this respect. There are also many analogous instances in the history of savage tribes, whenever through war, pestilence or other causes the number of men has been so reduced that women are greatly in the majority. Such a condition, if present, will hardly be corrected by the promulgation of edicts concerning the styles.

The fact of woman's recent emancipation, whether for good or ill, is clearly symbolized in her present attire, which permits her greater freedom of bodily movement than ever before; and when the high heels shall have followed the train, hoop-skirt, long skirt, and corset nothing of real value will have been lost. This is also true with respect to the primitive custom of painting the face. On the other hand, it is certainly desirable that the impulse of woman to adorn her body in legitimate ways shall persist. If we agree with Schiller and Wundt with respect to the intimate relationship between aesthetics and ethics, and especially with the view of Schiller and Selenka that such ornamentation of the body is a case of legitimate *moralischer Schein,* we may well hope that this tendency shall be kept alive and that eventually some reflex influence of it may become manifest in a more aesthetic garb for man.

THE PSYCHOLOGICAL APPROACH TO ESTHETICS

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The desire to account for the facts of esthetics in terms of a prevailing psychology has been manifest throughout the recent history of the subject. Such accounts have sometimes lapsed into obscurity with the decline of the general view in question, and have sometimes made definite additions to the stock of ideas on esthetic matters. Today, however, if the dictum be true that there is no longer psychology, but only psychologies, this procedure is less applicable than when a more uniform attitude could be adopted. We must first catch our psychology before we can apply it; we must select from a large body of claimants to the title, and be exposed to criticism from the holders of the positions we reject. It therefore seems a fair question whether something may not be gained by reversing the process—that is, by seeing what sort of psychological position is suggested by, or would follow from, the more or less admitted facts of esthetics.[1] The present paper will make that attempt by first summarizing some previous contributions that seem adapted to the purpose, and then outlining a method by which their results may, with some additions, be applied to the treatment of esthetic problems.

I

It seems no rash assumption to hold that the main problems we have to deal with fall within the general field of problems of value; we must therefore first ask what occurs in a consciousness which takes account of a value. It seems to me clear that perception of value depends primarily on a certain detachment from the current of experience on the part of the percipient. Anything which is regarded simply as a signal or a cue for action can scarcely be considered as in itself valuable, though of course it may have value as an element of the total situation. The point is that it can not then and there be felt as valuable unless the percipient can in a sense 'stand off' far enough to see it as an element qualitatively distinct. Obviously, there are many occasions in ordinary life when we have neither time nor

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[1] For my general position on these matters see, The scale of esthetic values, *J. Philos*. 21, 1924, 617-631. The three chief determinants of esthetic value are there defined as interest in sensuously agreeable material, interest in the complete of its kind, and interest in a 'good job' at meeting the requirements of a situation (621).

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attention to devote to such aspects, and when, consequently, values are largely or entirely unfelt. But a consciousness of a purely automatic kind would hardly be what we mean by a normal human consciousness; and consequently we also find situations in which values of one sort or another definitely emerge.

The resulting constitution of consciousness has been most clearly defined, by MacDougall,[2] as a system of habits and a system of ideas, which are progressively organized and elaborated. "Each of these factors," he says, "has a specific function and a continuous history in mental development. The system of habits represents the organism's response to the permanent aspect of the environment; the system of ideas represents its response to the variational aspect. Wherever the system of stimulations presents recurrence, the organism responds with a habit; wherever that system presents novelty, the organism responds with an idea."[3] Personal experience is thus more or less stabilized between these two levels. "Without habit experience would be an irrational choas; without ideas it would have no existence—for experience is not posited of any organism in virtue of adaptation in general, but only in so far as adaptation constitutes an ideal process determined by a specific aim and a sense of value."[4] Its simplest form is "attention to the stimulus. Instead of provoking an habitual response, it arouses an awareness of its own quality and of the relations in which it stands."[5]

Whether or not this account holds for all perception of value—and for my own part I believe that it does—its fitness for the discussion of esthetic value is unmistakable. A purely routine, habitual type of mind would surely be unconscious of such values; and we find in actual experience minds so nearly of that type as to be practically insensible of them. At the same time, habit has its place both as affording a basis or background with which the value-perception may be contrasted, and as furnishing, by its easy and natural functioning, a pleasurable element of considerable importance in the total esthetic situation. But this is obviously not a complete account. The interaction of the two levels, habit and idea, creates in the percipient a more or less definite state of tension. It is possible both to conceive, and to discover in actual experience, many degrees of this tension, from a degree so slight as to be barely perceptible above the level of habit to one so intense as to border on self-forgetfulness or mystic rapture. Moreover, the individual percipient may exhibit many degrees of this tension in his own experience, according to his state of adjustment in any particular set of circumstances, and according to the nature of the particular situation offered to him for esthetic response.

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[2] Robert MacDougall, The system of habits and the system of ideas, *Psychol. Rev.*, 18, 1911, 324-335.

[3] *Op. cit.*, 327. [4] *Ibid.*, 335. [5] *Ibid.*, 326.

This notion of tension has played a somewhat indefinite part in psychology for a considerable time. Some twenty years ago Bawden discussed it in relation to the 'functional' psychology, and defined it as the emergence of consciousness in a disturbed state of equilibrium. "The organism, with the brain as its neural center of gravity, is a machine made up of a delicate system of balances. When this equilibrium is disturbed beyond a certain point, which varies according to the inheritance and previous history of the particular organism, consciousness emerges. Consciousness represents what, comparatively, we may call the tensional equilibrium of the organism, whereas habit represents its relatively stable equilibrium. This conception is divested of all objections from the metaphysical side, if it be constantly borne in mind that consciousness is no more an entity than habit, and that, like habit, it simply represents the life of the organism under a given set of conditions."[6] Again, "What are the conditions of consciousness? What are the laws which determine when an act becomes conscious or ceases to be conscious? These are: the law of tension or obstruction in activity, and the law of habit or facilitation in coördination. By the law of tension is meant simply this, that consciousness appears only when the process of action is relatively impeded or interrupted. Action is going on all the time, in tropism, reflex, and instinct. But these become consciously performed acts when there arises stress in adjustment, whether the forces of the tension be intra-organic or extra-organic."[7] Another writer who has used the notion is Janet, who has recently summarized its advantages as a means of bringing together a wide range of psychological phenomena.[8]

The fact that neither of the writers just mentioned gives us a very clear notion of the mechanism (if any) underlying this tension is no argument against its use as a descriptive concept. As Dumas himself remarks at the end of a chapter on the expression of emotions: "Est-il nécessaire, en terminant ce long chapitre, de constater que la plupart des problèmes qui y ont été posés ne sont pas, en l'état actuel de nos connaissances, susceptibles d'être résolus complètement, ou même d'être résolus du tout? Qu'il s'agisse des conditions psychologiques ou biologiques de la mimique, du mécanisme psychologique ou cérébral des émotions, de leurs centres, de leur nature, nous avons toujours abouti à des solutions incomplètes."[9] We are therefore not debarred from the use of the concept in discussing the esthetic experience, if we feel that we cannot conveniently do without it; and of the existence of tension as an empirical fact in the esthetic experience I entertain no doubt whatever. Moreover, it is a means of bringing together, and making coherent, various other views concerning esthetic data. For instance, it readily accommodates Edward Bullough's important doctrine of 'psychical distancing;' it enables us to accept empathy or *Einfühlung* as a useful principle without trying to extend it, as its more enthusiastic adherents incline to do, to every part of the esthetic field. Since, as we shall see in a moment, it adapts itself to a most important aspect of that field, no further justification is needed for regarding it as fundamental to any psychological account of the esthetic experience.

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[6] H. H. Bawden, The functional theory of parallelism, *Philos. Rev.*, 12, 1903, 309 f.

[7] H. H. Bawden, The meaning of the psychical from the point of view of the functional psychology, *ibid.*, 13, 1904, 299.

[8] Pierre Janet, La tension psychologique et ses oscillations, in G. Dumas, *Traité de psychologie* I, 1923, 919-951.

[9] G. Dumas, *Traité de psychologie* I, 1923, 686.

We have, then, as a general description of a consciousness that takes account of values, an awareness of qualities and relations more or less stabilized between an upper and a lower level, and displaying a variable tension, the rise and fall of which may easily become rhythmic. The appropriateness of this for describing the esthetic consciousness has been shown by Bawden in another set of papers, devoted especially to aesthetic values.[10] He points out, for instance, that tension is also a balance between intellectual and emotional elements. "A state of absolute emotional saturation is never actually realized any more than a state of purely unemotional intellectual cognition. These represent limits within which experience moves; limits which if ever reached would mean the destruction of consciousness."[11] This at once introduces possibilities both of conflict and of increased richness into the esthetic experience. "In the case of esthetic emotion there is the maximum of stimulation compatible with a final reinforcement by the corresponding intellectual habits or apperceptive systems,"[12]—in other words, there must be a balance-wheel or control to stabilize the tension, in order that the value may be apprehended. Hence, "if imagery in general represents controlled impulsive and habitual responses, the esthetic image represents the maximum of such mediation or control compatible with the experience as a whole remaining pleasurable. And since all successful control is normally pleasurable, it follows that all practical and intellectual reconstruction of experience tends to culminate in an esthetic moment."[13] This notion of control is, as we shall shortly see, an important addition to our means of analysis.

This enlarged conception has been brought still closer to the most specific traits of the esthetic experience by Parkhurst.[14] In two papers published in 1920 she showed how the intermittent character of consciousness may be interpreted as a challenge to us to make the most of its possibilities, after a fashion to which the artist gives us a clue; and further, how the whole course of human evolution may be read as a constant striving toward mastery of the environment, taking the shape of actual control when it can, and of an aspiration toward control when thwarted by unfavorable conditions. This added notion of mastery is the key to certain traits which the simple notion of control does not fully explain, such as Bacon's famous statement of the use of poetry "to give some shadow of satisfaction to the mind of man in those points wherein the nature of things doth deny it." Moreover, she shows us how rhythm, present from the beginning in the living organism, favors maximum ease and facility of performance, but likewise induces unconsciousness, a lapse to the habitual, reflex level. Non-rhythmic activities, on the contrary, favor maximum awareness of response, but a minimum degree of rapidity and ease; the mere effort to acquire control may, for a time at least, submerge the perception of the value intended. Here, then, is yet a third aspect of tension, as something to be maintained between a rhythm not allowed to lapse into the uniformity that brings unconsciousness and a break in rhythm not so sharp as to destroy the underlying continuity. This inclusion of the rhythmic factor that plays so great and so obvious a part in all esthetic activity is a valuable addition to the availability of the concept of tension.

Putting together these various contributions, we have in the tension necessary to the perception of esthetic value a contrast of specific awareness with habit, of non-rhythmic with rhythmic activity, of intellectual and specific control with emotional and wished-for mastery. In view of the interaction of all these, it is easy to see why individual esthetic experiences should vary so widely as we actually find that they do; to see also that all genuinely esthetic perception is active, not passive, as Bosanquet, in opposition to the 'associationist' psychology, made clear some thirty years ago.[15] Hence we need not be surprised to find that unexpected shifts which are yet felt to be parts of a single experience should accompany many of the higher reaches of esthetic reaction, as Catullus realized when he found that he could hate and love in the same moment. To put the matter in a form which is only outwardly paradoxical, tension allows us to 'enter into' the esthetic experience as conscious participants at the same time that we 'stand outside' it lest it or we be absorbed and the value, in consequence, dissipated.

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[10] H. H. Bawden, The nature of aesthetic value, *Psychol. Rev.*, 15, 1908, 217-236; The nature of aesthetic emotion, *ibid.*, 265-291; Aesthetic imagery, *ibid.*, 16, 1909, 124-141.

[11] *Psychol. Rev.*, 15, 1908, 225.

[12] *Ibid.*, 268.

[13] *Psychol. Rev.*, 16, 1909, 131.

[14] H. H. Parkhurst, The evolution of mastery, *Int. J. Ethics*, 30, 1920, 404-422; The obsolescence of consciousness, *J. Philos.*, 17, 1920, 596-606.

But the notion we have thus built up is too complex to be immediately applied to the analysis of specific esthetic situations; we need a device by which we can, as it were, 'spread out' or magnify the situation, in order to see its constitution in any specific instance. This device I find in the conception of the esthetic situation as essentially triadic. By the help of this conception we are enabled to do justice to the various aspects of the esthetic experience without confusing them, and at the same time to prevent their falling apart or being set in misleading opposition to one another. It is further important because it occurs not only in a complete state but in states relatively less complete, and thus provides a means of approaching certain problems which prove refractory to other modes of treatment. Our next step, then, is to determine the nature of this esthetic triad.

If we construct an ideal scheme of the general esthetic situation, we find on the one side an experience in which its possessor has discovered an esthetic value; on the other, an experience into which that value is to be transmitted; and between the two, a device by which the transmission is to be effected—or, in simpler terms, an artist, a work of art, and a recipient. If we assume that the original value is both singly perceived and efficiently embodied, and then received without perturbation by irrelevant influences, we shall have the triad in its 'purest' state. But it is obvious that such a state is not always actually realized, and that even when it is, there may be unmistakable differences in the values revealed. The initial experience may be more or less dense or rich, and one artist may get more out of his experience than another can; while the sensitiveness and single-mindedness of recipients are notoriously variable, and there are also the individual traits of the work of art to be taken into account. From the strictly psychological standpoint, however, the first necessity is an accurate description of the esthetic aspects of the experience of artist and recipient respectively.

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[15] B. Bosanquet, On the nature of aesthetic emotion, *Mind*, N. S. 3, 1894, 153-166.

It is also obvious that the complete esthetic triad does not often exist intact for any great length of time. An artist's period of activity may not be long, and must in any case be closed by his death; a recipient's period of acceptance may be equally interrupted. Both Shakespeare and the audiences for whom he wrote have long quitted this mortal sphere; and many living artists have doubtless not yet found their fit audience. But the work of art, by stabilizing the situation into which it is introduced, makes possible the constitution of new triads, and also serves as the basis for inference as to the traits of the original ones. In the case of Shakespeare, for instance, we have not only the plays that he wrote, but a body of information about the conditions amid which he wrote them, and also the chance of working backward from the plays to some conjectures about his own mentality. This means that the work of art is not and cannot be separated from experience, that it gets its significance precisely because of the experiences between which it is placed, though the generating experience may have passed away, and one receiving experience be replaced by another. Take the familiar remark made by the reader of a refractory novel, to the effect that he "can't get into it." He means that he has not so brought it close to his own experience that he can pass from that to the experience embodied in the novel; but there would be no point in the remark unless the work did embody an experience which could, under appropriate circumstances, be entered into.

The triads we actually encounter, then, are often temporarily incomplete; and two causes in particular render them so. One is the absence of recipients, or at least of properly qualified recipients; the other is the necessity, in certain forms of art, of a partial recreation of the work by others than the artist himself. It is notorious that many poets and musicians have had to wait years before they could count on an understanding public; and some works have not been recognized at their true value till long after their creators' deaths. Again, a building, a painting, a statue, can be seen if we can be where they are; but a piece of music must normally be audibly played, and a drama be acted. While these conditions are in part accidental, as in the collection of especially intelligent persons in particular places, or in the diffusion of skill on a particular instrument, they are not pure accidents, and their practical importance in the development of the several arts should never be overlooked. They do not, however, require further discussion at this point; and we may pass to an examination of a model triad supposed to display all three of its factors in a fully developed state.

II

The first impulse toward the constitution of such a triad must obviously lie in the perception, by the artist, of an esthetic value somewhere in his experience. It does not appear that we can profitably inquire what sort of experience is most likely to yield such a value; the records of artistic achievement show conclusively that artists have appeared in all sorts of material conditions, and at all points of the social scale. There are, to be sure, conditions that are more favorable than others to artistic development; but the cases in which apparently favorable circumstances fail to be improved, the even more obvious failure of means of artificial encouragement, and on the other side the numerous cases of successful emergence from adverse conditions, are enough to show that external encouragements and discouragements are at best merely contributory causes. Nor can we lay down with any greater exactness what qualifications an artist should have. The actually observable variety among artists stands in the way of reducing their attitudes to any standard patterns, least of all to the conventional notion of the artist as a casual, dreamy, impractical soul, hopelessly adrift in a practical world. Those who have best illustrated this notion are either not true artists at all, or artists of an inferior sort; those who seem to do so are usually perceived in the inevitable reactions from a labor not open to inspection, or in a more or less deliberate attempt to keep off the uncomprehending.

It is likewise impossible to reduce to uniformity the various motives which set artists to work and keep them at it. The mere pleasurableness of activity is no more generally effective with them than with men in other stations. If some artists work quietly and comfortably, others work like "mad laborers," as Chesterton aptly said of Carlyle. Balzac's secretary said he would not change places with his chief for the fame of Napoleon and Byron combined. There is no necessary connection between either attitude and artistic merit; it is merely a question of personal adjustment. Just so, a Milton may strive to make his own life a heroic poem, but not all poets follow his example, and he himself fell somewhat short of success in the effort. Even the desire to do as good a job as the circumstances permit, though it probably exercises a wider sway than the two motives just mentioned, is hardly universal. Those who aim at perfection most overtly are not always those who come closest to it, and happy accident sometimes carries off an unexpected triumph. It is, however, fair to say that such accidents cannot be counted on by the really lazy, and that the substitution of short cuts and makeshifts for needed exertion is a prolific cause of artistic failure.

This last remark, I think, brings us as close to the artist's attitude toward his experience as we can profitably come. We seem justified in demanding of him that he shall view his experience for himself, and find there what he is genuinely impelled to find. But we cannot prescribe either the kind or the amount of what he should see. One man will get far more than another out of masses of experience which seem pretty nearly equivalent; one man exhausts his power of discovery, where another goes on constantly enlarging and strengthening his. But we cannot judge either success or failure in such discovery until the results are reported in communicable form. The man who successfully embodies and conveys a genuine esthetic value is in so far an artist, though he does it but once; a man who does it regularly is a greater artist; a man who only pretends to do it, or who deceives himself into thinking that he has done it, is not an artist at all.

It is obvious that this double demand for technical excellence and for sincerity does not leave the artist in unfettered freedom. Even if we grant him the largest possible liberty in contemplating his experience, he must still yield to the control of external factors when he tries to embody the results of his contemplation. He must work in a definite medium, the nature and capacities of which he must know. If he transgresses physical limitations, his work will simply collapse or be impossible; if he transgresses limits seemingly less tangible, such as the conditions of literary communication, he will none the less come to grief. Yet in all this he is suffering no more than the limitations of life prescribe for everyone. So far as man is finite, he must be limited; the practical question is, where?—and the artist is no more unfairly limited than anyone else. If he is sometimes more acutely conscious of the limitations, and able to protest against them more eloquently, that may make him more conspicuous, but does not really set him apart from his fellows, or make him a peculiarly notable victim of circumstances. I think it fair to say that the artists who "do protest too much" are in general not those whom we value most highly.

The upshot of all this is that the artist is not so radically different from humanity at large that we need a radically distinct psychology to explain his conduct and his productivity. A French writer once produced a "new esthetic based on the psychology of genius;" but I do not think that inspection showed either that the esthetic was new or that genius had a psychology of its own. In truth, the introspective testimony of an artist, even when it has been recorded—as in most cases it has not—is exposed to the defects of such testimony in general, with the added ones imposed by greater excitability or ostentation, where those influences are present. Even so circumstantial an account as that which Poe gives of the genesis of the *Raven* can hardly be accepted unchecked. "When I read," says Bacon, "in Tacitus the actions of Nero or Claudius, with circumstances of times, inducements, and occasions, I find them not so strange; but when I read them in Suetonius gathered into titles and bundles and not in order of time, they seem more monstrous and incredible." Just so, when we consider the lives of Cellini or Whistler merely as sensational biographies, we find them strange enough; but after all, the record of their artistic achievement is really another matter, and when we approach them from the less excited cases with which the history of art abounds, they seem less strange even as human documents.

In short, the desire for a 'standard' type of artist is the result partly of the desire for false simplicity which we meet elsewhere in esthetic discussions, and partly of the desire that men who have accomplished striking results should themselves be striking. So long as the artist cannot readily be equated with his experience, and so long as what a man does may be inexplicable in terms of what he outwardly is, neither desire can be satisfied. But the longing for such an ideally satisfying type is keen, and so long as material to support it can be found, as of course it can, it will probably continue. But when we see, on the other side, how little we really know about many men who have actually achieved much, not to mention those of whom we know not even the names, we can hardly hope that the investigation of the artist himself will afford the best clue to the meaning of the esthetic. What we can hope to discover in this field I shall try to suggest later; meanwhile, let us accept the artist as simply a remarkable development and combination of sundry normal traits, and pass on to view our triad from the side of the recipient.

The psychological mode of approach is made easier in this case by the fact that, whereas most of us can enter but imperfectly into the artist's state of mind, we must all occupy the position of recipients if we are to perceive esthetic values at all. I do not wish to exaggerate the contrast, since I believe that the artist has no feelings which are absolutely peculiar to him; but the position of recipient certainly comes to us more frequently, and we can study it in its repeated occurrences with a much greater degree of ease and fullness. It deserves study too, since it is as necessary to the maintenance of the triad as is, on its side, the artist's activity. It is sufficiently evident that if there could be no works unless there were artists to create them, there would not long be artists if there were no recipients for their works. Mere desire for self-expression would not, I think, be a permanently effective motive; at least the possibility of acceptance and encouragement seems to be presupposed, even if an artist may, in a given case, fail of its realization. We may therefore take the two sides as necessary complements, as indeed they must be, if the triadic view is sound.

What, then, are the qualities which the recipient must bring to the sustaining of the triadic situation? They may be defined as readiness to comply with the artist's legitimate demands, and freshness in the power to respond. Compliance must not be carried so far as to accept whatever the artist offers as if his mere offering guaranteed its value; that is the road to the acceptance of all sorts of fashions and futilities. But, on the other side, the recipient should not set up irrelevant considerations of his own, and challenge the artist to overcome them; that is the road to dogmatic imposition of standards, and to critical absurdity. A state of mere compliance becomes habitual, and so non-esthetic; certain marks are held to show that the artist has done his duty, but there is no tension, and hence no genuine response. But to urge that the artist must comply with *our* standards may be equally habitual, and equally destructive of response. We may accept Pater's dictum that "failure is to form habits," if we interpret it to mean that it is forming the wrong kind of habits; an interpretation which Pater himself admits when he defines scholarship as "habitual and summary recognition." Without some basis of habit, as we have seen, the tensional reaction is impossible; but the wrong kind of habit means at best the wrong kind of tension, at worst no tension at all.

Any form of habit, then, which deadens or impedes the fresh exercise of response is esthetically mischievous. The tendency of the merely pleasurable to pass into a vague state of satisfaction with habitual stimuli has always been a difficulty, and is an added proof of the inadequacy of mere pleasure as an esthetic determinant. Still worse is the lethargy which detests anything new 'on principle,' or which declines to make the least effort toward a genuine response. That "weakness of the spectators" which Aristotle noted in the audiences of Athens has returned to plague dramatists ever since, coupled as it often is with a reaction which drives them to an excess of emotional sentimentalism, equally to the upsetting of true tragic effect. The result of the latter impulse is a tension artificially excited and maintained, a parody of the genuine sort of tension, damaging both to the recipients who submit to it and to the artists who cater to it. If such damage is most obvious in the case of the drama, its ravages are not confined to that field, nor are they less harmful for being less concentrated.

Presumably, therefore, the type of habit-formation which favors the genuine esthetic tension is one which has learned to recognize the general properties and capacities of the artistic medium in question, so as to recognize that competent handling of it which is one of the esthetic determinants, and thereby to estimate the new esthetic values presented by the individual artist. This assumes, in the recipient, a sensitiveness which can be trained by use, but which seemingly cannot be artificially supplied if it is not there to start with. Now since such a sensitiveness can neither be uniform over the entire range of the several arts, nor uniform in the temporal experience of the individual recipient, variations are bound to arise, and we need not be surprised to find that esthetic judgments fluctuate. Even if the impediments caused by unsatisfactory habitual reactions could be wholly removed—as we know they cannot—there would still remain the difficulty that the individual cannot always maintain his responsiveness at the highest pitch. We have all, I suppose, had the experience of looking forward to a concert or an art exhibition from which we promised ourselves great satisfaction, only to find the actual experience disappointing; whereas some apparently accidental or trivial occasion has given us keen esthetic pleasure, because we were precisely 'keyed up' to it. Much that is called capricious in esthetic valuation is really due to conditions beyond the recipient's control; all he can do is to discount them, and to hope for better luck next time.

In view of all these facts, we must hold that the formation of the esthetic experience in any individual recipient is a process bound to be attended by difficulties. He must always be partially limited by the situations which he actually finds in his environment; he may not be able to react with equal sensitiveness to all those which he does find; he may not be able to maintain his sensitiveness at the proper pitch as often as he would like. But at least he can discern the directions in which the chief difficulties lie, and can do his best to remove them, by seeking fresh opportunities, by getting all he can out of those he has, and by endeavoring to keep his powers of response both fresh and disposable. It is obvious that different recipients will be able to do this in different measures, and neither we nor they should quarrel with the results, so long as they are sincerely reached. The one sure means of killing true esthetic activity is to pretend that it is there when it is not; honest inability to perceive a certain value may yield to further experience, but insincerity taints the entire process, and inevitably ruins it if allowed to run for long.

We can now see more clearly that the simplified and ideal triadic scheme with which we began is subject to many modifications in the actual examples. In a sense, the artist has done his part to establish it when he has embodied his chosen esthetic value; but if we consider in sequence several works by the same artist, our judgment of each is somewhat colored by the rest. Again, the only point at which we can estimate the artist's command of his means of expression is precisely in the work which he accomplishes; and to estimate that correctly, we must fall back on a knowledge of the medium gained from observing other instances of its use. Finally, as we have just seen, the same recipient may react differently to the same work on different occasions, and in the actual process of reception one recipient is constantly being replaced by another. Consequently, what we actually find is not so much a single determinate triad as a series of triads, fluctuating according to all the possible variations of tension in all the possible recipients. The work of art tends to stabilize these by focusing the esthetic value; but it focuses with varying sharpness according to the sensitiveness of the experience to which the value is transferred, and to the presence or absence of disturbing influences. It is therefore apparent that any over-rigidity in the triad taken as a mere concept is made up for in actual experience by the inherent variety in the process of reception, a variety for which the fluctuation in individual judgments is not too high a price to pay.

III

If the foregoing descriptive account of the psychological aspects of the triad be accepted as correct, what can we say of the means at our disposal for the more detailed study of them? In the case of the artist, for reasons already suggested, I believe that we cannot go very much farther. Any cases of direct statements by artists about their procedures of course deserve careful collection and scrutiny; but they are at best less numerous than we could wish, they do not always bear on the points which most need elucidation, and they must always be examined for unintentional misstatements. An artist is not always able to record the different stages in the working out of a complex conception in the exact order of their actual occurrence; and he may not have paused to note, or may be incapable of noting, the deeper forces which led him to do as he did. Consequently, to work back from the existent facts of a work of art to the presumed state of the artist's mind in a way which is at best highly conjectural; and it has yet to be proved that the procedures of psychoanalysis are really any sounder. Even apparently objective data may be manipulated in a hazardous way, as is seen, for instance, in certain recent studies of Shakespearian punctuation,[16] or in the efforts to assign anonymous paintings and statues to definite artists by the sole help of technical criteria. When employed with care, and in conjunction with all the other external evidence available, such modes of study have their use; but when employed for their own sake, they are fatally likely to run into extravagance, and the only 'psychology' on which they are likely to enlighten us is that of the critic.

The other great resource of present day psychology, experimental procedure, seems equally inapplicable to the artist's mentality. We can scarcely subject the artist to laboratory conditions while he is in the actual course of production; and even if we could, it is to be feared that the resulting facts would not be of a nature to throw real light on his achievement. Since such procedure is obviously out of the question in the case of artists no longer living, a vast range of esthetic phenomena is by definition beyond our grasp, except as we can more or less reconstruct it. I repeat that I see no valid objection to such attempted reconstruction, provided that we recognize the extremely hazardous nature of the task, and the extremely provisional character of the results. What we are likely to get by it is not a contribution to scientific esthetics, but an experiment in hypothetical biography, the subject of which happens to be an artist. The results may be interesting enough in their way, but we must beware of taking them for what they are not, and cannot be.

When we turn from artist to recipient, we seem at once to be on surer ground. We can all, as we noted above, be recipients; and there is no reason why the conditions of reception should not be made the object of experimental study. This, indeed, is precisely the nature of experimental esthetics as it has thus far been carried on. Here too, however, the results seem to be limited. The matters most susceptible of experimental treatment must be relatively simple and easily repeatable; which amounts to saying that they are for the most part rather unimportant elements in any actual esthetic situation. By themselves they do not carry us very far; and as elements in a more complex situation they may be obscured or altered. Hence the actual results of experimental esthetics have been somewhat meagre, and even within their own limits they have not been free from uncertainty.[17] Moreover, they have sometimes been worked out with too small a body of subjects, making the extension of the results insecure; or, if a wider range of subjects is sought, the difficulty of securing uniform conditions, and of excluding disturbing factors, becomes steadily greater. It therefore seems unlikely that the direct experimental method can do more than determine the general conditions favoring the acceptance of the simpler esthetic values, discounting personal variations except in so far as they throw light on differences of individual sensitiveness, and so may help to constitute distinct types of esthetic reaction.

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[16] Excellently summarized and critised by R. M. Alden, The punctuation of Shakespeare's printers, *Publ. Mod. Lang. Assn.*, 39, 1924, 557-580.

It may be thought that we are thus brought to a dilemma. If psychology be interpreted in a narrow and strictly experimental sense, and if everything not susceptible of experimental treatment be ruled out, it would seem as if the psychological aspect of esthetics must be severely limited. If, on the other hand, we use less rigid methods, like reports of personal experiences or widely distributed questionaires, we may be accused of forsaking proper scientific methods, and sacrificing all hope of certainty. It does not seem to me, however, that our condition is really quite so desperate. Until there is greater agreement among psychologists as to the exact scope and procedure of their science, it seems permissible to argue that both methods are legitimate, and that the second, under proper restrictions, may be used to supplement the first. If once we grant that the experiences of artist and of recipient are susceptible of psychological treatment—and I see no reason why we should not grant it—the question becomes simply one of the best way to accomplish such description, and if one device does not work, we are at liberty to try another, provided we do so with proper caution. A frank recognition of the difficulties in the road is not a reason for giving up, but only for being more careful; and if the esthetic situation is really as complex as it appears to be, there is the more reason for supposing that only a correspondingly elaborate attack on its problems can hope for success.

Such a hope, however, seems to me to depend on utilizing the notion of the triad as a means of keeping both psychological aspects of the situation always in view, even if, in default of evidence or in pursuit of a special aim, we temporarily emphasize one at the expense of the other. "The oral discussion," says Hoernlé in his report of the symposium on esthetic problems at the Oxford Congress of Philosophy in 1920,[18] "turned chiefly on this very point: does the artist's creative activity or the spectator's enjoyment give the better clue to the esthetic psychosis?" Surely the truth is that neither by itself gives adequate clues, since neither in the long run stands by itself or functions by itself; and to concentrate attention exclusively on either aspect will at best over-simplify, and at worst disrupt, the entire situation. Furthermore, if two aspects of that situation are directly psychological, the third—the work of art—is not; and, as normally more permanent than the experiences between which it is interposed, it often serves as the most convenient point of departure from which to study the entire situation, and is our only basis for psychological inference when the situation is no longer accessible in its original entirety.

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[17] The work of Fechner and his school is reviewed by Charles Lalo, *L'esthétique experimentale contemporaine*, 1908. Some more recent contributions are discussed by Edward Bullough, Recent work in experimental aesthetics, *Brit. J. Psychol.*, 12, 1921, 76-99. For the present status of experimental procedure see Knight Dunlap, The experimental methods of psychology, *Ped. Sem.*, 32, 1925, 502-522.

It may make the issue clearer if we briefly apply this notion of the triad to two specific problems of esthetic theory: the status of 'natural' beauty, and the possibility of self-criticism on the part of the artist. The existence of situations in which, by definition, no work of art is present, and which are yet felt to offer an esthetic value, has long been a disputed matter. The controversy on the subject has swung between the extremes of declaring only art to be beautiful, and of declaring that art is a mere parasite on nature; but it has gradually settled down to a recognition that nature cannot readily be so defined as to make possible an absolute separation of it from art. To say with Whistler that "art is art because it is not nature" is after all not to give a very clear idea of what nature is; to say with Hegel that works of art have a monopoly of beauty seems to fly in the face of ordinary experience. Yet if the triadic notion is sound, it ought to be possible to show that the natural situation can, without straining, be made to reveal at least its germ.

Let us take the simplest imaginable case, that of a natural situation which is felt not merely to be pleasurable, but to contain a positive esthetic value. Is it not true that we tend to note this latter as an aspect within the whole, an aspect which, if we had the requisite ability, we might very well try to embody in a work of art? Again, does not the photographer who seeks an effective result incline to wait for the appropriate background of clouds, or the appropriate lighting, which will make the scene look as it 'ought to look'? He does not take the situation as he finds it, but reacts to it in such a way as to emphasize a present value or to note the absence of the determinants of such a value; and he will recognize that he must either wait for more favorable conditions or accept something which he feels to be less valuable. In other words, every observer must partially detach the esthetic element from the total and consider it more or less by itself, if he is to perceive it as esthetic at all. The work of art would then simply carry this detachment one stage farther, and, by making permanent the value in question, supply the possibility of creating new situations after the first one has passed away or been passed out of. I agree that the complete triad as we at first formulated it is a development from the original natural situation; but I hold, also, that it is a continuous development.

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[18] R. F. A. Hoernlé, *Philos. Rev.*, 30, 1920, 67. The symposium, a landmark in the recent literature of the subject, is printed in *Brit. J. Psychol.*, 11, 1920, 1-56. See also the brief summary and comment by C. E. Whitmore, *Monist*, 31, 1921, 601-608.

The second question, that of the possibility of the artist's occupying the position of recipient toward his own work, has also been variously answered. The view which would regard the artist as having his own 'psychology of genius' is likely to deny it because of more or less mystical doctrines of 'inspiration,' according to which the artist mysteriously receives his impulse from without, and is the purely passive channel through which the work finds expression. No doubt evidence in support of this view exists; but on any showing it is not the whole view. There are numerous cases of artists who set about their work in a very business-like and objective way, and who are afterwards able to tell us a good deal about it. In terms of our tensional psychology, the esthetic impulse is valueless unless it is subjected to the retarding influence of a work of art which not only stabilizes it but allows it to set up new situations. But how far any individual artist is conscious of this process while it is going on, or cares to reconstruct it after it is over, will depend on his idiosyncrasy. An indication of this is the very different degree in which artists concern themselves about their works after they have been produced. Some keep systematic note of them and are much interested in their fortunes; others seem to care little about them, but at once go on to a fresh task. Such differences are natural enough, as soon as we have abandoned the notion of a standard 'artist-mentality;' and the sensible conclusion would seem to be that such self-division on the artist's part as shall enable him to see his own work objectively is entirely possible, but by no means universal.

If we now pause for a moment to review the various points which have thus far been made, I think it will be seen that we have in this notion of the esthetic situation as a tension between two levels, reconciling within itself a complexity of factors and differently adjusted in artist and in recipient, perhaps the only one which is adequate to the variations of the esthetic activity as we actually find it in experience. By its help we can find psychological equivalents for the intellectual element of control and the emotional element of mastery, and by the added device of the triad we can keep both in contact with the work of art which is the vehicle of the esthetic value. Moreover, as Bawden previously saw, it helps to give us a right conception of the much disputed notion of 'freedom' in art. "If our preceding analysis of the relation of the sensuous and ideal elements in beauty is correct, it is not the presence or meaning of purpose, but the irrelevance of the purpose, which interferes with an object being beautiful. Meaning is no barrier to beauty if the meaning be intrinsic, *i.e.* relatively adequate as an embodiment of the relations which it suggests. We are free in the esthetic experience, but we are not free from all ends or meanings; we are free only from necessary relation to an extrinsic end. Indeed, the freedom is gained just by the controlled or relatively adequate organization of what would otherwise be transgredient ends, into terms of an harmonious but internally diversified system."[19] Again, "Not the presence of purpose but the particularity and arbitrariness of the purpose militate against beauty. There must be relevancy as well as elusiveness in order to call out those deep-lying intellectual habits whose exploitation along not too unfamiliar lines is pleasurable."[20] Art is in one sense not free, so far as it is bound to aim at the purest and intensest of embodied esthetic values; it is, in another sense, free to adopt all sorts of means towards that end. The freedom of mastery which ends in mere aspiration without corresponding control is barren; the bondage of control which issues in command of a particular expressive medium may be the artist's true means of liberation. It is the blend of mastery and control in the achievement of the esthetic value which reconciles the two, so far as such reconciliation is humanly possible.

What, in conclusion, are we to say as to the value of the psychological approach to esthetics? I believe that it can tell us much more about the psychological demands which are imposed upon both artist and recipients than about the causes of the esthetic experience. These demands, in a word, issue in the requirement of maintaining relevant sensibility at the highest possible pitch. The portions of an artist's experience or of his efficiency which he cannot bring to bear for artistic ends do not interest us esthetically; the ability of one recipient to contribute more in the way of response than another can do, to stand a higher degree of tension, determines for the former an intenser esthetic experience, not to be depreciated by another who is incapable of it. We must realize, that is, that genuine esthetic values are relative to the capacities of actual human beings, and that the important thing is to discover in the actual cases just what artists and recipients are capable of under the most favorable conditions, whatever those may prove to be. Any mode of psychological study which tends to reduce its results to a dead level of 'average' achievement is bound to leave out of account many of the most characteristic aspects of the esthetic experience. So long as the desire for 'objectivity' in psychology leads in that direction—as in certain cases it seems to do—I doubt if genuine esthetic theory will be greatly helped by it.

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[19] H. H. Bawden, *Psychol. Rev.*, 16, 1909, 132.

[20] *Ibid.*, 133

But it also seems to me unlikely that the psychological approach can ever bring us very close to the chief determinants of esthetic value; that what it tells us about the demands made on artist and on recipient can ever be translated into explanations of esthetic valuation. If we accept the triadic view of the situation, it is at once clear that at most only two-thirds of that situation can be interpreted in psychological terms; that the psychology of the individual artist is often undiscoverable; that the psychology of the recipient tends to reduce itself to a general statement of the elementary conditions that favor esthetic awareness; and finally, that neither of these sides is psychologically existent until an actual esthetic value has been discovered and embodied. Moreover, the control of such values by the sense of efficient technical handling falls outside the psychological field, and in fact demands a very different line of approach. If a large part of all this does not fall within the field of psychology, I should be at a loss what to name its location; but enough remains outside to explain why it is that esthetic theory can still be pursued while the rival systems of psychology are trying to decide which of them, if any, can swallow up all the rest.

AN EXPERIMENTAL STUDY OF FATIGUE IN THE AUDITORY MECHANISM[1]

By FRANK A. PATTIE, JR., Harvard University

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It seems that in the past most physiologists and psychologists have assumed that the auditory mechanism is not subject to fatigue. No mention is made of such fatigue in any textbook of physiology or psychology known to the writer except Nagel's *Handbuch.* The work done on fatigue in audition has been meagre, with technique not above criticism, and often with negative or uncertain results. In fact, the failure of previous investigators to demonstrate the phenomenon with any certainty led Professor L. T. Troland, relying mainly upon the negative results of Sewall, to say in 1914 that "stimulation of the ear with a sound of constant intensity does not result in a decrease in the apparent intensity of the tone or noise, once the tensor tympani is adjusted, if the attention is unwavering."[2]

The work here reported was undertaken for the purpose of clearing up the uncertainty connected with the problem of fatigue in audition. The problem was to find out (1) whether there is a decrease in the intensity of a tone after the ear has been stimulated for a definite period of time, and (2) whether this decrease, if it occurs at all, is conditioned by psychological (especially attentional) factors and is only an apparent decrease, or is actually due to fatigue of a physiological nature.[3]

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[1] From the Psychological Laboratories of Harvard and Princeton Universities. The writer takes this opportunity to express his thanks to Professors H. S. Langfeld, E. G. Boring, and M. J. Zigler for suggestions about this problem, and to the Scientific Equipment Division of the Western Electric Co. for the loan of apparatus.

[2] L. T. Troland, Adaptation and the chemical theory of sensory response, this JOURNAL 25, 1914, 500.

[3] The meanings of 'physiological' and 'psychological' as here used are illustrated by the names given to Hering's and Helmholtz's theories of simultaneous visual contrast; the former is very often called a physiological, the latter a psychological theory. The words are not meant to have any definite theoretical (*e.g.* dualistic) implication.

HISTORY OF THE PROBLEM

There have been three lines of research on auditory fatigue; investigations have been reported on (1) the effect of prolonged stimulation of one ear upon the intensity of tones subsequently heard by it, (2) the effect of such monotic stimulation upon the localization of a subsequent diotic tone, and (3) the ultimate disappearance from consciousness of very high tones near the upper limen for pitch. This paper is concerned with the first of these methods of investigation.

A summary of casual and experimental observations (except for the work of Huijsman) prior to 1905 is given by K. L. Schaefer in Nagel's *Handbuch*.[4] Albrecht[5] summarizes all the work prior to 1919. It is therefore necessary to mention here only the more important experiments.

J. J. Müller[6] in 1871 reported that a clang sounded weaker and less rich if the ear had previously been stimulated by the frequencies corresponding to the overtones of the clang. The clang heard in an ear thus fatigued was compared with the same clang when heard in the unfatigued ear.

Urbantschitsch[7] in 1881 used tuning fork tones conveyed to the ears through rubber tubes. A tuning fork, after sounding before one of the tubes for 10 or 15 sec., was partially damped; and after its tone had died out completely it was removed to the open end of the tube leading to the other ear, whereupon the tone was regularly heard for several seconds by the unfatigued ear. The fatigue reduced in this way the intensity of tones, but only of those tones having the same frequency as the fatiguing stimulus. The duration of the fatigue-effect ranged from 2 to 5 sec.

In 1884 A. Huijsman,[8] of the laboratory of F. C. Donders, reported experiments designed to test the conclusions of Müller and Urbantschitsch. The latter's experiment was repeated with the added precautions of keeping the *Os'* heads steady by means of a biting board and fastening the fork in a block. With these conditions, the results were negative with 5 *Os*; without these conditions the results were "uncertain." Urbantschitsch had said that a fork tone loud enough to be heard by both ears was heard as louder by the unfatigued ear, but the *Os* could not be sure that this was the case. Huijsman, after a long account of experiments on the disappearance of high tones, reports experiments which do show fatigue to some extent, but apparently the two sets of experiments are not brought into relationship with each other. He conducted tones of 240 v. and 250 v. of equal intensity successively to the same ear. After stimulation by one of the tones for one or more minutes, the tones were compared, but the intensities were "little or not at all different." The apparatus was then modified so as to reduce the intensity of these tones until they were barely audible, whereupon "the difference remained slight, and where a difference was noticed it was due to a loss in the tone which had been used as a fatiguing stimulus." Forks of 210 and 250 v. were used, but with less convincing results, because of the difficulty of judging intensity where there was such a difference in pitch. In another experiment a 250 v. fork tone was led by a branching tube to the ears. After 2 minutes' stimulation of the left ear, the tone was heard stronger in the right ear when the tone was presented to the ears successively at low intensity. In several of these experiments no statement is made as to the number of *O*s.

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[4] K. L. Schaefer, in W. Nagel, *Handbuch der Physiologie des Menschen*, 3, 1905, 509-512.

[5] H. Albrecht, Zur Frage der physiologischen Ermüdbarkeit des Hörnerven, *Beiträge zur Anatomie* [etc.] *des Ohres* [etc.], 13, 1919, 202-229.

[6] J. J. Müller, Ueber die Tonempfindungen, *Ber. d. sächs. Ges. d. Wiss., math.-phys. Kl.* 23, 1871, 115-124.

[7] V. Urbantschitsch, Zur Lehre von der Schallempfindung, *Arch. f. d. ges. Physiol.*, 24, 1881, 574-585.

[8] A. Huijsman, in *Onderzoekingen gedaan in het physiologisch laboratorium der Utrechtsche Hoogeschool*, 3de Reeks, 9, 1884, 87-142.

The work of Sewall[9] was designed to give quantitative results. He planned to fatigue one ear with a given tone, then conduct the tone to both ears and have the *O* regulate the intensities so as to get equal loudness in the two ears. He hoped to use median localization in the head as a criterion of this intensive equality. The tones (from organ pipes and an electromagnetic siren) were conducted through telephones to the *O*s. The wires from the microphone near the source of sound ended in a primary coil, around which was a secondary coil, by adjustment of which the intensity of the tone received could be varied.

Preliminary tests showed that the adjustment of the sound intensities was very much modified, or at least rendered difficult, by the influence of suggestion. In order to eliminate this factor, the apparatus was so arranged that the position of the regulatory coils gave no clue to the relative intensities in the two ears.

Sewall stimulated one ear, say the left, for one or more minutes, and then the circuit to the right ear was also closed; whereupon *O* had to say in which ear the tone was louder (judging this indirectly from the localization within the head). In the first experiments the *O* regularly had the impression, as soon as the circuit on the right was closed, that the sound was very much louder on that side and that the source of sound was located in the right ear. *E* then sought to diminish the intensity on the right side so that the sounds would appear equally intense after the fatiguing stimulation, but such an enormous attenuation was required for the compensation of the 'fatigue' that Sewall came to doubt the previous interpretation of the phenomenon. The localization, moreover, did not change gradually from the unfatigued ear back to the median plane, but suddenly. These facts led Sewall to believe that the phenomenon was due to an attentional factor; the sound in the unfatigued ear sets in as something new, while in the 'fatigued' ear the tone simply continues as before. To test this hypothesis as to an attentional factor, he interrupted the tone in the 'fatigued' ear before allowing the two sounds to act diotically. An interruption of less than a second was sufficient to cause the 'fatigue' effect to vanish.

Albrecht[10] in 1919 reported a small number of experiments with himself and Dr. K. L. Schaefer as *O*s. A tuning fork was struck (c^2, c^4, and c^5 were used), and the average time during which it was audible was noted. Then a whistle of the same pitch was used as a fatiguing stimulus for one or more minutes, and immediately thereafter the fork was again struck and the duration of audibility again noted. This period of audibility was regularly shorter than it was when the ears were unfatigued.

J. C. Flügel[11] investigated the effect of fatigue on intensity by the use of tuning forks and rubber conducting tubes after the manner of Urbantschitsch but with some improvement on the method. In a series of experiments on 5 *O*s 40 trials were made on each ear, and in 46 % of the trials fatigue was shown. There were no marked differences between *O*s or between ears of the same *O* with respect to the degree or duration of the fatigue, which seemed to reach a maximum after stimulation for 40 sec.; a 3 min. period gave no increase in fatigue. No difference was found between the fatigue-effects of a loud sound and of a weak sound. The duration of fatigue was seldom more than 30 sec. In an experiment designed to test the effect of a fatiguing stimulus upon the loudness of tones of other pitches, all combinations of the forks 256, 320, and 512 v. were used. In all 214 judgments were taken, and in only 10 % of the judgments did the *O*s report that stimulation by one frequency caused a decrease in the intensity of other frequencies. Flügel regards this apparent decrease as due to errors of observation, since in about the same percentage of cases the opposite effect occurred (*i.e.* the sound was heard longer on the fatigued than on the unfatigued side). Further experiments with two *O*s showed that transfer of the fatigue-effect did not occur outside the range of their DLs for pitch.

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[9] E. Sewall, Beitrag zur Lehre von der Ermüdung des Gehörorganes, *Zsch. f. Sinnes., physiol.*, 42, 1907, 115-123.

[10] Albrecht, *op. cit.*

[11] J. C. Flügel, On local fatigue in the auditory system, *Brit. J. Psychol.*, 11, 1920, 105-134, esp. 123-133. The greater part of this paper is concerned with the shift of localization produced by fatigue.

The latest work is by Weinberg and Allen.[12] A tone from a Stern variator enclosed in a sound-proof box was interrupted periodically by a revolving disc and the slowest rate of interruption was found which still gave the impression of a continuous tone. The ears were then stimulated by another variator-tone for two minutes, and another reading was taken. The tones ranged from 140 to 250 v. The results indicated that "as a result of fatigue the persistency of sound impressions is greatly increased. The increase is a maximum when the frequency of the fatiguing tone is the same as that upon which the measurements are taken." The entire range affected was about 8 v. on each side of the maximum, and only a few tones very near the maximum were affected to any great extent. An idea of the order of magnitude of the effect may be had from the statement that fatigue reduced the length of the separate impulses of sound necessary for the impression of a continuous tone from about 0.020 sec. to about 0.018 sec. A decrease in the fatigue effect as the fatiguing tone increased in intensity "was clearly indicated," as was also an increase of fatigue with an increase of frequency. The authors interpret their results in terms of the resonance theory of audition.

The thesis of E. Schäfer[13] lies somewhat out of the range of this paper because of the phenomena he studied. His work is rendered of doubtful scientific value because he does not say whether he used more than one *O* (presumably himself). He investigated the fact that, if one sounds two tones, one from a tuning fork and the other from a monochord loud enough to mask the fork tone, and then damps the monochord tone suddenly, an interval of 0.01-0.15 sec. ensues in which the fork tone is not heard. The louder the monochord tone, the longer the silent interval. After becoming audible, the tone reaches a maximum loudness which exceeds the loudness it would have if heard without the preceding monochord tone, *i.e.* the subjective intensity runs beyond the objective intensity, as it were. These phenomena are explained as "more a psychological than a physiological process," since they are interpreted as due to cortical processes correlated with a shift of attention; and Schäfer gives an elaborate psychological explanation, adhering very closely to Wundtian theory.

He also investigated the fact that the length of time a struck tuning fork takes to become inaudible is increased if a fork of not very different pitch is sounded at the same time. The accompanying fork is damped at the moment when, on the average, the first fork (sounding singly) becomes just inaudible, and it is then found that the first fork is heard a few seconds longer than this average time. Schäfer gave a physical explanation of this phenomenon; he believed it to be due to a slight shift of the locus of excitation of the basilar membrane caused by the interaction of the two frequencies. This shift, too small to be appreciated as a pitch change, would allow the fiber that would have been stimulated by one of the tones sounding singly to "rest" and therefore to respond to the tone so as to give a sensation for a few seconds after the damping of the accompanying tone. Hence Schäfer thought that this work was an indirect attack on the problem of fatigue. He was not able to duplicate the results of Urbantschitsch with the latter's method, but by closing the ears before a tuning fork was struck and afterwards opening them at the time when the fork tone would otherwise be just inaudible, he regularly heard the fork a few seconds longer.

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[12] M. Weinberg and F. Allen, The effect of aural fatigue upon the critical frequency of pulsation of tones, *Phil. Mag.*, 47 (6th ser.), 1924, 126.

[13] E. Schäfer, *Ueber Ermüdung des Ohres*, 1911.

To sum up critically, it may be said that doubt is cast upon the efficacy of Urbantschitsch's method by the work of Huijsman and Schäfer. The only quantitative data on the method are given by Flügel, who demonstrated fatigue in only 46 % of his trials, a percentage which shows that fatigue, as revealed by this procedure, is a labile phenomenon. The disadvantages of this method are that it involves, in at least some of its forms, reports on the presence or absence of liminal tones and that these difficult reports may be influenced by such factors as suggestion, confusion by other sounds, diversion of attention, etc. Sewall found in his experiment that suggestion was a strong influence.

The reason why Sewall was not able to demonstrate fatigue seems to be the fact that his measure of fatigue was unreliable. He tried to have the *Os* judge on the intensities of two tones presented simultaneously, one to each ear. The judgment of intensity was hence indirect, mediated by the localization of the resulting (fused) tone within the head. The adjustments of the apparatus to secure median localization, when both ears were unfatigued, were subject to considerable variation; and Sewall says in this connection, "Man sieht, dass die Genauigkeit binauraler Tonstärkevergleichung sich hier als eine sehr geringe herausstellt."[14] Undoubtedly it required a greater intensity-difference between the two ears to cause an appreciable shift in the localization of the acoustic image than that difference which was produced by one of the ears having been stimulated for two minutes.

We have in the work of Schäfer no direct proof that the ear is subject to fatigue, since his interpretation of his results as fatigue rests upon a very shaky theoretical structure.

Some references to articles not mentioned in this paper on account of lack of space are given in a footnote.[15]

APPARATUS

The instrument used to produce tones was an audio-frequency oscillator with a range extending from 440 to 4500 cycles per sec. The tones were produced in the telephones of a radio headset, and the intensity of the tone in one telephone could be varied independently of the other tone by passing the current through a variable resistance. A circuit diagram of the oscillator is given in Fig. 1.

Rubber holders or cushions of annular shape, about 5 cm. in diam. and 3.5 cm. in depth, enclosed the telephones and held them against the ears, making a snug and practically sound-proof joining around them. The diaphragms were about 1 cm. further from the meatus than they would have been if the holders had not been used.

A difficulty was encountered at the beginning of the experiments, in that the telephones, even when disconnected from the oscillator, would give a weak tone (through induction) when the oscillator was operating nearby. This trouble was removed by short-circuiting the wires leading to the telephones by means of small relays. When absolute silence was wanted in a telephone, it was short-circuited; when the experimenter wished to, make a tone in it, he closed a double-pole switch, which (1) connected the oscillator circuit with the wires leading to the telephone, and (2) closed a circuit through a relay, the effect of which was to break the short-circuit of that telephone. Small electric buzzers of the type used for the practice of radio signals were adapted for use as relays and attached to the backs of the telephones. The headset with these relays was rather heavy, but a counter-weight passing over a pulley was arranged to relieve the *O*s of the burden on their heads.

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[14] Sewall, *op. cit.*, 118.

[15] W. Köhler, Akustische Untersuchungen III und IV, *Zsch. f. Psychol.*, 64, 1913, 95 f.
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R. Bleyl, Die funktionelle Ermüdung des Gehörorganes, *Arch. f. Ohren-, Nasen-, u. Kehlkopfheilkunde*, 108, 1921, 192-197.

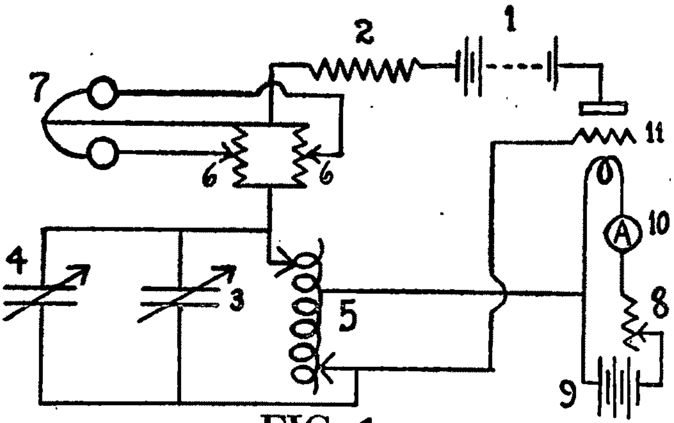

FIG. 1

Fig. 1. Circuit Diagram of the Audio-Frequency Oscillator. 1, 67 volt 'B' battery; 2, 5000 ohm resistance; 3, Dubilier step condenser, ten 0.001 mf. units; 4, General Radio Co. air condenser, 0.001 mf.; 5, tapped inductance, 3 lb. No. 30 D. C. C. wire; 6, 400 ohm potentiometer; 7, telephones; 8, 6 ohm filament rheostat; 9, 6 volt 'A' battery; 10, filament ammeter; 11, Western Electric Co. 216-A vacuum tube.

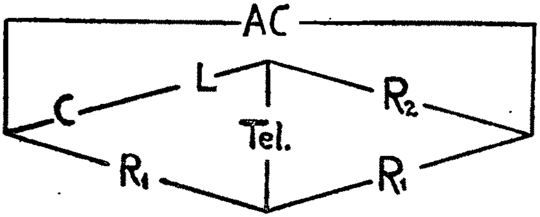

FIG. 2

Fig. 2. Diagram of Audio-Frequency Bridge. The output of the oscillator is received at AC. C, capacity; L, inductance; R, resistance; Tel., telephone.

The oscillator was calibrated for frequency by means of an audio-frequency 'bridge' (Fig. 2) similar to the Wheatstone bridge for the measurement of resistance. This divided circuit, with constant capacity C and resistances R_1 and R_2 (the values of R_1 and R_2 being chosen to balance with the resistance component of the LC circuit), but with variable inductance L, received the output of the oscillator of unknown frequency at AC, and the inductance L was then varied until the tone heard in the telephone was of minimum loudness. With this value of L the apparent resistance to the alternating current of the branch of the circuit containing C and L balanced R_2. The value of L was now read off and the frequency of oscillation found from a curve plotted from the formula $f = \frac{1}{2\pi\sqrt{LC}}$.

Since the tones of this oscillator were not pure, the need was felt for confirmation of the results with pure tones. The Scientific Equipment Division of the Western Electric Company very kindly loaned one of their 2-A Audiometers (which produce pure tones by means of electric oscillations) for this purpose. This instrument furnishes 8 tones at intervals of an octave, beginning with 64 and ending with 8192 v. These tones are approximately as pure as tuning-fork tones of the same intensity.

A further advantage of this instrument is that the intensity of the tones is accurately calibrated in terms of 'sensation units', *i.e.* when the energy of the tone is increased by one sensation unit the initial energy is multiplied by the Fechnerian ratio.[16]

METHOD OF INVESTIGATION

The fatigue was investigated by means of a comparison of the intensities of a tone of constant energy[17] and frequency when heard (1) by a normal ear, and (2) by the same ear after being subjected to prolonged stimulation. This comparison was made by a method which may be described as follows:

Let E_l and E_r represent the left ear and the right ear, respectively. Two tones of the same pitch, symbolized by T_l and T_r, were used as stimuli for E_l and E_r, respectively. The two tones were presented successively, T_l always to E_l and T_r always to E_r, each for about one second, for comparison as to intensity. The energy of the tones was so arranged by the experimenter that in a small percentage of the cases the intensity of T_r was judged greater than that of T_l. Not less than 50 of these preliminary judgments were taken. The instructions given *O*, who was seated where he could not see the adjustments of the apparatus, were: "Compare the intensity of the tone heard in the left ear with that of the tone heard in the right ear, and report whether the intensity of the tone last heard is greater than, equal to, or less than that of the tone first heard." Other pairs of tones of the same pitch but of rather pronounced differences in intensity were interspersed among the T_l-T_r pairs, in order to keep *O* alert, but only the judgments on the T_l-T_r pairs were recorded.

After these judgments had been taken with both ears in a normal condition, E_l was stimulated by the fatiguing tone F_l (often T_l and F_l

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[16] A more detailed definition is furnished by the handbook accompanying the Audiometer as follows: "When the dial is rotated, thus changing the voltage at the receiver terminals, the number of sensation units corresponding to this change is equal to 20 times the common logarithm of the voltage ratio. At any given pitch, the pressure on the ear is proportional to the voltage. The pressure on the ear is therefore changed by a ratio of 10 to 1 when the dial is rotated through 20 sensation units. The energy absorbed by the ear is changed by a ratio of 10 to 1 by rotating the dial through 10 units. The smallest steps are 5 sensation units, corresponding to a pressure ratio of 1 to 1.77 or an energy ratio of 3.16." (The pressure of a tone is directly proportional to the square root of its energy.)

[17] Hereafter in this paper the word 'intensity' refers only to sensation and not to stimulus, for which 'energy' is employed.

were the same) for 1 min. At the end of the minute F_l was cut off, and the tones T_l-T_r were given successively for comparison immediately thereafter and also at intervals of 10 sec. during a time ranging from 30 to 100 sec. after the cessation of F_l. After every pair *O* reported his judgment on the intensities of T_l and T_r, and the judgment was recorded by the experimenter. After F_l had been given once for the minute-period and the subsequent series of intensity-judgments taken, the experimenter then had recorded one judgment taken immediately upon the cessation of F_l, one judgment taken 10 sec. afterwards, one taken 20 sec. afterwards, and so on. The fatiguing tone was now given again and another series of judgments at 10-sec. intervals taken, and this process was repeated 15 or 20 times in the course of the sitting.

Since E_r was not subjected to any prolonged stimulation, it may be considered, for the purpose of this experiment, as unfatigued. Hence the intensity of T_r in E_r was treated as a constant element in the experiment and served as a standard against which the intensities of T_l (1) when E_l was fatigued and (2) when E_l was unfatigued were measured. The intensity of T_r was the middle term which allowed these two intensities of T_l to be compared and a fatigue effect was thus shown, if present, by a decrease in the intensity of T_l after prolonged stimulation of E_l.

This method may seem rather indirect, in that the fatigue is measured by the *O*'s reports indicating a relative increase in the intensity in E_r, the unfatigued ear, rather than by reports showing a decrease in intensity in E_l. The merit of the former procedure lies in the probability that it is less open to the influence of suggestion: if *O* were asked to report on the intensity in E_l he might naturally expect it to be less after a minute's stimulation, and this expectation would be a complicating factor in his judgments; whereas, in order to gain an expectation of an increase in the intensity in E_r, *O* would have to think over the experimental situation, a less likely possibility.

Variations of this fundamental method are discussed in a subsequent section.

The data obtained by this procedure may be presented either in tabular or in graphic form. The results of one experiment, in which the left ear of *C* was fatigued for 1 min. by a tone of 512 v. at an intensity obtained by setting the potentiometer dial[18] at 85, are shown in the graph in Fig. 3. The ordinate represents the percentage of judgments in which the intensity in the right ear was judged greater. The abscissa is marked off in units of 10 sec. and represents the time after the cessation of F_l. What the graph is designed to show is the percentage of judgments of 'greater intensity on the right' as a function of the time elapsed since the cessation of F_l. The dotted horizontal line which is drawn through the 18% line is called the 'normal line' and represents the percentage of judgments of 'greater on the right' which occurred when, at the beginning of the experimental hour, the *O* compared T_l and T_r under normal conditions 100 times. Each point plotted on the curve is derived from 20 judgments after stimulation of E_l. This particular curve is one of the smoothest obtained in this investigation.

The data upon which this curve is based are given in tabular form under Fig. 3. The same method of tabulation is used in Tables I to IV. The italicized numbers under the column heading 'Energy' represent sensation unit values on the audiometer; plain numbers represent readings on the potentiometer dial of the oscillator.

When the effect of a loud tone was to be compared with that of a weak tone, the loud tone was first used to stimulate the ear for 1 min., and a series of judgments was taken immediately afterward. Then the weak tone was used as a fatiguing stimulus for 1 min. and a similar series of judgments taken. The two tones were thus used alternately until the end of the experiment. The purpose of this alternation was to insure that any changes occurring in either *O*'s condition or the apparatus during the experimental hour would be reflected in both fatigue curves. The same method

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[18] Large numbers on the potentiometer dial of the oscillator corresponded to loud tones, and conversely.

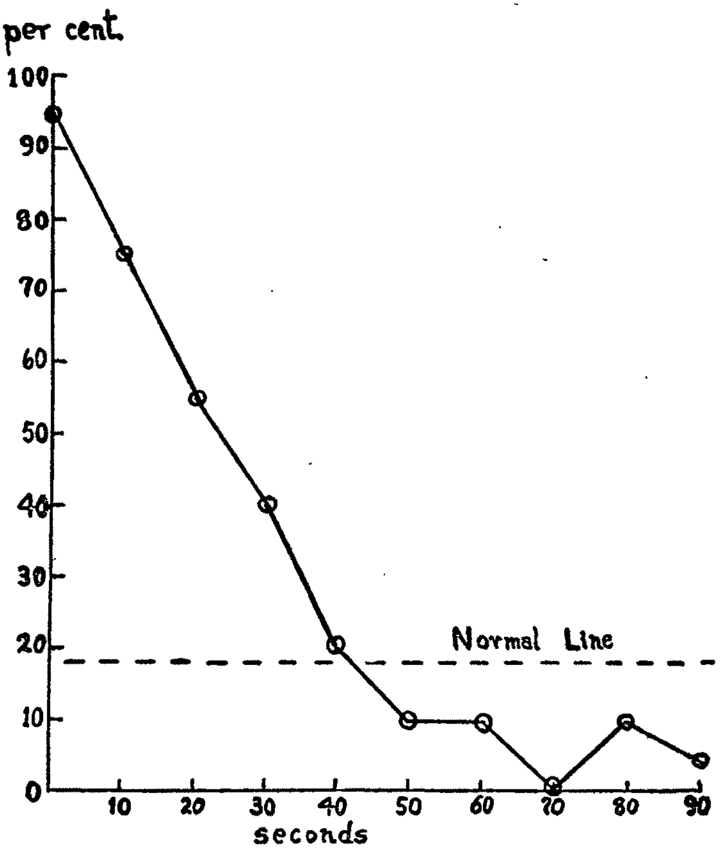

FIG. 3. GRAPH OF RECOVERY FROM FATIGUE, ILLUSTRATING THE RESULTS OF ONE EXPERIMENT. (See text.)

Tabulation:

		Fatiguing tone F						Percentage of judgments 'greater on unfatigued side' at									
O	Ear	pitch	energy	duration	T_l-T_r pitch	Normal line at	No. judgments	0	10	20	30	40	50	60	70	80	90 sec.
C	L	512	85	1′	512	18	20	95	75	55	40	20	10	10	0	10	5

of alternation was used when the effect of increased duration of the fatiguing tone was tested and also when the influence of different pitches was investigated.

OBSERVERS

Thirteen *O*s were employed, 4 at Harvard in 1923-1924 and 9 at Princeton in 1924-1925.

The curve of fatigue shown in Fig. 3 represents the typical form of the curve which was eventually given by all *O*s except one. Every one of the *O*s of the first group required considerable practice before his results on different days attained consistency among themselves and agreement with those of the other *O*s. The second group (at Princeton) did not require any practice; the curves given at the first hour they served were of the same kind as that in Fig. 3.

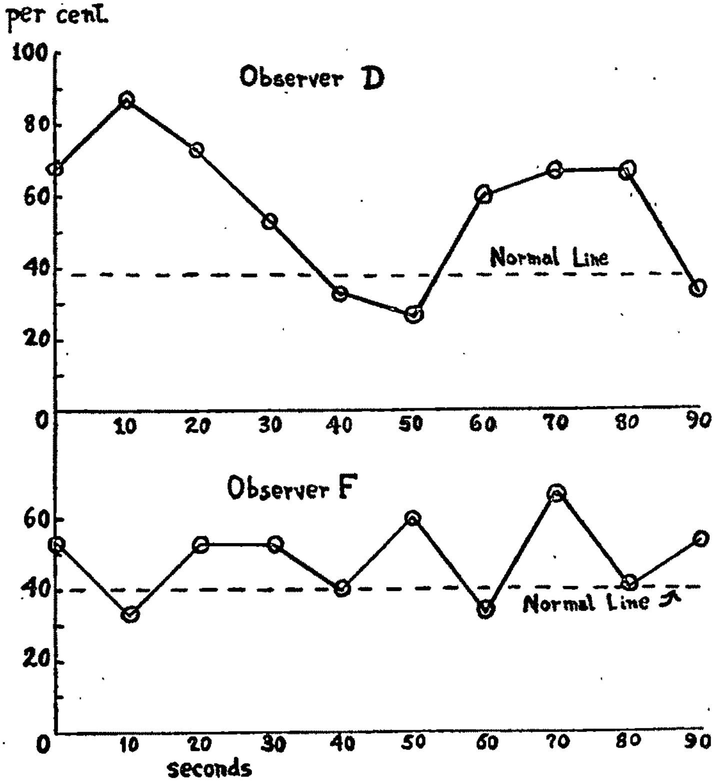

FIG. 4. SAMPLE CURVES OF OBSERVERS *D* AND *F*. Every point on the curves is determined by 15 judgments. *D* finally gave curves like the one in Fig. 3, while *F*'s curves continued to be as irregular as the one here shown.

The *O*s of the first group were: *F*, *A*, graduate students, and *C* and *D* senior students. *F* and *C* were men; *A* and *D* were women. Every *O* worked for about 15 weeks, coming once a week for an experimental period of two hours. *C* and *A* acted as *O*s for about 5 weeks before they began to give the usual form of curve. After they began to give this curve, they continued to do so the rest of the year. The results of the work with *F* and *D* were very irregular. Samples of curves given by them are shown in Fig. 4. Their results showed no consistency except that the curves were usually above the 'normal line.' These two *O*s were not discarded, however, and finally, during the last two experimental periods of the year, *D* gave four curves of the usual form. *F* never gave a curve of the usual form—his remained like those in Fig. 4.

The members of the second group of *O*s were: *G*, professor; *B*, instructor; *K*, *L*, *M*, graduate students; and *H*, *J*, *E*, *I*, senior students—all of the Department of Psychology. These *O*s were employed for varying lengths of time and at rather irregular intervals.

Why the difference as to the need for practice existed between the two groups has never been clear. The apparatus and method, except for the addition of the audiometer in the second year, were the same in both years. With respect to *D*, it seems that the failure to obtain agreement with the other *O*s for so long a time might be due to her lack of interest in the experimental work, which she may have regarded merely as means of obtaining college credit. This explanation does not hold for other members of that group of *O*s, and no adequate explanation (in terms of conditions lying outside the *O*s themselves) has been found.

RESULTS

The results of the experiments are recorded in Tables I to IV, inclusive. A study of the Tables yields the following facts concerning the fatigue-effect:

(1) *The degree of fatigue.* The maximum amount or degree of fatigue was found by subtracting the percentage of 'greater' judgments in the T_l-T_r comparisons before fatigue was established (represented in the tabulated data as the 'normal line') from the greatest percentage of 'greater' judgments which occurs after the cessation of the fatiguing tone. This method can be applied only to those series where the maximum percentage after fatigue is less than 100; for it is obvious that, once a sound is judged greater in 100% of the cases, its intensity may then be increased indefinitely and the percentage of 'greater' judgments will not reflect this change in intensity.

The subtraction mentioned was made in 53 series of results in Tables I, II, and III where the fatigue was produced by stimulation for 1 min. with a fairly loud tone (about 100 on the oscillator dial and 75-105 sensation units on the audiometer). The arithmetic mean of the increase in 'greater' judgments on the unfatigued side was found to be 57%; the median was 60%, with a mean deviation of 16%; *i.e.* the decrease in intensity of a tone due to fatigue under these conditions is, on the average, somewhat more than would be necessary to constitute a difference limen under usual conditions.

(2) *The duration of fatigue.* The method employed in these experiments will furnish only an approximate result with respect

TABLE I. THE NON-SPECIFICITY OF FATIGUE

In this and following Tables, L and R, under the column headed 'Ear,' stand for 'left' and 'right;' in other places, G, E, and L stand for 'greater,' 'equal,' and 'less,' respectively. Pitch is given in d.v. Where the oscillator was used, the numbers given under the column headed 'Energy' refer to readings on the potentiometer dial, and such readings for T_l and T_r are omitted because of the oscillator's lack of energy-calibration. Where the audiometer was used, the numbers under the 'Energy' column refer to sensation-units and are italicized. For further explanation of the method of tabulation, see the text referring to Fig. 3 and the tabulation under Fig. 3.

			F			T_l-T_r				Percentage of judgments 'greater on unfatigued side' at							
No.	*O*	Ear	Pitch	Energy	Duration	Pitch	Energy	Normal Line at	No. Judgments	0	5	10	20	30	40	50	60 sec
1	*A*	L	627	100	1′	512		20	15	100	—	67	20	0			
2	*B*	L	256	*85*	1′	2048	*70*	14	15	100	27	27	13	0	7		
3	*B*	R	256	*85*	1′	2048	*70*	8	15	60	53	47	20	7	7		
4	*B*	L	1024	*80*	1′	256	*70*	24	20	95	45	30	25	5			
5	*E*	L	512	100	1′	4500		20	15	73	67	47	53	27	13	13	
6	*E*	R	1024	*90*	1′	256	*55*	34	15	93	—	93	73	40	33	47	
7	*E*	L	512	100	1′	1330		34	16	94	69	81	69	69	31	13	
8	*E*	L	256	*85*	20″	2048	*50*	12	15	60	27	13	0	0			
9	*J*	L	512	100	1′	4500		10	15	93	87	73	53	33	40		
10	*J*	R	256	*85*	1′	1024	*70*	30	16	100	94	81	56	56	50		
11	*J*	L	1024	*95*	1′	2048	*80*	42	20	65	50	50	45	15			
12	*L*	R	4096	*80*	1′	256	*85*	38	15	87	93	73	47	40	40		
13	*M*	L	256	*65*	1′	1024	*70*	28	15	47	40	20	7	20	7		
14	*N*	L	512	100	1′	1330		22	15	100	100	100	87	67	60	40	27
15	*B*	L	512	100	1′	512		12	15	80	—	73	53	20	33	13	
			550							27	—	40	7	20	13	13	
16	*B*	L	512	100	1′	512		18	15	40	—	53	27	20	7	20	
			550							80	—	40	27	13	7	7	
17	*B*	L	512	100	1′	512		18	15	93	—	80	73	33	40	20	
			550							87	—	60	47	33	20	33	
18	*B*	L	512	*85*	1′	512	*70*	16	15	80	53	27	13	27	13		
			1024							67	53	27	20	13	7		
19	*C*	R	512	100	1′	512		20	15	93	—	80	33	0	0		
			627							100	—	93	13	0	0		
20	*C*	R	512	100	1′	512		38	15	100	—	80	53	0	0		
			627							33	—	0	0	0	0		
21	*C*	L	512	100	1′	512		13	15	100	—	60	0	0	0		
			627							93	—	60	13	0	0		
22	*C*	L	512	100	1′	512		14	15	100	—	80	87	47	0		
			1330							93	—	53	20	0	0		
23	*K*	L	512	100	1′	512		0	15	93	87	40	27	27	7	13	
			550							73	87	87	40	27	27	27	
24	*K*	L	512	100	1′	512		0	15	60	—	47	53	40	20	0	
			550							100	—	60	20	20	7	7	
25	*K*	R	256	*85*	1′	2048	*70*	0	15	40	33	27	27	20	20		

to the duration of fatigue, since one point on a fatigue curve (or one value in the series tabulated) is determined by only 15 or 20 judgments, and these judgments were taken at intervals of 10 sec.

The duration was measured by taking the time required, after the cessation of the fatiguing stimulus, for the fatigue curve to reach the 'normal line,' *i.e.* the recovery-time. This measurement was made in the series in Tables I, II, and III where the fatigue was produced by stimulation for 1 min. with a tone of an energy equal to 75-105 sensation units on the audiometer (or a tone of 512 v. at intensity 100 on the oscillator).

The time of recovery in 51 series of this sort ranged from 10 to 60 sec. The median time was 33 sec.; the mean was 30 sec., with a mean deviation of 10 sec.

TABLE II. THE EFFECT OF THE ENERGY OF THE FATIGUING STIMULUS
(See legend of Table I)

			F			Tl-Tr				Percentage of judgments 'greater on unfatigued side' at								
No.	*O*	Ear	Pitch	Energy	Duration	Pitch	Energy	Normal line at	No judgments	0	5	10	20	30	40	50	60	70 sec
1	*B*	L	512	100	1′	512		16	15	73	73	67	60	33	27	33		
				20						60	60	40	40	40	47	40		
2	*B*	L	512	90	1′	512		24	15	47	—	53	47	7	7	13		
				30						27	—	20	0	13	20	13		
3	*B*	L	512	100	1′	512		20	15	93	67	47	40	27	20	7		
				20						27	40	7	7	27	13	7		
4	*B*	L	1024	*90*	30″	1024	*75*	10	20	80	30	25	10	0				
				70						65	25	0	0	5				
5	*B*	L	1024	*95*	30″	1024	*70*	4	20	75	30	20	15	0				
				70						75	50	15	10	20				
				60						35	10	5	5	0				
6	*C*	L	512	100	1′	512		35	15	100	—	93	100	67	47	7	0	0
				20						33	—	40	20	20	7	0	7	0
7	*C*	L	512	100	1′	512		20	15	100	—	100	93	87	60	27	13	7
				20						73	—	80	47	27	13	0	0	0
8	*C*	L	1330	100	1′	512		14	15	93	—	53	20	0	0			
				20						33	—	27	7	13	0			
9	*C*	L	512	100	1′	512		36	15	100	—	93	93	87	67	27	7	0
				20						93	—	80	60	27	13	7	7	0
10	*D*	L	512	100	1′	512		10	15	67	—	40	20	7	7	27	7	27
				20						20	—	13	13	13	20	13	27	20
11	*I*	L	1024	*90*	30″	1024	*70*	*	15	G 67	53	27	7	— no 'less' judgments —				
				70						G 73	40	27	0	— only 2 'lesses' —				
				60					20	E 60	60	55	50	— no 'greater' judgments —				
12	*I*	L	512	100	1′	512		18	15	100	93	93	60	33	33	40		
				5						53	47	33	27	33	27	27		
13	*K*	L	512	100	1′	512		20	15	80	60	60	7	0	0	0		
				20						20	20	7	7	7	0	7		
14	*K*	L	1024	*105*	1′	1024		0	20	95	25	5	0	0				
				75						70	30	0	0	0				
15	*M*	L	512	100	1′	512		58	15	93	60	40	27	27	27	33		
				10						40	20	20	27	20	20	27		

* 36% 'equal,' 64% 'less.' Tl—Tr given in order left-right.

In 9 series in these Tables the 'normal line' was not reached within the 40- or 50-sec. limit of the series, and they were not considered in the above calculation on account of their indeterminateness.

Significant individual differences in the duration of fatigue do not appear. Twelve series of *B*, 12 of *C*, and 6 of *K* gave mean durations of 28, 34, and 33 sec. respectively. The mean deviations were 7, 10.5, and 11 sec., respectively.

(3) *The non-specificity of fatigue.* The results exhibited in Table I show, without exception, that fatigue produced by a

tone of a given frequency affects the intensity of all other tones used in the experiments. The tones used as fatiguing stimuli were 256, 512, 550, 627, 1024, 1330, and 4096 v. The comparison stimuli were 256, 512, 1024, 1330, 2048, and 4500 v. The series obtained when fatigue is produced by a tone of a pitch different from that of the comparison stimuli show, when plotted as curves, the same form as those curves obtained when fatiguing and comparison stimuli are of the same pitch.

Ten sets of results, Nos. 15 to 24, inclusive, embrace two lines of figures. These lines represent 2 series which were obtained in the same experimental hour for the purpose of comparing the degree of fatigue set up by a tone of 512 v. with that set up by some other tone. The comparison stimuli were in all cases 512 v. These data were all obtained by the method of alternation previously described.

In these 10 series, the effect of the other tone is approximately equal to that of 512 in 5 (17, 18, 19, 21, 23), less than that of 512 in 3 (15, 20, 22), and more than that of 512 in 2 (16, 24).

From these double sets of results and from the other series in Table I, the conclusion may be drawn that any of the tones used is as effective a cause of fatigue as any other one used in this investigation.

This finding does not cast any light upon a theory of hearing, since the seat of the fatigue is yet unknown.[19] The fact of nonspecificity merely eliminates the statement, formerly accepted as a fact supporting the resonance theory, that the fatigue is confined to the pitch producing it.

(4) *The dependence of fatigue upon the energy of the stimulus.* In Table II 15 sets of results are given, wherein the effect of a loud sound is compared (in the same hour) with that of the same sound when diminished in energy.

Without exception these results show that less fatigue is produced by a weak tone than by a strong one, *i.e.* the fatigue is less and recovery from it is quicker.[20] A considerable difference in energy is required if a difference in fatigue-effect is to be shown, *e.g.* in Nos. 5 and 11 the tone 1024 v. produces the same result at 90 or 95 sensation units as it does at 70, but at 50 sensation units a markedly less effect is shown.

(5) *The dependence of fatigue upon the duration of the stimulus.* In Table III are gathered 18 sets of results in which the effect of increased duration of the fatiguing tone is tested. Here the results are not very clear cut. In 5 sets the effect of a 1-min. period is compared with that of a 2-min. period. In 4

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[19] It is unlikely that the *tensor tympani* is concerned in the fatigue, since its reflex contraction is consensual. Cf. W. Köbler, *Zsch. f. Psychol.*, 54, 1909, 250.

[20] Nos. 1, 10, 11 and 12 form an exception to this last clause, since the values are very rregular at the ends of the series.

TABLE III. THE EFFECT OF THE DURATION OF THE FATIGUING STIMULUS

(See legend of Table I)

No.	O	Ear	F			T_l-T_r		Normal Line at	No. judgments		Percentage of judgments 'greater on unfatigued side' at											
			Pitch	Energy	Duration	Pitch	Energy				0	5	10	20	30	40	50	60	70	80	90	
	A	L	512	100	2′	512		17	12		100	—	100	83	58	50	33	25	25	17	17sec.	
					1′						100	—	75	58	8	8	17	0	0	0	8	
2	*A*	L	512	100	2′	512		12	12		100	—	100	75	58	50	50	25	17	17	17	
					1′						100	—	75	58	17	0	8	8	8	0	0	
3	*C*	L	512	90	2′	512		21	15		93	—	87	73	60	47	7	0	0			
					1′						73	—	67	27	7	0	0	0	0			
4	*C*	L	512	100	2′	512		12	15		93	—	67	20	0	0	7	0	0			
					1′						93	—	80	33	0	0	0	0	0			
5	*K*	L	512	100	2′	512		14	15		87	—	80	47	27	20	7					
					1′						60	—	47	33	27	33	40					
6	*H*	L	512	100	1′	512		7	15		87	67	47	13	7	0						
					30″						87	73	53	27	7	27						
7	*J*	R	256	*85*	1′	1024	*70*	30	16		100	93	81	56	56	50						
					30″						88	75	56	38	19	50						
8	*L*	L	512	100	1′	512		34	15		100	87	73	67	47	67	40					
					30″						100	87	80	73	80	67	73					
9	*B*	R	256	*85*	1′	2048	*70*	8	15		60	53	47	20	7	7						
					20″						60	67	73	7	13	7						
10	*B*	L	1024	*80*	1′	256	*70*	24	20		95	45	30	25	5							
					20″						85	50	35	15	20							
11	*B*	L	1024	*70*	1′	1024	*70*	0*	15		73	40	20	0	20							* All equal.
					20″						87	80	20	7	0							
12	*E*	R	1024	*90*	1′	256	*55*	34	20		70	70	55	30	25	35						
					20″						50	55	40	25	25	20						
13	*E*	L	256	*85*	1′	2048	*50*	12	15		47	33	27	7	0							
					20″						60	27	13	0	0							
14	*H*	R	1024	*85*	1′	1024	*75*	0*	15	L	60	33	27	13	0							* 12% less. No 'greaters' in the whole experiment. Comparison stimuli given in order left-right.
					20″					L	33	7	7	0	0							
15	*I*	L	1024	*100*	1′	1024	*70*	0*	15		60	40	7	0	0							* 22% equal, 78% less. No 'lesses' given except in judgments before fatigue.
					20″						27	13	7	0	0							
16	*J*	L	1024	*95*	1′	2048	*80*	42	20		65	50	50	45	15							
					20″						60	35	40	20	40							
17	*K*	L	2048	*80*	1′	2048	*80*	0*	20		90	35	5	0	0							* All equal.
					20″						70	15	5	0	0							
18	*L*	R	2048	*85*	1′	256	*70*	88	15	G	13	20	67	93	93	87						Comparison stimuli given in order left-right, right judged in 88% of cases greater. Right ear fatigued.
					20″					L	60	27	0	0	0	0						
										G	27	53	87	87	80	87						
										L	27	7	0	0	0	0						

instances stimulation for 2-min. gives a greater effect than for 1 min., while in the remaining series the effect of the shorter period is very slightly greater.

Of the 10 sets of series comparing the effects of 1-min. and 20-sec. stimulation, the longer period gives a greater effect in only 6 sets, in one set (No. 10) the effect is practically the same in both, and in Nos. 11, 13, and 9 (a very irregular curve) the 20-sec. stimulation gives more fatigue.

The remaining 3 sets of data deal with periods of 1 min. and of 30 sec. In Nos. 6 and 8 the effect is greater for the shorter period of stimulation, but longer stimulation gives greater fatigue in No. 7.

To summarize: the longer period gives the greater fatigue in 11 cases, less fatigue in 6, and no significant difference in 1 case.

If, however, we consider the results obtained immediately after the cessation of the fatiguing tone (entered in the column headed '0 sec.'), we find the shorter period producing the greater fatigue in only Nos. 11 and 13. The value under '0 sec.' is undoubtedly the most reliable point on the curves, since the *O*s said that the judgments then were easiest.

From the facts given above, it may be concluded that when periods of stimulation shorter than 2 min. are used, a period of 1 min. usually produces less fatigue than one of 2 min., and a period of 20 or 30 sec. usually produces less than one of 1 min.

(6) *An apparent sensitization-effect.* In Table IV are given 5 series from 3 *O*s which seem to indicate that occasionally a very weak tone, when used as a stimulus for 1 min., has an effect opposite to fatigue, *i.e.* its effect is to make subsequent tones of greater rather than of less intensity.

No. 1 in the Table shows in the first line the usual fatigue produced by a loud tone. The only judgments made in this part of the experiment were 'greater' and 'equal.' The next three lines show the judgments after the left ear had been stimulated with the same tone at a much lower level of energy. The result of the weak stimulation was to eliminate the 'greater' judgments entirely. The *O* (Professor Langfeld), well trained in such observation, reported 'less' and 'equal' only—never 'greater.' In the first members of the series, the 'less' judgments predominated, but they gradually diminished as time passed and gave way to increasing numbers of 'equals.'

No. 2 is of the same type as No. 1, but not so clear cut.

Nos. 3 and 4 are cases where the only effect of the weak tone was to depress the percentage of 'greater' judgments below the 'normal line' instead of raising it above in accordance with the usual effect. In No. 5, with the same *O*, 'less' judgments occurred for the first time in the experimental hour after the ear had been subjected to weak stimulation.

The writer is restrained, however; from concluding that the effect of weak stimulation is to sensitize the auditory mechanism, because his efforts to produce the sensitization-effect subsequent to its first appearance have in practically all cases failed. He has tried many times to produce the effect, but the result has been slight fatigue; the appearance of the sensitiza-

TABLE IV. AN APPARENT SENSITIZATION-EFFECT

(See legend of Table I)

No.	*O*	Ear	F			T_l-T_r Pitch	Normal Line at	No. judgments		Percentage of judgments at									
			Pitch	Energy	Duration					0	5	10	20	30	40	50	60	70 sec.	
1	*G*	L	512	100	1′	512	20	15	G	100	73	73	47	7	0	0			No 'lesses.'
				20					G	0	0	0	0	0	0	0			
									L	93	80	73	13	20	0	7			
									E	7	20	27	87	80	100	93			
2	*L*	L	512	100	1′	512	42	15	G	93	60	47	53	60	47	53			
									L	0	7	20	7	27	20	20			
									E	7	33	33	40	13	33	27			
				20					G	13	13	20	27	53	73	47			
									L	60	33	20	20	0	13	7			
									E	27	54	60	53	47	14	47			
3	*A*	L	512	100	1′	512	60	15	G	100	—	93	67	13	7	7	0	0	No 'lesses.'
				20					G	7	—	0	7	0	0	0	0	0	
4	*A*	L	512	100	1′	512	26	15	G	100	—	100	93	73	33	27	13	7	—do.—
				20					G	0	—	0	0	7	0	7	0	7	
5	*A*	L	512	100	1′	512	16	13	G	61	—	13	0	0	0	0	0	0	No 'lesses.'
				50					L	62	—	46	31	15	31	46	46	46	No 'greaters.'

tion-effect has been on rare and unexpected occasions, when he was not trying to produce it. It is this feature of the effect which prevents him from drawing a conclusion, and not any suspicion of the actual data, which were obtained throughout by the method of alternation and with as much care as any of the other data in this paper.

VARIATIONS OF THE FUNDAMENTAL METHOD

One of the objects of the investigation was to vary the conditions of the experiment in such a way as to insure, so far as possible, that the fatigue-phenomenon was of physiological, and not of psychological, nature.[21] As an example of a phenomenon of psychological origin, we may consider one of the facts reported by Sewall: if, after giving a fatiguing tone on the left side, he now threw in the tone on the right (without cutting off the left tone), the tone on the right seemed much more intense. He attributed this effect to an attentional factor; the tone on the right began as something new and changing, attracting attention to itself, and was therefore judged to be of greater intensity. The 'psychological' nature of the effect was shown by the fact that if he interrupted the fatiguing tone for one or two seconds before presenting the two tones simultaneously, the 'fatigue' effect vanished completely.

It might be objected that similar factors were at work in this experiment; after the fatiguing tone had been heard for a minute and was then cut off, the tone on the opposite side beginning immediately thereafter may, by virtue of its novelty, possess more clearness than the fatiguing tone. The relative durations of the tones may play a part also in conditioning judgments of intensity; the longer tone, being monotonous, may lose clearness. Of two tones differing only in clearness, the clearer might be judged the more intense, since (1) clearness and intensity are, to a certain extent, covariants and (2) the *O*s employed may not have been sufficiently trained to abstract from clearness in making their judgments.

The usual method of investigation was, therefore, varied in order to eliminate as far as possible any psychological influences which might be giving a false appearance of fatigue and in order to see if the fatigue-effect would disappear in other experimental settings.

The methods used were four:

(1) When F_l was the same tone as T_l the method was: E_l was stimulated with F_l. Simultaneously with the cutting off of F_l, without any appreciable interval, T_r was given in E_r. The *O* was instructed to compare the intensity of that part of F_l heard after the experimenter's ready-signal (given about 2 sec. before the end of F_l) with the intensity of T_r.

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[21] Cf. footnote 3.

This procedure was used with *A*, *C*, *D*, and *F* whenever the fatiguing tone was the same as the comparison tone on the same side.

(2) This method was the same as (1) except that between F_l ($=T_l$) and T_r an interval of about 1 sec. intervened. This plan was followed in order to eliminate the shock, so to speak, of the very quick change from F_l in one ear to T_r in the other, a change which might be accompanied by an increase in clearness of the experience as the second tone comes suddenly into consciousness.

This method was used with *B*, *E*, *G*, *H*, *I*, *J*, *K*, *L*, *M* whenever F_l and T_l were the same.

(3) This method was employed when F_l and T_l were different in intensity or pitch or both. After F_l had been heard in E_l during the fatiguing period, it was cut off, and after 1 sec. T_l was given in E_l; then, after another pause of 1 sec., T_r was given in E_r.

This procedure was followed with all *O*s whenever the tones F_l and T_l were not the same.

In (2) and (3) both of the short tones T_l and T_r appeared as new stimuli with respect to the long fatiguing stimulus and were separated from each other and from the fatiguing stimulus by short intervals. It seems that, if clearness accrues to short sensations because of their novelty, then both T_l and T_r should receive equal increments. If there should be an unequal distribution of clearness due to the temporal order of the short tones, then the order should be changed to eliminate this factor. This change was made in Method 4.

(4) In this method, the usual order of presentation of T_l and T_r was reversed, and the stimuli were given in the order T_r-T_l. This method was used in the following series: Table I, Nos. 6 and 25; Table II, No. 13; Table III, Nos. 6 and 8; Table IV, No. 1.

It is apparent from an inspection of the series obtained by means of these different methods that the fatigue-effect appears in all of them and that no significant differences are shown by the results.

Since the first work with most of the *O*s consisted in obtaining the fatigue-effect when the fatiguing stimulus was of the same pitch as the comparison tones T_l and T_r, it might be objected that the *O*s acquired a 'set' or bias from this earlier work which led them to give the same results when the fatiguing tone and the comparison tones were of different pitches. This objection is met by Nos. 5 and 9 of Table I (transfer of fatigue produced by 512 v. to the intensity of tones of 4500 v.), which were obtained on the first occasion that each of these *O*s served.

The persistence of the fatigue-effect through all these changes of conditions affords ground for considering it to be of physiological not psychological nature. The facts, too, that there are no significant individual differences among the *O*s and that the results of any one *O* show consistency among themselves, afford further ground for believing that the phenomenon is conditioned by physiological changes caused by prolonged stimulation and is not due to a distortion of judgment produced by attentional factors, psychological contrast, or the like; for in the latter case a greater variability and instability of the fatigue-effect would not unreasonably be expected. Of course, no absolute proof of the physiological nature of the effect can be given from the facts here set forth; the effect can only be shown to occur persistently in a variety of experimental settings.

SUMMARY

(1) The result of prolonged stimulation of the auditory mechanism is a diminution of the intensity of the stimulating tone and of tones subsequently heard. This effect is called fatigue in this paper.

(2) This decrease in intensity amounts to somewhat more than one difference limen, when the stimulating tone is 1 min. in duration and of the energy corresponding to 75-100 sensation units of the Western Electric audiometer.

(3) The duration of this fatigue-effect is about 30 sec. under the conditions of paragraph (2) above.

(4) The fatigue is non-specific, *i.e.* the intensity of tones of pitch different from the stimulating tone is decreased by the latter.

(5) The degree of fatigue produced by a weak tone is less than that produced by a strong tone.

(6) The amount of fatigue produced by stimulation for 2 min. is usually more than that produced by stimulation for 1 min. Stimulation for 1 min. usually produces a greater amount of fatigue than stimulation for 20 or 30 sec.

(7) The fatigue-effect persists when the experimental setting is varied in such a way as to eliminate, so far as is possible, attentional or other 'psychological' factors.

AN EXPERIMENTAL STUDY OF THE DIFFERENTIATION OF TEMPERAMENTS ON A BASIS OF RATE AND STRENGTH[1]

By Mildred Frances Baxter

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I. Introduction

As Allport has said in regard to the classification of temperaments, "the four-humor doctrine has haunted the house of psychology for twenty-five centuries."[2] During recent years a new version of this four-fold classification has been put forth. Bahnsen,[3] Wundt,[4] Ribéry,[5] and others have suggested that the four temperaments are a quick-strong, a quick-weak, a slow-strong and a slow-weak type. This classification has repeatedly found a place in text-books as a possible basis for the grouping of individuals with respect to temperament. We have in this research endeavored to determine whether or not this system of differentiation is justified experimentally.

First we shall try to make clear what is meant by our terms quickness or rate and strength. Writers have not agreed on their significance. Bahnsen[6] refers to quick or slow receptivity and to strong or weak spontaneity, Wundt[7] to the quickness and strength of the emotions, and Ribéry[8] to strong and quick motor and sensory reaction. The terms quickness and rate, as we have used them, require little explanation. The time required for the performance of a task has been taken as the speed index. The situation is very much more difficult when we attempt to determine what has been meant by the term strength and how it shall be defined for the present purpose. There seems to have been no uniformity, and it has been applied to various traits. We learn from Stern[9] that Dorsch speaks of strong or weak bodies and minds, Herbart of strong or weak excitability and manifestation of the emotions; that Hellwig uses these terms in connection with reception and reaction, Kreibig with respect to the will, Klages with regard to motive power and resistance, and Ach with reference to the determining tendency and sensory-motor excitability. According to the interpretation that we have put on the term, strength is that element in efficiency which remains when the speed or rate factor in a performance is disregarded; *i.e.* quality or excellence.

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[1] From the Psychological Laboratory of the University of Michigan.

[2] G. W. Allport, Personality and character, *Psychol. Bull.*, 18, 1921, 445.

[3] W. Stern, *Die differentielle Psychologie in ihren methodischen Grundlagen*, 1911, 482.

[4] W. Wundt, *Grundzüge der physiologischen Psychologie*, III, 1903, 638.

[5] W. Stern, *op. cit.*, 484.

[6] *Loc. cit.* [7] *Loc. cit.* [8] *Loc. cit.*

Our aim, then, has been to ascertain whether there appear to be four types of individuals based on combinations of the rate and strength factors, and, in case they do seem to exist, whether they correspond to the types of temperament indicated by descriptions secured through questionaries.

II. Experimental Procedure

A. General

In attacking the problem we first secured 3 sets of scores or measures for both rate and strength. We then endeavored to determine whether any interrelation exists. The first set of scores was obtained from the results of a series of experiments which were selected with a view to including as great a variety of performances as possible. The second set of measures were physiological in character, and included pulse rate as a speed score, systolic blood pressure as a strength score, and temperature. The third set of scores was secured through a group of questionaries. These, in addition to judgments of the rate and efficiency of an *S* in performance, also yielded scores for the speed with which emotions were aroused and for their intensity, of the tendency to be cheerful or to be depressed, for the degree of activity in which *S* usually indulged, and for the tendency to be steady or to fluctuate.

The 138 young women who served as *S*s in this research were university students who were at the time enrolled in the elementary course in psychology. No one of them was acquainted with the purpose of the investigation. Two groups are considered. Group I, composed of 90 *S*s, was used for trying out methods and standardizing materials and procedure, preliminary to the selection of experiments to be used in gathering the final data from Group II. The coefficients of reliability, except for the tapping and carbon-copy experiments, and the physiological measures were computed from data obtained from this group. Group II comprised 48 *Ss*, from whom 44 complete sets of data were obtained. A set was considered complete when scores were secured for every experiment, where no more than one of the six questionaries had to be omitted (there were two cases of one omission), and where the physiological measures were secured for at least one morning and one afternoon period. The age range for the members of Group II, with the exception of one *S* aged about 40, was 16 to 25 yrs.; the mode was 19.

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[9] *Op. cit.*, 481 ff.

The work with Group I extended over a period of 2 years, though that with any one individual was usually completed within 2 weeks. Group II worked for six one-hour periods between March 25 and June 2, 1923. The first and third periods were morning hours, the second and fourth afternoon hours; the last two came in the evening. The first two periods were arranged so that an interval of 28 hr. intervened. It was found to be impossible to carry out this plan quite as rigidly for the third and fourth hours. In 9 of the 44 cases from 2-5 days elapsed between these periods, and in one case 9 days. The evening hours came exactly one week apart.

With Group II the experiments were conducted individually, except for part C of the carbon-copy experiment and for the filling out of questionaries C and D, where the work was done in groups of from nine to fifteen. The procedure was identical for all *Ss*, the order for the first four periods being: (1) experiments in a fixed sequence, (2) physiological measurements, (3) questionary B. Four of the experiments were divided, for the purpose of obtaining sets of scores from which coefficients of reliability might be computed, into two parts which were given on different days. The parts of the carbon-copy, clock problem, and maze experiments were given during the first two periods, those of the tapping experiment during the second and third hours. Where an experiment yields more than one score we shall describe the apparatus or material and general procedure when the experiment is first referred to; the method of obtaining other scores will be treated later.

B. EXPERIMENTS

(1) *For Rate Measures.* These experiments vary in the degree to which speed was required, ranging from tapping and the learning of nonsense syllables in as few trials as possible, where speed was the only element of interest, through experiments such as the cancellation of words, where the effort was to be divided between speed and accuracy, to those where the rate was purely voluntary. Another aspect in which they are different is the amount of motor activity required in their performance and the degree of mental organization involved. We have attempted to consider these factors in listing and grouping the experiments, placing the emphasis on the first in cases where the two conflicted.

(R-1) Tapping Experiment. Apparatus. We used a tapping board in the form of an oval brass plate mounted on a 6 × 4 × ⅞ in. block, and a tapping stylus with flexible connecting wire attached. For recording the number of taps an Ewald's chronoscope[10] was placed in the circuit. Before beginning experiments in March we tested the chronoscope by comparing the number of taps recorded by it with the number simultaneously recorded on a kymograph. Light, heavy jarring, and glancing strokes were all tried. The chronoscope was adjusted until the two records, for all but glancing strokes, agreed.

Procedure: Position and method were demonstrated. *S* sat with right arm (except in the case of one who was left-handed) resting comfortably on the table, elbow touching but wrist free. We were careful to see that the stylus was held erect to avoid glancing strokes and was firmly held so that adjustment would not be necessary during the performance. The instructions were: "Tap as rapidly as possible from the signal 'now' to the signal 'stop' which will be given about one half minute later. Pay attention only to your tapping." The switch was opened and stop-watch started as soon as *S* began to tap, and closed at the end of 30 sec. with the giving of the 'stop' signal. Two trials with a one-minute rest interval were given at the beginning of the second and third hours. The average of the first two was correlated with the average of the second two for an index of reliability, and the average of all four trials used for the score. The coefficient of reliability computed from results from 44 members of Group II was found to be .878 ± .0198.[11]

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[10] Ewald's chronoscope was used without tuning fork as an electric counter.

(*R-2*) *Simple Reaction-Time to Sound. Apparatus.* The Hipp chronoscope, a relay, sound hammer, and telegraph key, were set up according to the Dunlap break-to-break system. The initial adjustment of the chronoscope for absolute time and constancy was made with a control pendulum. While the experiment was in progress checks for accuracy were made, at intervals during each day, with a gravity control apparatus which was always in the circuit.

The average m.v. for the test series of 10 times previous to the beginning of an experiment was never more than .6σ and usually much lower, even as low as .0σ. This m.v. was for a time interval of about .483σ and included any error introduced by the control apparatus.

Procedure. We explained the purpose of this experiment to *S* and made her familiar with the stimulus and mode of reacting before the experiment was begun. The interval between the 'ready' signal and stimulus was 2 sec. Thirty reactions were taken. This number was thought to be sufficient for this and for the association reaction-time experiment, on a basis of preliminary work done with Group I. Eighteen series of 50 reaction-times were secured from this group. When the m.v. was calculated for each of these series and for the first half of each series the coefficient of correlation between the m.v.'s for the long and short series was found to be .941 ± .019. Thirty reactions were taken instead of 25, since 30 was the number used by Woodrow[12] in his experiment for the measurement of attention, which we have included in our series of experiments for securing strength scores. Occasionally there were reactions unquestionably premature, and long ones due to lapses of attention, which it was thought should not be included in the average time. The method of eliminating these errors was to omit arbitrarily all reactions below 100 σ and the three longest in every series. The coefficient of reliability, computed from pairs of scores obtained one week apart from 31 *S*s in Group I, is .589 ± .079.

(*R-3*) *Association Reaction Time Experiment. Apparatus.* An Ach card-exposer apparatus was connected in place of the sound hammer used in the simple reaction-time experiment, and a lip key[13] in place of the telegraph key. A series of 30 familiar words was constructed, including 18 nouns, 6 adjectives, 3 words which might be understood as adjectives or nouns, and 3 which might be nouns or verbs. Only one had as many as 6 letters, and 4 as many as 3 syllables. The words were typed in small letters in the center of white cards 3⅛ × 1⅝ in. Five blank cards were introduced at irregular intervals during the series, as a means of detecting any tendency to release the key at the sight of the stimulus word instead of waiting for the appropriate response.

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[11] The formulae used for computing all coefficients and their probable errors are, unless otherwise stated, the familiar product moments formulae.

[12] H. Woodrow, The measurement of attention, *Psychol. Rev. Mon.*, 17, 1914, No. 5.

[13] This key was found to be less satisfactory than was anticipated for it was extremely difficult to get all *S*s to react in the same manner. Though the instructions were to release the key as the word was spoken, some had a tendency to get the lips away from the key before speaking the word and so were in danger of letting go even before an associate had occurred to them, while others tended to retain their hold on the key while they spoke the word.

Procedure. *S* was instructed to hold the key closed between her lips until the word exposed had suggested another word but to speak this word as soon as it came to mind. The procedure was demonstrated: 2 practice trials in responding with the first associate were given without the apparatus; then a series of 3 trials using 2 cards with words and a blank one between, was given with the apparatus. Whenever a word in the regular series was reacted to prematurely or while the key was kept closed, it was repeated after several others had intervened. The average of the 30 satisfactory reactions was used as the score. To determine the coefficient of reliability, a second series of words agreeing exactly with the first in regard to the number of nouns, etc., and number of letters and syllables, and similar with respect to meaning, was constructed and given to 31 members of Group I one week after they had had the first list. The coefficient was found to be .698 ± .062.

(*R-4*) *Learning of Nonsense Syllables. Apparatus.* A modification of the Wirth exposure apparatus, arranged by Professor Shepard, was used. The syllables of the series, fastened to a revolving drum, were exposed in succession in an opening 1⅛ × 1⅝ in. A series of 8 nonsense syllables was used:[14] two series were constructed.[15]

Procedure. Series I was used with Group II. The syllables were exposed at the rate of one every 2 sec. *S* was required to spell the syllable aloud during the exposure, as Luh suggested, in order to prevent the forming of meaningful associations. Immediately after every exposure of the series *S* wrote all the syllables that she could recall. The exposures were repeated until the series was written correctly twice with no exposure between these recalls. Frequently one recall would be perfect, but errors introduced into the second reproduction of the list would indicate that the learning was not complete. The rate score was the number of exposures required. Series I and II were learned a week apart by 37 members of Group I. The scores obtained gave a coefficient of reliability for the rate of learning of .663 ± .062.

(*R-5*) *Carbon Copy or 'Accuracy of Movement' Experiment. Part B—Speeded Time. Material.* A complete description of the material will be given here, though the chief purpose for which it was arranged was to get a strength score which will be considered later. The time scores, *R-5*, *R-13*, *R-14*, were secured incidentally and served the purpose of hiding from *S* the real purpose of the experiment. Ten sheets of unglazed onionskin paper, 8⅛ × 5⅛ in., and 10 sheets of light weight black carbon paper were alternately arranged, the top carbon covered by a sheet of graph paper, and the whole set slipped into a white cardboard frame. The right-hand side of the frame was left open when working with individuals, where *E* could prevent handling by *S*; for group work the pads were sealed along that edge also. The graph paper was ruled in light blue, 6 squares to the inch. A black ink margin was drawn just outside the overlapping edge of the frame, and the area within it was divided into sections, one each for *S*'s name, the date and title of the experiment, and three parts, A, B, and C, of the experiment. In each of the last 3 sections three sets of squares, 25 in a row and 2 in height, with a row between sets, were outlined in red. Everything possible was done to keep the apparent but false purpose of the experiment, *i.e.* the measurement of accuracy of movement, before *S*. Uniform No. 3 pencils, kept strictly for this experiment and sharpened after each using, were used by all *S*s in every part of the experiment. In no case so far as is known was the real purpose suspected.

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[14] Originally the lists consisted of 12 syllables, but the time required to learn this number was prohibitive.

[15] The series were constructed according to the following plan:

(1) Every syllable was composed of an initial consonant, a vowel, and a final consonant.

(2) No consonant was used twice as the initial or final letter in the same series.

(3) The initial or final consonants of consecutive syllables were not adjacent letters in the alphabet.

(4) No vowel was used more than twice in the same series.

(5) No vowel was used more than once with the same consonant in any of the 16 syllables.

(6) No given vowel followed another given vowel in successive syllables more than once.

(7) No syllable was used that was found to have meaning according to English, Latin, French, or German dictionaries.

(8) Consecutive syllables did not form polysyllabic words.

Rules 1, 2, 6, and 8 are Müller and Schumann's as quoted by E. Meumann (*Psychology of Learning*, trans. by J. W. Baird, 1913, 365 f.); 7 was suggested by C. W. Luh (The conditions of retention, *Psychol. Rev., Mon.*, 31, 1922, No. 142, 3); the others are believed to be new.

Procedure. The instructions for Part A, *i.e.* for the voluntary time experiments, are given here, since those for part B depend on them. They were: "The purpose of this experiment is to determine how exactly you can control your hand movements. In Part A you are to make three lines of small and large loops, a loop between every pair of blue lines. The small loop is to be one space high, the large loop two spaces high. The line connecting the loops should just touch the lower red line. Be sure to touch the boundaries but do not go beyond them. Begin the first line with a small loop; it will end with a small one, so begin the second line with a large one, and then the third with a small loop. Take your own time; speed does not count." A pattern set of loops was shown to avoid any error. The instructions for Part B, which was given as soon as *S*'s hand was rested, were: "In Part B do exactly the same thing, except that this time do it just as quickly as you can. This time both accuracy and speed count equally. Ready—go." The stop-watch was started with the 'go' signal and stopped as *S* completed the final loop. This time is the score used for speed experiment *R-5*. Group II went through this experiment twice with a 28-hour interval between performances; *i.e.* it was given them during the first and second of their periods. The results for both performances were summed for the scores in all parts of the carbon copy experiment. The coefficient of reliability, computed from the two sets of scores for part B, was .897 ± .0199.

(*R-6*) *Maze Experiments.* It was thought that the kind of performance required in this experiment might bring out differences among the *S*s in their tendency to plunge ahead, sacrificing all else for speed, or to exercise caution.

Material: Two rather simple forms of maze, quite similar to those used with a very different procedure in learning experiments conducted in this laboratory, were employed. Form I was given during the first period and Form II on the second day. The times for the two were averaged for the final score.

Procedure: The instructions were: "I shall give you a maze similar to this one. [A third form was used for demonstration.] When I say 'go, start with your pencil at A (at the point of the arrow) and find your way through; you will come out at B. Keep your pencil on the paper from start to finish. Avoid such blind alleys as this. If you start to go into one I shall say 'no;' when I say 'no,' come out immediately and choose another path. Find your way through as *quickly* and *carefully* as you can. The time you take and the number of wrong turnings count equally." The maze was placed and kept squarely in front of *S*. The stop-watch was started with the 'go' signal and stopped when *S*'s pencil reached B. *S* was required to go through the maze until two successive perfect tracings were made. The time was recorded for all trials, though the first two on each day were the only ones included in the final score, which was an average of these 4 times. The practice-effect, where many trials were necessary, cut the time to such an extent that the average time in those cases was not comparable with the average time of an *S* whose first two trials were without error. The time-scores for one performance with each of the two forms (one performance = 2 trials) by 80 *S*s from Groups I and II gave a coefficient of reliability of .629 ± .046; the coefficient of reliability, obtained from 31 sets of scores from Group I, where the scores correlated were the total time for the first performance of Forms I and II and the total time of a second performance, a week later, of both forms, was .808 ± .042.

(*R-7*) *Münsterberg Experiment.* This experiment is a repetition, as nearly as could be planned from the descriptions available,[16] of the one which Münsterberg employed in testing the efficiency of electric railway motormen.

Apparatus. A frame held a pack of 12 cards which represented streets. Each of these had lines through the center for car tracks and was scattered with black and red digits which stood for moving objects in the road. A belt, having an opening representing a motorman's window, revolved about the frame parallel to the tracks.

Procedure. *S* was informed regarding the assumed movements of the digits on the cards and was told to see how rapidly she could pass the belt over the card and still detect, through the exposure window, all sections of the track endangered by the moving objects. The space between the tracks was divided into 26 sections, each of which was lettered. In case a moving object and the car reached any section simultaneously its letter was to be called. *S* was warned that accuracy and speed counted equally and was given practice in securing these. *E* slipped each card from under the belt as soon as it had been exposed. The time was taken from the exposure of row A on the first card to the covering of row Z on the last card. This time-score for 30 of Group I for two trials a week apart gave a reliability coefficient of .882 ± .027.

(*R-8*) *Cancellation Experiment. Material.* The passages used for this and other experiments to be mentioned were adapted from a small volume by Dunbar.[17] All three selections as arranged contained 19 lines and 212 words. The *Antwerp*[18] and *Holy Week*[19] passages, which were used for the cancellation, have each 95 words to be cancelled. Those entitled *Caterina* and *Antwerp*, and used for the recall of ideas in the test for logical memory, were each divided arbitrarily into 100 'ideas.' To determine the comparative difficulty, for cancelling, of the first pair of selections, they were given on the same day to 27 members of Group I and again a week later to 23 of these. The resulting data indicated that the two selections were of approximately equal difficulty with respect to the time required and the number of errors made. The *Caterina* and *Antwerp* passages were tested in much the same way and accepted as about equal for reading time and difficulty in the recall of ideas.

Procedure. *S* was required to read the selection aloud, word by word, not by phrases, and cancel every word as she came to it, that had an 'a' or an 'e' or both those letters in it providing there was no 'i' or 'o' in the word. The sense of the passage was to be disregarded. Speed and accuracy were emphasized equally. The purpose of the oral reading was to rule out as far as possible the tendency to reread any parts of the selections which were to be read later for recall. The plan was to use the cancellation performance with the *Holy Week* passage as a control for the cancellation with *Antwerp* where the added task of attending to the sense for the purpose of recall was to be the only changed factor in the experiment. The time required to complete the task was the score used. The results obtained by giving the *Holy Week* selection twice, a week apart, to 33 members of Group I give a coefficient of reliability of .883 ± .026 for this time. When the time-scores of the first of the two above sets were correlated with the time-scores secured on the same day with the *Antwerp* passage the coefficient is equally high, .884 ± .025, although in the second performance there was introduced a factor not included in the first.

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[16] H. Münsterberg, *Psychology and Industrial Efficiency*, 1913, 68 ff.; Marx Lobsien, Prüfung der Aufmerksamkeit an Kindern mit der Münsterbergschen Schlittenmethode, *Zsch. f. angew. Psychol.*, 13, 1918, 395 ff.

[17] M. L. Dunbar, *Felicilla's Easter*, 1903, 7 f. [18] *Ibid.*, 13 f. [19] *Ibid.*, 1 f.

(*R-9*) *Clock Problem Experiment. Apparatus.* Two series, each of 6 clock problems such as Terman [20] uses in his intelligence scale, were used with the exposure apparatus by which the nonsense syllables were shown.

Procedure. The instructions were arranged to suggest the use of visual imagery rather than the seeking for and application of some mechanical means of arriving at the answer. As *S* spoke the answer to the problem exposed the next one was moved into place. The time was taken for the entire series. As previously stated one series was given in each of the first two periods. The rate score is the total of the times required for both series. Two more series were constructed and given a week after the first to 31 members of Group I. The rate scores for the first two and last two series were correlated and gave a coefficient of reliability of .794 ± .045.

(*R-10*) *Mystery Problem Experiment. Material.* The plan of this experiment is similar to a single part of the Burt reasoning test.[21] The mystery story, for which possible solutions have to be selected, is an adaptation from a Scandinavian story by Ingeman.[22] It comprised a single type written sheet. In the final form 14 solutions are suggested on the answer sheet, of which 6 are possible and 8 impossible according to the evidence presented. The first step in standardizing was to seek an arrangement of the material and a procedure that would prove satisfactory from the points of view of scoring and time required. The second step was to compare the results obtained by this procedure with those secured in 3 other tests requiring the thinking through of a situation or problem. At first *S* was required to suggest all possible solutions that occurred to her and signify the one regarded as most probable. This method gave results extremely difficult to score and required too long a time. Next 25 solutions were listed from which *S* selected those she considered possible. In this form the experiment was given twice, a week apart, to 21 members of Group I. We tried various methods of scoring and finally settled temporarily on the following. Only 14 of the 25 solutions were scored. These included the 6 possible ones, 4 which involved one of the 2 impossible conditions, *i.e.* either wounds on the body or the presence of a second person, and 4 which necessitated both of these. Points were given as follows: +1 for every possible solution checked, −1 for every impossible one. The one selected as most probable was given an additional score of from +1 to +6, according to the degree of probability, if a possible solution, and −1 if it involved one of the impossible conditions or −2 if both were required. When the data for the coefficients of reliability for the rate and strength scores had been gathered it was found advisable to make a slight change in the scoring. We gave a score of +4 if no solution involving either of the impossible conditions were checked, −2 if any solution checked involved one of these impossible circumstances, and −4 if both were required by anyone of the solutions checked. To this was added the number of points received for the doubly checked solution. This latter method was used in treating the results on which the coefficients of reliability are based and with the final data; the former method in all other calculations.

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[20] L. M. Terman, *The Measurement of Intelligence*, 1916, 321.

[21] C. Burt, The development of reasoning in school children, *J. Exper. Ped.*, 5, 1919, 73 ff.

[22] B. S. Ingeman, *The Sealed Room*, 1, 1909, 271 ff.

The tests used for comparison were the Burt Reasoning Test,[23] Syllogism Test D,[24] and the arithmetic problems in Test 5 of the Terman Group Test of Mental Ability, Form B.[25] Two sets of 15 problems from the Burt scale were arranged by the writer and Miss Kurtti to be given by the group method. For the most part only the more difficult problems were used. These were selected to include the widest variety of situations possible, and were divided into 2 balanced sets.

The four tests to be considered were given to 22 members of Group I; the first as an individual test, the others by the group method. Only 10 members of this group could be secured for a second performance of the tests a week later. The coefficients of reliability for the scores were:

Mystery Problem	.835 ± .063
The Burt Reasoning Test	.685 ± .115 (Forms I, II)
Syllogism Test D.	.850 ± .058
Arithmetic Problems	.798 ± .075

To investigate the possibility of a factor common to all 4 tests and to determine to what extent this factor entered into the solving of the mystery problem, every score in the 4 sets was reduced to terms of the standard deviation for the set of which it was a part. This score was expressed as a percentage of the deviation and marked (+) if above the average score and (−) if below it. The 4 reduced scores thus obtained for *S* were averaged for a composite score. The 4 sets of reduced scores were then correlated, using the method described by Woodworth,[26] with this set of composite scores and yielded the following results:

Mystery Problem and Composite Score = .712 ± .067
Burt Reasoning Test (Form I) and Composite Score = .781 ± .057
Syllogism Test D and Composite Score = .630 ± .087
Arithmetic Test and Composite Score = .685 ± .077

We believe these results indicate the existence of a common factor and justify the use of the Mystery Problem experiment for investigating it.

Procedure. We placed the selection to be read and the answer sheet face down on the table before *S*, the story at her right, then read the following instructions.

"I want to see how well you can think through this problem. It is a mystery to solve. You are to determine how Lieut. Ribot met his death. First read the passage straight through aloud. As soon as you have finished reading it turn this other paper over. There you will find suggested a number of solutions to the mystery. There is one *correct* solution; the others may be possible or impossible. You are to put a check before *every possible* cause of Lieut. Ribot's death and a second check before *the one* which you judge to be the *correct* solution. Before you check the solutions weigh carefully the evidence presented throughout the passage. Part of the evidence points to the one correct solution, part to other possible solutions, and part is misleading, suggesting solutions which *the facts* given show to be impossible. Accept all *statements of fact* as true. You may refer to the passage while checking. Work quickly and carefully. Tell me just as soon as you have finished. Are there any questions? Now begin: read the passage aloud."

We required oral reading to prevent the immediate rereading and studying of sections by some *S*s, and thus insured a more uniform method of attack. The stop-watch was started when *S* began to read the passage and stopped when she reported that she had finished, or, since the instruction to report was frequently forgotten, when she started to hand the paper over or looked up indicating that she was satisfied with the placing of her second check. The coefficient or reliability for the rate score based on 32 cases is .628 ± .072.

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[23] Burt, *op. cit.*,

[24] *Syllogism Test D*, Dept. Educ. and Psychol., Carnegie Institute of Technology, 1921.

[25] L. M. Terman, *Terman Group Test of Mental Ability*, 1920.

[26] R. S. Woodworth, Combining the results of several tests: A study in statistical method, *Psychol. Rev.*, 19, 1912, 97 ff.

(*R-11*) *Voluntary Reading Time.* This rate score was secured as incidental to the strength score *S-9*, and was the rate which *S* voluntarily selected as her usual rate for oral reading.

Material. The *Caterina* selection mentioned under *R-8* was the passage read.

Procedure. For the instructions, see experiment *S-9*. The coefficient of reliability, based on 58 cases, is .852 ± .024.

(*R-12*) *Judgment of Weights, Voluntary Time.* We followed rather closely Whipple's[27] description of Binet's test for depersonalized suggestion by progressive weights.

Materials. A set of 15 weights, of identical size and appearance, and numbered conspicuously from 1 to 15 were used. The first four weighed 42, 63, 84 and 105 grm. respectively; the remaining 11 each weighed 126 grm. These were arranged as numbered in a row, about 2 cm. apart, on a thick black cloth, No. 1, the lightest, at the left, and the eleven heaviest, at the right.

Procedure. The instructions were: "Here is a series of weights, 15 of them. You are to lift them, one after the other, like this, with thumb and fore-finger of the right hand to the height of the weights (10 cm.). As you lift each weight you are to say whether it is heavier, lighter, or the same as the one just before it. Compare each weight as *you lift it.* Here is the first weight, No. 1 at the left of the row." *S* did not know that a record of her rate was being taken; time was not mentioned unless *S* waited for a signal to start; in that case *E* told her to start when ready, that time did not matter. The stop-watch was started when *S* lifted the second weight and stopped when the judgment on No. 15 was spoken. The time was measured from the second weight because some *S*s paused to ask a question while holding the first one. The coefficient of reliability for the rate of judgment, based on 62 pairs of scores, is .711 ± .043.

(*R-13*) *Carbon Copy Experiment. Part A — Voluntary Time.* The material and procedure have been treated in detail under *R-5*. The rate score is the time required for the task. Before this experiment was undertaken at all we endeavored to determine how constant *S*'s 'own time' or 'usual rate' as selected by her would be, and what effect would be exerted on such a rate by one or more speeded experiments preceding it. Twenty-seven members of Group I served as *S*s, though complete records were not obtained from all. Task 1 was to write the numbers 21 through 35, *i.e.* 30 digits; task 2 to write the letters of the alphabet *a* through *z* and again through *d*, *i.e.* 30 letters. Coefficients of reliability were obtained as follows:

(1) Where the 2 performances of task 1, at the voluntary rate, were separated by a short interval, but one in which the same task had been done as rapidly as possible and task 2 both at a voluntary and speeded rate, r = .937 ± .025.

(2) Where the second performance came at the end of the hour, so that a number of tasks, usually at a speeded rate, had been done in the meantime, r = .896 ± .032.

(3) Where the second performance followed the first after an interval of one week, both trials coming at the beginning of the hour, r = .845 ± .05.

(4) Where the first performance came at the beginning of the first hour and the second at the end of the second hour, a week later, r = .796 ± .06.

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[27] G. M. Whipple, *Manual of Mental and Physical Tests*, II, 1915, 232.

We thought it safe to conclude that an *S*'s choice of her 'own time' or 'usual rate' was sufficiently constant to be relied upon. The coefficient of reliability for the voluntary rate score, based on the 44 cases in Group II, justified this decision; it was + .813 ± .034.

(*R-14*) *Carbon Copy Rate Ratio.* This ratio, obtained by dividing the speeded time score by the voluntary time score, is used as an index of the degree to which *S* fails to work at the speed of which she is capable, or of the degree to which she sacrifices speed to accuracy. The ratio scores from Group II give a coefficient of reliability of .628 ± .062.

(*2*) *For Strength Measures.* (*S-1*) *Strength-Weight Ratio,* (*st/wt*). The muscle strength test used to obtain the strength score for this ratio was worked out and partially standardized by Lovett, Martin, and Rich.[28] It measures with a dynamometer the maximum resistance to a steady pull which is sufficient to overcome the contraction of the muscle group under consideration. That this is not a measure of the total potential strength of the muscle tissue, and not, taken alone, an index of physical condition, is evident. Besides the factors of the amount and of the quality of muscle tissue, as Martin and Rich[29] point out, there enters in the factor of the degree to which *S* is able and, it may be added, willing, to innervate the muscles. Rather than the utmost positive effort, the value obtained for each muscle is the 'breaking strength,' the tension shown on the scale at the instant the resistance of the contracted muscle is overcome by a pull in the opposite direction, exerted through the balance. In the complete scale 22 pairs of muscles were measured.[30] Martin and Rich[31] found, however, that a score could be computed for this entire group, from measures secured by testing 4 of the 22 pairs of muscles, which gave a coefficient of correlation of .94 ± .01 with the score gained by actually testing all 44 muscles. The procedure used in this experiment was the abbreviated one by which the strength of the right and left pectoral, forearm flexor, thigh adductor, and thigh abductor muscles was measured.

Apparatus. A light weight flat-face spring balance with a scale capacity of [illegible] lbs. by 2 lbs. intervals was equipped with a self-registering device. A wooden handle was attached by a swivel to the upper end of the scale and a woven strap attached in a loop about 30 in. in circumference by another swivel to the lower end of the balance. A stout, rather narrow, table with a cleat firmly fastened across one end and a cushion for *S*'s head were necessary for the last 3 pairs of the measurements. A door frame was used for *S* to lean against in the first part, and to aid *S* in steadying herself a rope with a wooden handle was attached to the wall about 10 ft. to the side of *S*.

Procedure. *E* was assisted by an adjustor whose duties were: (1) to place the loop in the assigned position about the arm or leg; (2) to support the loop with one hand and if necessary the arm or leg with the other hand; and (3) to give the commands 'hold back' to mark the beginning of the pull and 'stop' to mark the end. *E* held the handle of the balance in her right hand and the body of the balance in her left. At the command, 'hold back,' *S* contracted the muscle group being tested, and simultaneously *E* pulled the spring-balance, developing tension as rapidly as possible without jerking, and increasing it until the resistance of *S* was overcome. At the 'stop' signal the pull was discontinued, the scale read and recorded. Two trials were given with each group of muscles and the higher of the two measures used. Detailed directions for testing the 4 pairs of muscles are given by Martin and Rich.[32] The strength of these 8 muscles is equal to 15% of the entire strength by the complete test; therefore to calculate the total strength score, the sum of the measures for these 8 muscles was multiplied by the reciprocal of 15 or 6.67.[33] The ratio of the estimated total strength score to *S*'s weight measure taken at the beginning of this experiment was used as the final score. Martin and Rich[34] considered this a more reliable index, since a coefficient of correlation of .58 ± .04 was found between the strength score and weight. The coefficient of reliability of the ratio, based on 30 cases from Group I, is .749 ± .054.

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[28] R. W. Lovett and E. G. Martin, Certain aspects of infantile paralysis; with a description of a method of muscle testing, *J. Amer. Med. Ass.*, 66, 1916, 729 ff. Cf. also E. G. Martin, Muscular strength and muscular symmetry in human beings: I. In children, *Amer. J. Physiol.*, 46, 1918, 67 ff.; and E. G. Martin and W. H. Rich, Muscular strength and muscular symmetry in human beings: II. In adult males, *ibid.*, 47, 1918, 29 ff.

[29] *Ibid.*, 39 f. [30] Lovett and Martin, *op. cit.*, 730. [31] *Op. cit.*, 34 ff.

(*S-2*) *Carbon Copy Score.* The purpose of this experiment is to measure the amount of physical force exerted as pressure during the performance of set tasks. The material and the procedure for part A and B have been taken up in detail under *R-5*. The procedure for part C and the method of scoring will be considered here. Part C was given to the subjects in small groups. The aim here was to set a task difficult to start with and increasing in difficulty to the point where there was little chance of its being done perfectly, so that the *S*s would be obliged to put forth every effort possible. For the first row of this part the instructions were: "This is another test for accuracy. Do it as in Part C. I shall dictate, rapidly, a series of digits which you are to write down as I give them, one digit between every pair of vertical blue lines. The height of the digits does not matter. Leave no spaces; there are 25 spaces and I shall dictate 25 digits. If you miss a digit, guess at it, for I shall not repeat any." *E* illustrated the rate, which was 2 per sec., then read the following series: 3754946281573681492385697. In the instructions for the next section, the *S*s were told, "This time I shall dictate letters rapidly. You are *not* to write the letter I say but the one that *follows* it in the alphabet. As before, write one between every pair of vertical blue lines. The height of the letters does not matter. Leave no spaces; if you are not sure of a letter guess at it." Here the rate was 1 letter per sec.; it was illustrated before the following series was read: aplgwcxnitfgyhobsjmudkvwe. The instructions for the last part were: "This time you are to write numbers. I shall give a number of two digits and you are to write the number which you get by subtracting one from the first digit and adding one to the second digit. Put one digit in each space, *i.e.* use 2 spaces for each number. Leave no spaces." To make sure the directions were understood the method was demonstrated on the blackboard with the number 58. This series was read at the rate of one every 2 sec. The numbers used were 72, 46, 60, 38, 81, 47, 28, 32, 97, 53, 25, 64.

The method of scoring was to count, for every part, including the heading where *S* wrote her name, the date, and Accuracy of Movement Test, the number of sheets of onion skin paper on which any of the carbon could be detected. Care was taken to do this in a bright but not glaring light. At times it was necessary to examine the carbon to differentiate between markings in the paper and carbon on its surface. If there was carbon on the second sheet under the heading, on the third under A, on the fourth under B and on the sixth under C, the score would be 15. As has been said, this experiment was given twice to Group II. The sum of both scores was used for the final strength score. The coefficient of reliability computed from these two sets, is .833 ± .030.

(*S-3*) *Woodrow's Measurement of Attention Experiment.*[35] This experiment may measure attention as Woodrow claims, or it may get at a more highly organized mental process, that of holding things (signal, stimulus, reaction, etc.) in order, and measure *S*'s ability to carry out a type of plan, as Professor Shepard suggests. Which supposition is nearer the truth does not matter here, since we were satisfied that the experiment did give a measure of something not touched by the other experiments.

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[32] *Op. cit.*, 35 f.

[33] L. M. Moore and J. L. Barker, Monthly variations in muscular efficiency of women, *Amer. J. Physiol.*, 64, 1923, 3, 405. In their work, these investigators multiplied the sum of the 8 measures by 7.04, since this sum has been found to be 14.2% of the total strength in the case of women where it is 15% for men.

[34] *Op. cit.*, 38 ff. [35] *Op. cit.*

Apparatus and Procedure. This was just as in the simple reaction-time experiment which preceded it and furnished one of the 2 scores used in computing the final score. The instructions were: "In the next part of the experiment the interval between the 'ready' signal and the stimulus will vary in length. Otherwise it is identical with the first part. React *as quickly as you can*, just as soon as you hear the stimulus. Do your very best *every reaction*." The set of irregular intervals used was the one given by Woodrow.[36] Practice was given on the last 3 of the set before the times were recorded. The score is the absolute difference between the average reaction time for all the 2 sec. intervals and the average for all the irregular intervals. A low score is taken as an indication of strength. The coefficient of reliability for this score, based on 2 sets of 31 scores secured a week apart, was .425 ± .099.

(*S-4*) *The Maze Experiment.* The material and procedure have been given in detail under *R-6*. The strength score used was the total number of errors made in all trials with both forms. A coefficient of reliability of .402 ± .05 was obtained when 2 sets of 80 scores each were correlated. The scores in one set were the total number of errors made in all trials with Form I on the first day that it was given; those in the other set the total number made with Form II on the first day that it was given. Thirty-one *S*s worked with both forms of the maze again a week after their first performance with them. The coefficient found between the total number of errors made with Form I and II the first week, and the total number made the second week was .429 ± .099.

(*S-5*) *Münsterberg Strength Score.* Here again it is uncertain just what process or processes were being tested. It is probable that breadth of vision, perception, and attention—either its breadth or its flexibility—played a large part in the performance, which required the noting of the color, form, and locality of all the digits. The ability to construct speedily a systematic procedure quite probably increased efficiency to a great extent. The apparatus and procedure have been given in detail under *R-7*. The score used here was the total number of danger points that were omitted; though recorded, no attention was paid to the letters wrongly called. The coefficient of reliability, computed from 2 sets of 30 scores obtained a week apart, is .869 ± .031.

(*S-6*) *Cancellation Experiment.* An account of the material and procedure for this experiment has been given under *R-8*. The score used here was the total number of errors, both words wrongly cancelled and those omitted. The coefficients of reliability for this score, when computed for 2 trials a week apart, both using the *Holy Week* selection, with 33 *S*s, is .800 ± .042. When based on the first trial by 35 *S*s with *Holy Week* and the first trial with *Antwerp*, given on the same day, the coefficient of reliability is .875 ± .027, though a new factor, that of attending to the sense of the passage for the purpose of recall, entered into the latter task.

(*S-7*) *Clock Problem Experiment.* The material and procedure have been given under *R-9*. The score was the total number of problems in both series correctly answered. An answer was considered correct if it fell within the five minute interval; for instance, should the answer be 3:23, any answer between 3:20 and 3:25 inclusive would be accepted as correct. To determine the coefficient of reliability a third and a fourth set of 6 problems each were given, a week after the first sets, to 31 *S*s; it was found to be .638 ± .072.

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[36] *Ibid.*, 127.

(*S-8*) *Mystery Problem Experiment.* The material, procedure and method of obtaining the strength score have been described under *R-10.* The experiment was performed a second time, a week later, by 32 *S*s. They were instructed to go at the problem as carefully as though it were for the first time; they might arrive at the same conclusions or different ones. They seemed to carry out the directions quite faithfully, and many changes occurred in the second series of checkings, though the same types of solutions were considered possible. These data gave a coefficient of reliability of .502 ± .089.

(*S-9*) *Logical Memory Experiment. The Recall of Ideas. Material.* The *Caterina* selection mentioned under *R-8* was used.

Procedure. The instructions were, "This experiment is one to see how much of a selection you can recall after reading it through once. When I say 'go' turn the paper over and read the selection straight through aloud. Read it at your usual rate for oral reading. Put your attention on the sense rather than on your manner of reading. As soon as you have finished reading turn the paper over, and write as much of the passage as you can recall. Use the words of the passage as far as you can remember the exact wording; when you do not recall it, use your own words. Read it straight through once. Go." After *S* finished writing she was further directed to put down any other separate words or phrases recalled out of context.[37] An idea recorded was considered satisfactory if it approximated the original to a rather close degree. The experiment by Henderson[38] served as a guide in this respect. The score was the total number of ideas recalled. When the *Caterina* passage was given twice, a week apart, to 33 *S*s, the scores gave a coefficient of reliability of .764 ± .049; when the scores for *Caterina* and for *Antwerp* were correlated, though the latter included a factor not in the former, *i.e.* cancelling, as with the *Holy Week* selection, while it was being read for recall, the coefficient of reliability for recall for 35 cases was .782 ± .044.

(*S-10*) *Suggestibility Experiment.* The material has been given under *R-12.* The score was the number of weights that were incorrectly judged as heavier. Here as in *S-3* a low score is considered indicative of strength. The coefficient of reliability for scores secured a week apart from 62 *S*s is .567 ± .058.

We had planned for two other strength scores, but gave them up because of their apparent unreliability. The score for the written recall of the nonsense syllables after a lapse of 28 hours gave a coefficient of reliability of .105 ± .12. We had hoped to secure an index of the breadth or the flexibility of attention by having *S* cancel words in the *Antwerp* selection, as she had done with the *Holy Week* passage, *while* she read it straight through for recall. The ratios of the cancellation time, cancellation errors and recall scores in the single tasks, to the same scores in the double task were to have been combined for this final score. Although the lowest coefficients of reliability for any one of the 6 individual scores was .659 ± .066, the coefficients for the ratios were disappointing. The time ratio gave a coefficient of reliability of .595 ± .076, the error ratio a coefficient of −.018 ± .114, and the recall ratio one of .247 ± .110. It was evident that a score composed of ratios apparently so unreliable would itself have slight if any reliability.

(*3*) *Summary of Results.* We have expressed the individual scores in terms of rank, 1 signifying the highest place for rate and strength, 44 the lowest place. See Table I and II for sample cases.[39] Composite scores for every *S* for rate and strength were computed in two ways. One composite score was obtained by averaging the ranks secured in all tests of the rate series and

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[37] She was also requested to underline every word that she was certain had occurred in the passage, but this score was not used, since we thought a low score might in some cases indicate weakness, as shown in lack of self-confidence, and in other instances strength, as evidenced by caution.

[38] E. N. Henderson, A study of memory for connected trains of thought, *Psychol. Rev. Mon.*, 5, 1903, 34.

TABLE I
Rankings in Rate Experiments

S	R-1 Tap.	R-2 S.R.T.	Av. R. 1 & 2	R-3 A.R.T	R-4 N.Syl.	R-5 CC.S	R-6 Maze	R-7 Mun.	R-8 Can.	Av. 3, 4, 5, 6, 7, & 8
A	12.0	22.5	17.5	9.0	3.0	9.0	35.5	29.0	13.0	21.6
B	40.0	25.5	32.8	38.0	8.0	42.0	43.0	27.5	25.0	34.4
F	42.0	27.0	34.5	43.0	32.0	32.0	33.0	12.0	42.0	29.8
M	41.0	20.5	30.8	12.0	14.0	19.0	41.0	11.0	4.5	18.9

S	Av. R. 7 & 8	R-9 Cl. P.	R-10 Myst.	R-11 R.T.	R-12 J.Wt.	R-13 CC.V.	Av. 11 12 & 13	R-14 CC/SV	Com. Score % Meth	Com. Score Av.R.
A	21.0	33.0	38.0	16.0	1.0	10.0	9.0	15.5	68	17.6
B	26.3	3.0	33.0	39.5	43.0	29.0	37.2	7.5	21	28.9
F	27.0	42.0	22.0	41.0	17.0	40.0	32.6	35.0	21	32.8
M	7.8	13.0	17.0	19.0	11.0	14.0	14.7	13.0	86	17.9

TABLE II
Rankings in Strength Experiments

S	S-1 S/W.	S-2 CC.	S-3 W's A.	S-4 Maze	S-5 Mun.	S-6 Can.	Av.R. 4 % 5
A	10.0	18.0	36.0	16.5	22.0	6.0	14.8
B	1.0	15.5	42.0	23.0	14.0	35.0	24.0
F	10.0	24.0	16.0	3.5	38.0	35.0	25.5
M	3.0	11.0	43.0	16.5	38.0	38.0	30.8

S	S-7 Cl P.	S-8 Myst.	S-9 R.Id.	Av. R 7, 8, 9	S-10 J. W.	C. Sc. %	C. Sc. Av.R.
A	39.0	16.5	7.0	20.8	14.5	80	18.6
B	15.5	16.5	1.0	11.0	6.5	75	17.0
F	31.5	25.0	33.0	29.9	27.5	35	24.4
M	39.0	33.5	21.5	31.3	41.0	35	28.5

again all in the strength series. For the other score the number of times that *S* gained a place in the quicker half of the group was expressed as a percentage of the number of possibilities of being in the group considered, *i. e.* as a percentage of the number of experiments in the series. The two forms of composite rate scores gave a coefficient of correlation of .894 ±.024, and those for the strength scores one of .859 ±.029. Because of the close correlation only one, the percentage score, has been considered to any extent. Further presentation of the results and their discussion will be considered later.

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[39] Copies of Tables I, II, IV, and VII in their complete form are on file in the University of Michigan Library and may be borrowed under the usual courtesies of library exchange.

C. PHYSIOLOGICAL MEASURES

Temperature, pulse rate, and blood pressure were taken at the end of the period with *S* seated. It was seldom necessary for *S* even to leave her chair more than once during the hour, so that the measures were taken after a comparatively long period entirely free from any muscular activity which would be likely to influence them. All *S*s had spent the time in performing the same mental tasks. They were comfortably seated beside the table on which they had been working and were instructed to relax, to rest, and "think about nothing in particular." The temperature was allowed to register in the mouth while *E* counted the pulse rate; blood pressure was taken last.

(*1*) *Pulse Rate.* *E* counted the pulse at the left wrist for two or three 30-sec. intervals to make sure that it was constant, and that the first rate had not been influenced by the newness of the situation. The rate for 30 sec. was doubled for the measure. The final rate used was the average of the 4 obtained during that number of periods. For the coefficient of reliability see Table III, and for samples of the measures, Table IV.

TABLE III
Coefficients of Reliability for Physiological Measures

Measured during	Pulse Rate	Blood Pressure	Temperature
A.M.	.691 ± .056	.679 ± .058	.212 ± .101
P.M.	.734 ± .049	.638 ± .063	.523 ± .076
A.M. + P.M.	.792 ± .041	.716 ± .054	.492 ± .084

TABLE IV
Physiological Measures

	Pulse Rate					Blood Pressure					Temperature				
S	AM$_1$	PM$_1$	AM$_2$	PM$_2$	Av.	AM$_1$	PM$_1$	AM$_2$	PM$_2$	Av.	AM$_1$	PM$_1$	AM$_2$	PM$_2$	Av.
A	84	84	74	74	79	104	96	96	92	97	97.2	98.6	98.2	98.4	98.3
B	80	76	76	60	73	92	90	96	96	94	98.2	99.0	98.4	98.6	98.6
F	96	96	92	86	91	100	88	80	84	88	99.0	99.0	97.6	98.0	98.4
M	74	76	76	68	74	110	102	104	106	106	96.6	97.4	97.6	97.4	97.3

(*2*) *Systolic Blood Pressure.* The Tycos sphygmomanometer was used with the auscultatory method. The compression sleeve was placed about the bare left arm just above the elbow, and the stethoscope applied over the radial artery at or above the elbow. Except in one or two instances where *S* was restless because of discomfort or haste, 2 or 3 readings were taken at each period, with rest intervals of at least a minute, for verification and to eliminate the possibility of the first rate's being high because of *S*'s unfamiliarity with the procedure. The measure used was the average of the 4 measures secured on 4 different days. For the coefficient of reliability see Table III, and for samples of the measures, Table IV.

(*3*) *Temperature.* A Tycos fever thermometer, requiring one minute for registration, was used. More than the required time was allowed and the reading taken at least twice to make sure that the temperature was not still rising. See Table III for the coefficients of reliability and Table IV for a few of the readings.

(*4*) *Summary of Results.* It will be noted (see Table IV) that the average temperature and blood pressure were low. Burlage[40] in a study of 1700 cases of public school and University girls found the average pulse rate for girls of 18 years to be 79.78 and to fluctuate little up to 26 years of age. Our average agrees closely with this. Schneider and Truesdell[41] found the average blood pressure of the group of normal women whom they were studying to fluctuate from 95 to 107 mm., with a midpoint of 101, for recumbency; and from 104 to 113.7 with a midpoint of 108.7, for standing position. Alvarez,[42] studying pressures obtained from 5807 women between the ages of 16 and 40 by palpation with *S* in standing position,[43] found the average for the entire age range to be 115 mm. This however, is higher than would be expected for our group, since he also found that, instead of the average pressure rising steadily with age as it had been supposed to do, it dropped between the ages of 17 and 25 years, which are the age limits that include all but 2 of our cases. The range for women proved to be practically 85 to 155; there were 4 cases, ages 16 to 21, with pressures between 80 and 84. We believe that our measures may be slightly low because taken after an hour of comparatively slight body activity but rather strenuous mental exertion. O'Hare,[44] in a study of normal and vascular hypertension cases, found that physical rest caused a marked fall in blood pressure, the range being from 0 to 46 mm. with an average of 21 mm. Pillsbury and Griffitts,[45] found that pressure also fell during a period devoted to the performance of mental tasks. It is possible also that the circumstance that the measures were all taken in the spring, during the last two months of the school year, was a contributing factor.

Since authorities do not seem to agree[46] on the presence or absence of a diurnal rhythm in blood pressure and pulse rate, we averaged all measures taken at each hour from 9 A. M. to 5 P. M. The results are stated in Table V. The differences between the averages for measures taken at different hours were less than the variations found in many instances in measures taken for the same *S* at the same hour on two days. We concluded that it would therefore not be safe to associate unexpected findings in any case with the hour factor.

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[40] S. R. Burlage, The blood pressures and heart rate, in girls, during adolescence: A statistical study of 1700 cases, *Amer. J. Physiol.*, 64, 1923, 259.

[41] E. C. Schneider and D. Truesdell, Daily variations in cardiovascular conditions and a physical efficiency rating, *Amer. J. Physiol.*, 67, 1923, 199.

[42] W. C. Alvarez, Blood pressure in university freshmen and office patients, *Arch. Int. Med.*, 26, 1920, 381 ff.

[43] The measures in this study would tend to be higher than ours because of the difference in *S*'s position, and on the other hand would tend to be lower because of the difference in the method used. These tendencies would probably just about compensate, making the data comparable with ours.

[44] J. P. O'Hare, Vascular reactions in vascular hypertension, *Amer. J. Med. Sci.*, 159, 1920, 370.

[45] C. H. Griffitts and W. B. Pillsbury, An experiment on indirect measures of fatigue, *Proc. VII. Inter. Cong. Psychol.*, 1924, 212 ff.

[46] Schneider and Truesdell, *op. cit.*, 194 ff. These investigators state that early students of pulse rate frequency came to the conclusion that it "is greater in the morning than in the evening," and that the heart was more excitable in the morning; also that Weysse and Lutz "concluded that the systolic pressure rises an average of 8 mm. immediately after the ingestion of food, then gradually falls until the beginning of the next meal, and that there is a general rise during the day, whereas their observations lend support to the more general increase in rate throughout the day and fail to indicate a decided diurnal change in the excitability of the heart," and show no general day-time rise in the level of arterial pressures for the groups that ate the usual meals, though such a gradual rise was found in a group that went without meals.

TABLE V

Distribution of Physical Measures by Hours

Hr.	Pulse Rate		Blood Press.		Temperature	
9	80 (*23*)		100 (*23*)		97.8 (*23*)	
	=79		=100		=98.0	
10	78 (*16*)		100 (*16*)		98.2 (*18*)	
		=79		=99		=98.2
11	80 (*25*)		98 (*26*)		98.1 (*26*)	
	=78		= 98		=98.3	
12	75 (*21*)		98 (*20*)		98.5 (21)	
2	83 (*24*)		100 (*24*)		98.1 (*22*)	
	=80		= 98		=98.2	
3	77 (*19*)		95 (*19*)		98.3 (*19*)	
		=79		=98		=98.3
4	81 (*21*)		95 (*21*)		98.4 (*21*)	
	=78		=97		=98.4	
5	74 (*20*)		98 (*20*)		98.4 (*20*)	

The figures in *italics* give the number of measures.

Another point on which there is disagreement[47] is that of the presence, (and nature, if present), or the absence of a monthly rhythm in cardio-vascular activities in women, and the presence or absence of a periodic mental and motor inefficiency. To determine whether or not this factor should be considered in our study, the average of all measures for every one of the 3 physiological processes was found for the menstrual period, for the 5 days previous to the onset, for the 5 days following the close, and for the inter-menstrual period. Our results, stated in Table VI, show slight differences for the 4 periods and are in general agreement with the conclusion reached by King,[48] and by Jacobi and other early workers mentioned by King, rather than with the findings of Moore and Cooper.[49] However, when the records of our individual *S*s were studied, the directions of the variations concomitant with differences in physical condition were not consistent. Also similar differences were found between measures for the same *S*, all taken during the same type of period; and, on the other hand, identical measures were obtained for an individual at different phases of the cycle. We therefore concluded, as in the case of the distribution of measures by hours, that it would not be safe to associate unexpected findings with this periodic factor.

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[47] L. M. Moore and C. R. Cooper, Monthly variations in cardio-vascular activities and in respiratory rate in women, *Amer. J. Physiol.*, 64, 1923, 416 ff. According to these writers the theory of presence of a rhythm was put forward by Jacobi in 1876 and held sway until investigations during the last decade in general produced evidence in apparent refutation. Their own study (*ibid.*, 420), however, and that by King (J. L. King, Concerning the periodic cardio-vascular and temperature variations in women, *Amer. J. Physiol.*, 34, 1914, 203 ff.) both support the rhythm theory, though they do not seem to agree concerning the order of the phases. King, as did Jacobi and others, found that there was "rhythmical movement in the life-processes," the highest point of which was reached from 2 to 4 days before the menses, the lowest point about 3 days after their cessation; whereas Moore and Cooper (*op. cit.*, 423) found that the typical variations are a low menstrual and a high inter-menstrual period. An investigation by Moore and Barker (L. M. Moore and J. L. Barker, Monthly variations in muscular efficiency in women, *Amer. J. Physiol.*, 64, 1923, 414) suggests a "menstrual decrease in strength." Another investigation, one by Hollingworth (L. S. Hollingworth, *Functional Periodicity*, 1914, 94) gives evidence in favor of there being no "periodic mental and motor inefficiency in normal women."

TABLE VI

Distribution of Psychological Measures (averages) with Respect to Menstruation

	Pulse Rate	Blood Press.	Temp.
Intermenstrual period	76 (*77*)	97 (*77*)	98.0 (*78*)
Within 5 days of onset	82 (*33*)	100 (*33*)	98.5 (*33*)
Menstrual period	82 (*32*)	101 (*32*)	98.3 (*32*)
Within 5 days after close	77 (*20*)	97 (*20*)	98.0 (*20*)

Figures in *italics* gives the number of measures.

The record of physical examinations and medical attention received while attending the university was investigated for all *S*s, with special note of any reference to thyroid abnormality. Such record as there was did not indicate, however, the nature of the disturbance, so that it was impossible to tell whether one should expect pathological cardiovascular conditions, and in which direction any change which might be present had occurred.

D. QUESTIONARIES

(*1*) *Questionary A*. Questionary A was for the purpose of obtaining general information and of giving instructions. It stated that all appointments must be kept in order to secure credit for the work, that the scores in the experiments would not affect the grade in the course provided all parts were conscientiously done, and that they should not discuss the experiment with any one taking the elementary course.

(*2*) *Questionary B*. A daily report of *S*'s physical and mental condition was secured through this questionary, which was filled out at the close of each of the first four periods. She was asked to report concerning the number of hours she had slept the previous night, whether sleep was continuous or broken, to note the affective and emotional quality of dreams, to state whether she had eaten breakfast and lunch on that day, and to evaluate her physical condition and 'frame of mind' according to a scale given her.

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[48] *Op. cit.*, 218; cf. footnote 47. [49] *Loc. cit.*

(*3*) *Questionary C.* This questionary was filled out twice, a week apart, by every *S* for herself. It was then filled out by a member of *S*'s immediate family, preferably her mother, and by three young women friends. (Through misunderstanding 6 of the 132 in the last group were filled out by men friends; this was not discovered in time to correct the error.) The intra-group method of procuring judgments could not be used, since the *S*s were for the most part strangers to each other. The following instructions were printed on the first sheet of the questionary:

"Please fill out this questionary carefully and frankly. It will be considered absolutely confidential and used only in connection with the research problem of which it is a part.

A number of characteristics have been arranged in pairs: 1, (a) and (b); 2, (a) and (b), etc. Each pair is composed of two opposed traits. To fill out the questionary you are to assign a group number to each characteristic (a) and to each characteristic (b). First imagine all the young women *of the population at large* who are between the ages of 17 and 25 years and who have had at least average educational advantages, that is, at least a year or two of high school work, divided into six groups according to the degree to which they possess each of the characteristics (a) and each of the characteristics (b). Those in group 1 are the one sixth of the entire section of the population selected for comparison (chosen on a basis of age and education) who have the characteristic under consideration, for instance 1 (a), to the least degree. Those in group 2 are the sixth having it to a greater degree and so on. Group 6 will be the sixth who have it to the greatest degree. The same will be true when trait 1 (b) is considered, except that the grouping will be reversed, since the traits are arranged in pairs of opposed characteristics; those who were in group 1 on a basis of trait 1 (a) will be in group 6 on a basis of trait 1 (b) and those in group 4 for characteristic 1 (a) will be in group 3 for 1 (b). To assign a group number to the characteristics listed below, decide in which of the six groups the person for whom you are filling out the questionary would belong with respect to each of the traits, first, 1 (a), and then 1 (b), and place the numbers of the groups in the appropriate places; next do the same for 2 (a), then for 2 (b) and so on. *Do not omit any.* The *sum* of the two group numbers assigned for any pair *must be seven.* Do not place the person between groups. You must place her in one of the six groups. Read the entire list through once before you begin to assign group numbers. Be careful to distinguish between characteristics which at first appear to be identical; no two are just the same."

A few of the sixty traits are listed here:

1: (a) To be quick in her movements,
(b) to be slow in them. (a)——(b)——

4. (a) To work carefully and accurately,
(b) to work carelessly and inaccurately. (a)——(b)——

18. (a) To be fluctuating,
(b) to be resolute. (a)——(b)——

26. (a) To be gloomy and depressed,
(b) to be cheerful and happy. (a)——(b)——

31. (a) To become angry easily, to be quick tempered,
(b) to be slow to anger and only under great stress. (a)——(b)——
47. (a) To act after deliberation,
(b) to act impulsively. (a)——(b)——

The method of scoring was to consider separately the traits, *i.e.* either (a) or (b), which were thought to indicate quickness, strength, activity, steadiness, or a tendency to be cheerful. We shall speak of these as positive traits. There are 14 traits considered indicative of quickness, 29 which were thought to be suggestive of strength, 6 were taken as indicative of activity and 7 to indicate steadiness. Seven characteristics were thought to point to a tendency to be cheerful; ease of, or quick initial arousal of, the emotions seemed to be suggested by 6; and an idea of the intensity or strength of the emotions may be gained from 4 traits.

For the final score for a trait the 2 numbers assigned by *S* to a trait were averaged and this score averaged with the 4 numbers assigned by the other persons to the same trait. With the scale of rating used, the greatest possible range for the 44 scores for any one trait is 5. The least actual range for any one of the 60 traits was 1.9, the maximum 5, and the average range 3.5. To find the composite rate and strength scores for the questionary, we found the average score for each of the positive traits, then gave as a rate score for *S* the percentage of times, out of the 14 possibilities, that she gained a score above the average for a trait in the rate group, and as a strength score the percentage of times she secured a rating above average in the 29 strength traits. The composite scores for the remaining groups, where the number of traits included was smaller, were the total number of points gained in a group by receiving a score of +1 for each trait within the group for which *S* secured a rating above the average for the trait and −1 for each trait in which she was below average. A few of these scores are given in Table VII.

TABLE VII
Questionary C Scores

S	Rate	Strength	Steadiness	Activity				Emotions		Mood	
				Physical	Mental	Both P&M	Total	Arousal Rate	Intensity	Score	Steadiness
A	89	69	+1	0	+3	+1	+4	+6	+4	+4	−1
B	89	55	+1	0	+3	+1	+4	+4	0	−1	−1
F	7	48	+3	−2	−1	−1	−4	−4	0	+4	−1
M	64	86	+5	0	−1	+1	0	−4	+2	0	+1

(*4*) *Questionary D.* *S* filled out this questionary after completing Questionary C. Part A consisted of a list of 35 activities such as baseball, dramatics, designing or drawing, and cooking. *S* was instructed to consider herself a participant in these and assign a value of −1 if displeasure or unpleasantness was derived from an activity, of 0 if neither displeasure nor enjoyment was derived, and a value of 1, 2, 3, or 4 as the degree of pleasure increased. Part B inquired into *S*'s habits of sleep; information concerning past and present health conditions was sought in parts C, D, and F. The questions in part E pertained to nervous habits such as the biting of the finger-nails and rocking. We made use of these data in the case studies which follow the statistical treatment of the results obtained from the experiments and the preceding set of questions.

III. INTERRELATION OF RESULTS

In Tables VIII and IX will be found the coefficients of correlation between the various experimental scores and between each score and the composite score for the series. The second composite rate score used in Table VIII was computed on a basis of the scores which had both a coefficient of reliability of .700[50] or more and a coefficient of correlation with the first composite score of at least .270. The second composite strength score in Table IX was based on the scores which had a coefficient of reliability of .429 or higher and a coefficient of correlation with the first composite score of at least .292. The coefficient of correlation between the two composite rate scores and between the two composite strength scores, however, are .906 ±.018 and .908 ±.009 respectively; also the coefficients of correlation between each one of the selected experiments and the second composite score were not very different from those obtained for the same experiment with the first composite scores. The 8 rate experiments gave an average coefficient of correlation of .426 with the first composite score and of .448 with the second composite score; the 7 strength experiments gave an average coefficient of correlation of .418 with the first composite score and of .454 with the second composite score. For these reasons we have used, to any extent, only the composite scores based on the entire series of experiments.

The coefficients of correlation between the 14 rate experiments can scarcely be said to give evidence of any one factor running through all. Fifty-eight of the 91 coefficients for pairs of experiments are positive; only 4 are at least 5 times their P.E., and 2 are 4 times it. The coefficients between the separate scores and the composite score are higher for the most part; 13 of the 14 are positive, 5 of these are 5 times the P.E. and one is 4 times it. The correlations are highest between the experiments in the group where the time was purely voluntary. The Münsterberg and cancellation experiments where speed and accuracy were stressed equally correlate relatively well with each other, and with the voluntary times for reading a selection and for judging the weights. The experiments requiring maximum speed do not here correlate well either with each other or with those of the other two types just mentioned. A similar impression is gained from an inspection of the coefficients for the strength scores. Of the 45 coefficients of correlation be-

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[50] Probable errors for coefficients of correlation where N = 44 are:

r	P.E.	r	P.E.	r	P.E.	r	P.E.
.000	.102	.250	.095	.500	.076	.750	.044
.050	.101	.300	.093	.550	.071	.800	.037
.100	.101	.350	.089	.600	.065	.850	.028
.150	.099	.400	.085	.650	.059	.900	.019
.200	.098	.450	.081	.700	.052	.950	.009

TABLE VIII

Coefficients of Correlation between Rate Scores

	R–1 Tap	R–2 S.R.T	R–3 A.R.T	R–4 N.Syls	;R–5 CC.S	R–6 Maze	R–7 Mun.	R–8 Can.	R–9 Cl.P.	R–10 Myst.	R–11 R.T	R–12 J.Wt.	R–13 CC.V	R–14 CC S/V	Pulse Rate	Temp.
R–1	+*.878*															
R–2	+.236	+*.589*														
R–3	+.022	–.020	+*.698*													
R–4	–.242	–.094	–.198	+*.663*												
R–5	+.059	–.211	+.098	+.096	+*.897*											
R–6	+.175	–.126	+.018	–.204	+.224	+*.809*										
R–7	+.112	+.071	+.010	+.212	–.020	+.196	+*.882*									
R–8	+.229	+.103	+.039	+.303	–.091	+.115	+.495	+*.883*								
R–9	+.120	+.041	+.329	+.150	–.041	+.246	+.181	+.454	+*.794*							
R–10	–.185	–.221	–.099	–.071	+.144	+.004	+.003	–.118	+.021	+*.628*						
R–11	+.327	–.038	–.238	+.181	+.121	+.055	+.305	+.385	–.033	+.024	+*.852*					
R–12	–.006	–.231	+.224	+.213	+.279	+.057	+.358	+.314	–.026	–.043	+.284	+*.711*				
R–13	+.041	–.130	+.016	+.031	+.715	–.037	–.053	–.025	–.153	+.273	+.297	+.236	+*.813*			
R–14	–.187	–.006	+.033	+.176	–.113	–.270	–.140	+.047	–.140	+.234	+.212	+.069	+.558	+*.628*		
P.R.	+.127	–.162	+.157	–.452	–.032	–.189	+.066	–.067	–.300	+.059	–.055	+.273	–.075	–.143	+*.792*	
Comp. Sc. 1.	+.094	–.03	+.239	+.293	+.277	+.271	+.419	+.587	+.366	+.144	+.455	+.581	+.455	+.275	+.098	–.0096
Comp. Sc. 2.					+.405	+.340	+.418	+.571	+.316		+.441	+.621	+.469			
Av. r. N=13	+.054	–.048	+.018	+.043	+.097	+.035	+.133	+.173	+.088	–.003	+.145	+.133	+.136	–.036		

TABLE IX
Coefficients of Correlation between Strength Scores

	S–1 St/Wt	S–2 C.C.	S–3 W's A.	S–4 Maze	S–5 Mün.	S–6 Can.	S–7 Cl. P.	S–8 Myst.	S–9 R. Id.	S–10 Sug.	Blood Pres.	Temp
S–1	+*.749*											
S–2	+.003	+*.833*										
S–3	+.162	–.142	+*.425*									
S–4	–.163	+.070	–.217	+*.429*								
S–5	–.201	+.222	+.124	+.159	+*.869*							
S–6	+.205	–.062	+.028	+.424	+.264	+*.800*						
S–7	+.111	+.051	+.130	+.173	–.015	+.246	+*.638*					
S–8	–.268	+.176	–.140	+.25	+.071	+.322	+.291	+*.502*				
S–9	–.103	–.083	–.043	–.187	–.101	+.261	+.006	+.126	+*.764*			
S–10	–.048	+.007	–.009	+.034	+.01	+.364	+.22	+.332	+.384	+*.567*		
Bl. P.	+.179	+.009	–.016	–.302	+.018	+.227	–.113	–.187	+.247	+.362	+*.716*	
Comp. Sc. 1.	–.275	+.231	–.083	+.432	+.343	+.552	+.38	+.47	+.292	+.46	+.058	–.129
Comp. Sc. 2				+.412	+.302	+.652	+.416	+.584	+.248	+.565		
Av. r. N=9	–.034	+.045	–.012	+.06	+.059	+.228	+.135	+.127	+.029	+.145		

tween the 10 experiments, 30 are positive, but only 1 is 5 times its P.E. and 2 are 4 times it. When correlated with the composite score, 4 of the 10 are at least 5 times the P.E. and 1 is 4 times it. Here again the experiments in which speed and accuracy are stressed equally appear to have some degree of interrelation; also the coefficient of correlation between the test for resistance to suggestion, where the rate factor did not enter at all, and the experiments in which quality rather than speed element predominated, suggest a closer degree of interrelation than do most of the other coefficients.

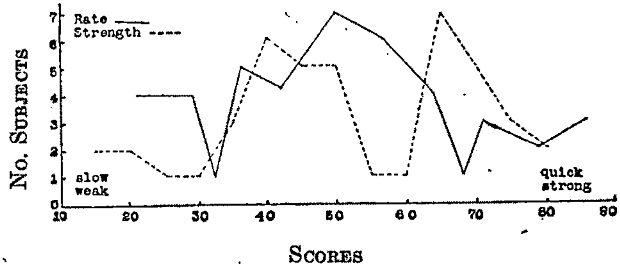


Fig. I. Frequency Curves for Composite Experimental Rate and Strength Scores. Scores by Percent Method

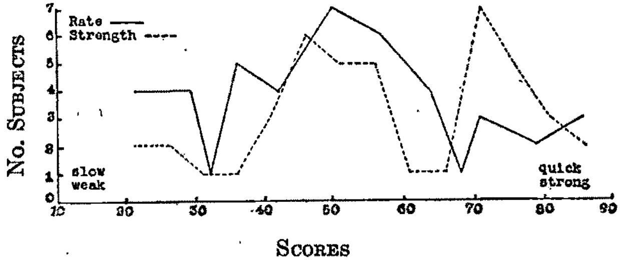
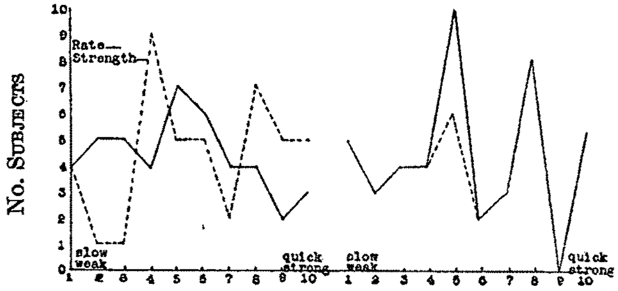


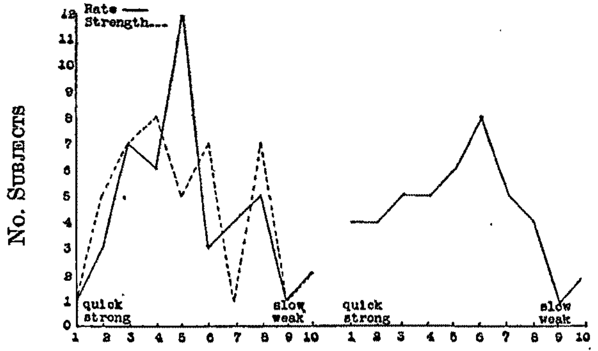
Fig. II. Frequency Curves for Composite Experimental Rate and Strength Scores with Strength Curve shifted so that the Ranges Coincide

If we assume that a common rate factor and a common strength factor may exist, the question then arises whether there will be found any grouping of the *Ss* with respect to these traits. Frequency curves for the composite rate, composite strength, and a combined rate and strength score are given in Figs. I, II, III, and IV. Those in Figs. I and II are plotted from the actual composite scores, whereas in all later curves the range of scores has been divided into 10 and the scores grouped accordingly. The range for the strength scores is just 6 points lower on the scale of 0–100%, therefore the distributions given in Fig. I were plotted in Fig. II with the strength scores all shifted 6 points up on the scale, so that the position of the high and low points of the two curves might be compared. The strength curve is unquestionaby bi-modal; the rate curve can scarcely be so considered, though it does rise to a lesser extent where peaks are found in the strength curve and falls to points equally low at the lowest places in the strength curve. The agreement between the curves is sufficient to suggest that a single factor might be influencing both the rate and strength aspects of the experiments. There is no evidence of this however in the coefficient of correlation for these two composite scores; it proves to be −.0025. Fig. V is a graphic representation of this zero correlation.



(A) Scores (B) Scores

Fig. III. A. Composite Experimental Rate and Strength Scores (Percent Method). B. Combined Scores, Rate and Strength



(A) Scores (B) Scores

Fig. IV. A. Composite Experimental Rate and Strength Scores (Av. R. Method). B. Combined Scores, Rate and Strength

In Fig. III-A the scores plotted are the same as those in Figs. I and II. They are, however, plotted as they fall into the 10 groups into which the entire range has been divided. The nature of the curves remains unchanged. For Fig. III-B each *S*'s rate and strength scores (percent method) were added and the total plotted. Curve B is definitely bi-modal. There appears to be a quick-strong group, and another group which a study of the 10 cases falling on the fifth abscissa shows to be composed of 6 *S*s with a low rate score and a high strength score that would classify them as slow-strong and 4 cases whose scores were both close to 50%. The line dotted between abscissae 4 and 6 indicates where the slow-strong group alone would fall. There are no quick-weak *S*s in this group. There is the suggestion in this curve of a possible 3rd group of slow-weak individuals.

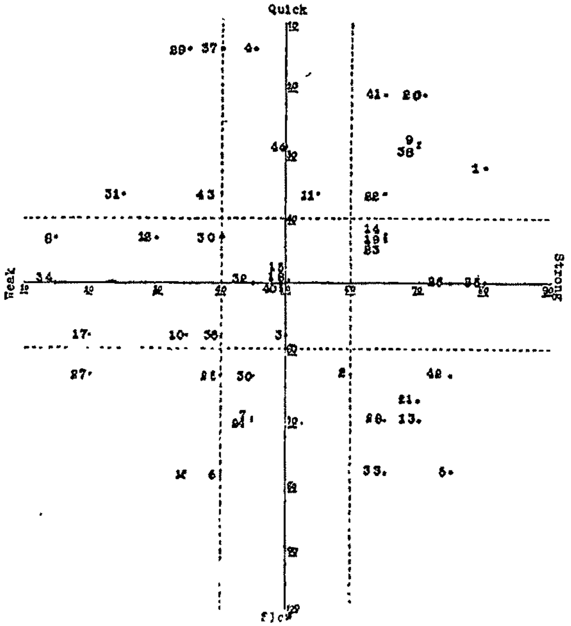


FIG. V. GRAPHIC REPRESENTATION OF THE LACK OF CORRELATION BETWEEN RATE AND STRENGTH SCORES

The scores plotted in Fig. IV, A and B, are the composite scores found by the average rank method, and are plotted as in Fig. III. The separate rate and strength curves have much the same form, but the curve for the combined scores loses its bimodality. As has been said before, we do not consider this

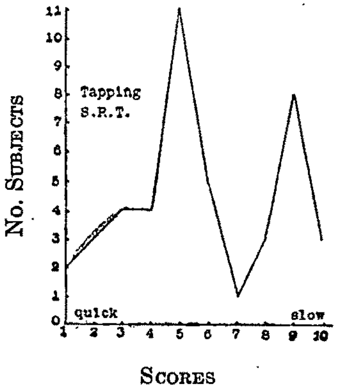


Fig. VI. Composite Score for Experiments where Rate is only Factor

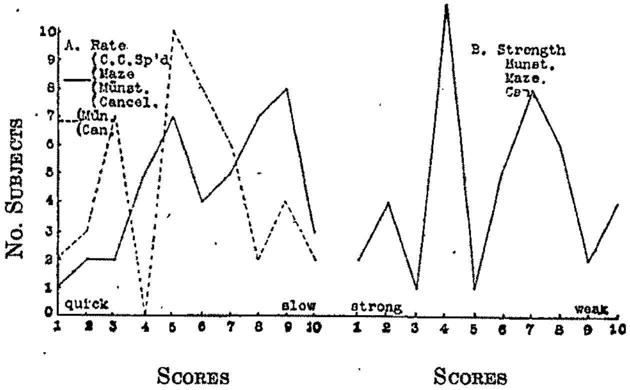


Fig. VII. Composite Scores for Experiments with Equal Stress on Speed and Accuracy

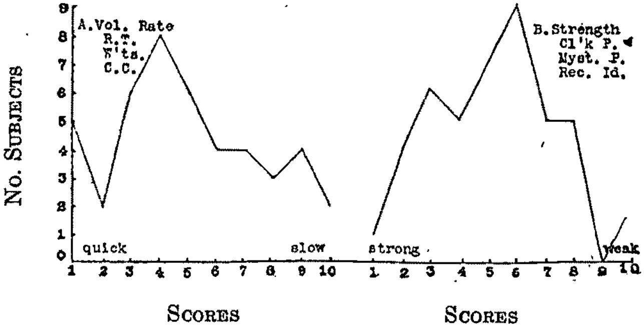


Fig. VIII. Composite Scores for Experiments with Stress either on Rate or on Quality

method of computing the composite scores as satisfactory as the percentage method, since undue weight may be given to an extreme score opposed to the general tendency of *S*'s performances, when this reversal may have been caused by a sudden shift in attitude due to extraneous factors.

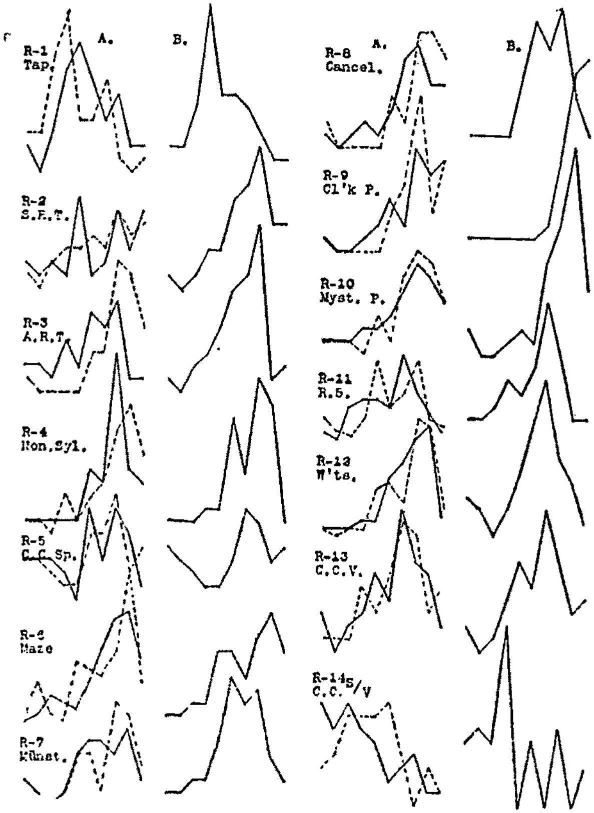


FIG. IX. FREQUENCY CURVES FOR THREE SETS OF SCORES FOR EVERY RATE EXPERIMENT

We next broke up the composite scores into scores for the various types of rate and strength experiments. The average rank method was used to obtain these scores, since 4 was the largest number of experiments in any group. These frequency curves are given in Figs. VI, VII, A and B, and VIII, A and B. The curves both for rate and for strength in the experiments requiring an equal effort for speed and accuracy are unquestionably bi-modal; as is also the curve for the average score for tapping and simple reaction time. Bi-modality is much less marked in the curve for the experiments in which stress was laid on speed and is not present in the one for those where emphasis was on quality.

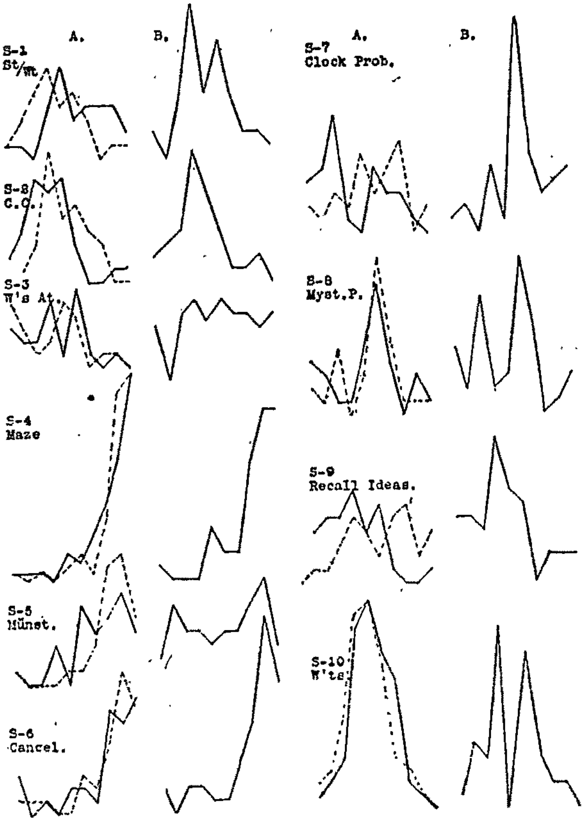
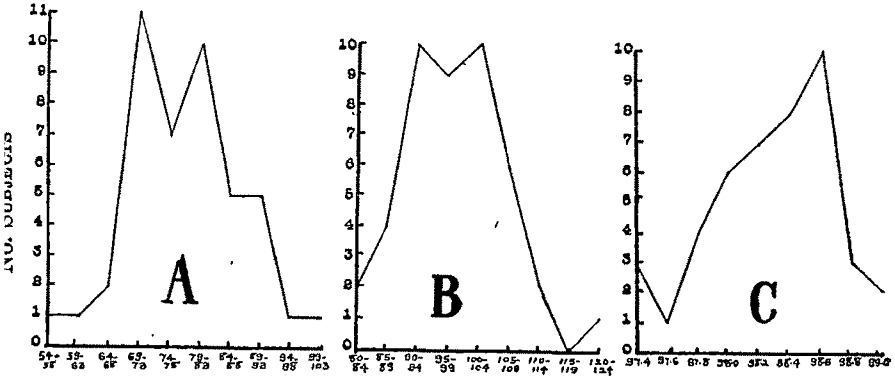


FIG. X. FREQUENCY CURVES FOR THREE SETS OF SCORES FOR EVERY STRENGTH EXPERIMENT

The search for bi-modality was carried further. We had three sets of scores for every experiment, the two sets from which the coefficients of reliability were computed and the final data. Curves were plotted for these 72 sets of scores to determine whether any experiment consistently yielded this form of curve. These curves are to be found in Figs. IX and X. The double curves in the A columns are from the scores obtained for the coefficients and those in the B columns from the final data. Of the 14 sets of rate curves the ones for *R-5* (carbon-copy speeded time) and for *R-14* (ratio of carbon-copy speeded to carbon-copy voluntary time) are the only ones which give even the slightest indication of bi-modality in all 3 curves for any experiment. Inspection of the 10 sets of strength curves yield similar results. The set for *S-8* (mystery problem) is the only one where more than one mode can be considered to exist in all of the curves.

The results which have been studied in the form of coefficients of correlation and in frequency curves cannot be said to show definitely that there is neither a common rate nor a common strength factor. On the other hand they do not by any means indicate conclusively that such factors exist; they suggest rather that there may be such a possibility.



PULSE RATE BLOOD PRESSURE TEMPERATURE

FIG. XI. FREQUENCY CURVES FOR PHYSIOLOGICAL MEASURES: A. PULSE RATE; B. BLOOD PRESSURE; C. TEMPERATURE

Curves for the physiological measures are given in Fig. XI. Neither the blood pressure nor the temperature curves give any indication of types. The curve for the pulse rate measures may be considered bi-modal and suggestive of a group of individuals with an unusually slow pulse in addition to the group whose rate agrees with what is accepted as normal. When the pulse rate for the cases scoring highest or lowest in the rate experiments is studied an unexpected relation appears to be present in spite of a coefficient of correlation of .098. Seven of the 8 *S*s receiving the highest experimental rate scores have an average pulse rate below 79, which is the average for all 44 cases, and 24 of the 31 measures for these *S*s are below the average, while 5 of the 8 with the lowest rate scores have an average pulse rate above 79 and 21 of the 32 measures are above it. It may be that the high pulse rate in these cases is an indication of an excited condition which interfered with success in the experiments, especially with respect to speed. On this point it is interesting to note that the coefficient of correlation between pulse rate and rate of emotional arousal is .303; this is not high, but it is suggestive. The only *S* (BeC) who has a high pulse rate with a high experimental rate score does fall down in her strength score. Among the 5 with high pulse rate but low experimental rate scores, 4 also have low strength scores; of the 3 who have low pulse rates and low experimental rate scores, all did well in the matter of strength. A similar study with respect to blood pressure and temperature measures, and experimental scores, does not disclose any hint of a connection between them.

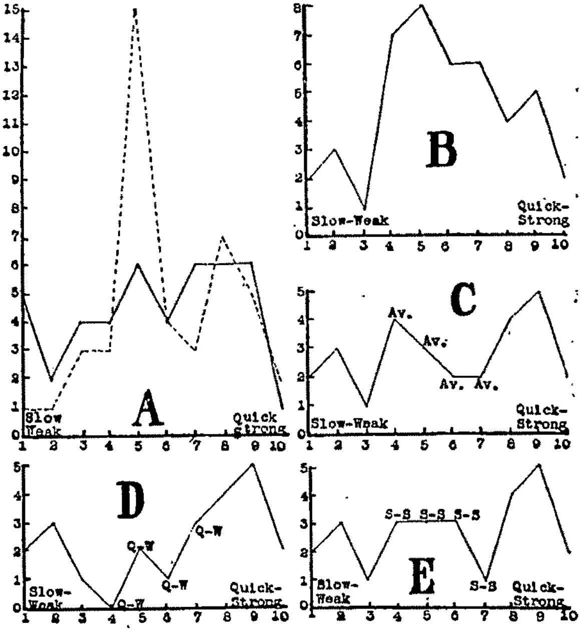


FIG. XII. QUESTIONARY: COMPOSITE RATE AND STRENGTH SCORES

A. Rate and Strength scores given separately.
B. Rate and Strength scores combined.
C. Average cases.
D. Quick-Weak cases.
E. Slow-Strong cases.

C, D, and E are plotted from the scores used in B. In them the modal group has been broken up into Quick-Weak and Slow-Strong cases and cases average in both traits. The rest of the curve remains unchanged. The *abscissae* represent the *scores*, the *ordinates* the *number of subjects.*

The frequency distribution of the composite rate scores and composite strength scores obtained from the questionaries is given in Fig. XII. The curves in part A are for these two sets of scores. Again the strength curve is more definitely bi-modal than the rate curve; the latter has 3 peaks of nearly equal height, suggesting a slow, an average, and a quick group. When the two composite scores are combined we have curve B which indicates a small slow-weak group, a larger quick-strong group, and a still larger middle group composed of the quick-weak and slow-strong cases and those average in both traits. The cases in this center group were sorted out and the next 3 curves plotted to show their distribution. In curve C only the cases average in both traits appear, in D the quick-weak, and in E the slow-strong cases; all are to some extent tri-modal. The coefficient of correlation between the composite rate and strength scores is −.107.

The degree of correlation existing between the other tendencies investigated by the questionary is as follows:

Steadiness and Composite Strength Score (Quest.) = −.557
Activity and Composite Rate Score (Quest.) = +.626
Emotional Intensity and Composite Strength Score (Quest.) = +.192
Emotional Arousal (Speed) and Composite Rate (Quest.) = +.788

When we come to investigate what relation exists between the experimental, physiological, and questionary results, we find that for the most part the correlations are negligible. See Tables VIII and IX for most of the correlations between physiological measures and experimental scores. Others, and those between the physiological measures and questionary results, are given below:

Blood Pressure Range on First Day and Comp. Exp. Rate Score = +.275
Blood Pressure Range on First Day and Comp. Quest. Rate Score = +.164
Pulse Rate and Comp. Quest. Rate score = −.048
Average Blood Pressure Scores and Comp. Quest. Strength Score = +.019
Pulse Rate and Rate of Emotional Arousal = +.303

To make a statistical study of the relation between questionary measures of rate and strength and the experimental scores, we wished to obtain as many correlation coefficients as possible between measurements secured by these two methods. We selected first the combinations most likely to give a correlation, next we used the experiments with the highest coefficients of reliability. When 232 of the possible 555 combinations had been plotted and partially worked out and 54 of the 119 rate and 35 of the strength coefficients had been found, the results were such that it did not seem profitable to go further with this undertaking. The extreme coefficients for the rate and strength groups as far as they were completed are as follows:

Quest. 2. (Speaks rapidly) and *R-11* (Reading Time)........ = +.355
Quest. 22 (Quick to note changes) and *R-10* (Myst. Prob.)... = +.280
Quest. 3 (Works rapidly) and *R-1* (Tapping)............... = +.256
Quest. 13 (Quick in mental processes) and *R-3* (Assoc. R. T.). = +.228
Quest. 13 (Quick in mental processes) and *R-9* (Clock Prob.). = −.250
Quest. 2 (Speaks rapidly) and *R-9* (Clock Prob.)........... = −.245
Quest. 2 (Speaks rapidly) and *R-3* (Assoc. R. T.)........... = −.242
Quest. 47 (Acts impulsively) and *R-6* (Maze)................ = −.210
Quest. 56 (Self-confident) and Comp. Exp. Strength Score.... = +.320
Quest. 37 (Intense likes and dislikes) and *S-2* (Carbon-Copy). = +.262
Quest. 10 (Dominates conversation) and *S-2* (Carbon-Copy).. = +.262
Quest. 4 (Works accurately) and *S-7* (Clock Prob.).......... = +.262
Quest. 41 (Calm in emergencies) and *S-7* (Clock Prob.)....... = +.252
Quest. 46 (Perseveres in difficulties) and Comp. Exp. Strength Score...................................... = +.236
Quest. 23 (Notices details) and Comp. Exp. Strength Score.... = −.223
Quest. 34 (Exerts self-control in emotion) and Comp. Exp. Strength Score........................... = −.219
Quest. 15 (Appreciates humorous sayings) and Comp. Exp. Strength Score........................... = −.207

Other coefficients found between composite experimental or questionary scores and scores for groups of experiments or groups of questions are given below:

Comp. Rate Score, 14 exps., and Comp. Quest. Rate Score..... = +.061
Comp. Rate Score, 8 exps., and Comp. Quest. Rate Score...... = +.052
Comp. Strength, 10 exps., and Comp. Quest. Strength Score.... = +.022
Comp. Strength Score, 7 exps., and Comp. Quest. Strength Score. = +.041
Comp. Quest. Rate Score and Tapping (speed only)........... = +.086
Comp. Quest. Rate Score and Av. R. in Tapping and Simple Reaction Time (speed only)........................... = +.079
Comp. Quest. Rate Score and Av. R. in Carbon-Copy Sp'd, Maze, Münst., and Cancel. (speed and accuracy)......... = −.114
Comp. Quest. Rate Score and Av. R. in Münst. and Cancel. (speed and accuracy).................................. = −.040
Comp. Quest. Rate Score and Av. R. in Reading Time, Judg.. W'ts. Carbon-Copy Vol. (voluntary rate)................ = +.227
Steadiness and Comp. Exp. Strength Score................... = +.077
Activity Score and Comp. Exp. Rate Score .... = +.092
Emotional Intensity and Comp. Exp. Strength Score.......... = −.207
Emotional Intensity and Comp. Exp. Rate Score............. = −.068

The negligible degree of correlation, in the case of both rate and strength, between experimental and questionary scores may be expressed differently. We find that 7 cases receive the same classification for both traits by the experimental and questionary scores; in 12 instances the classification is not changed to the opposite one for either trait, but one or more of the four scores are just average; in 18 cases either the rate or strength classification by one set of results is opposed to the classification by the other set, and in 7 cases both rate and strength classifications by one set are contradicted by the other.

The statistical treatment of the interrelation of results was supplemented by a study of the two extreme cases from each of the four groups, based on the experimental score. The cases are numbers 1 and 20 from the quick-strong group, 29 and 37 from the quick-weak, 5 and 23 from the slow-strong, and 16 and 27 from the slow-weak group. (See Fig. V for the grouping of the cases.) In this study we sometimes find a striking similarity between experimental results and parts of the questionary results, but equally often a marked dissimilarity, and at no time is the agreement perfect. The fact that of the 16 cases rated as definitely weak by the questionary scores only 4 are also rated slow, and that only 4 of the 21 whose scores classify them as slow are also considered definitely weak, suggests that those who filled out the questionaries may have been loath to give low ratings for both types of traits, but tended rather to balance them. Four of the case studies, one from every group, are given in illustration:

Case 1. A. An experimental rate score of 68, an experimental strength score of 80, a questionary rate score of 89 and a questionary strength score of 69 give this *S* a classification of quick-strong on a basis of both sets of results. In Questionary C she receives a rank above the average rank of the 44 cases in each of the rate traits except for rate of talking, which is just average. In the 29 traits taken as suggestive of strength her rank is above the average in 20 instances; in the other 9 she is very close to the average except in the control of her emotions and calmness in emergencies. She appears to be quick physically, mentally, and emotionally, to be forceful, energetic, and dominating, to be a steady worker, resolute and fixed in her opinions, but to be uneven in mood or disposition and to have changing antipathies and sympathies. She is ranked approximately average for optimism but below average for good-naturedness and slightly below in tendency to praise. This is suggestive of the unpleasant feeling tone which Ebbinghaus, Kreibig, and Meumann[51] among others attribute to this temperament. Another indication of this feeling quality is found when we consider the values assigned to the 35 activities listed in Questionary D; these values indicate the amount of pleasure derived from participation in the activities. To 5 activities she assigns the value which is average for the 44 *S*s, to 12 she assigns a value above the average, but one below average to 18 of them. From 11 of these 18 she derives no pleasure at all; even displeasure from 8 of them. Contrary to what we should expect of persons in this group, the data here for the most part show a disinclination for activities involving much physical exercise, also distaste for competitive games either physical or mental. This may be partly explained by a record at the University Health Department which states that her home physician advised against gymnasium because of a heart condition; this weakness may be a factor in her dislike of exercise. However, we find that in Questionary C her total score for the tendency to be active is +4, where the range of scores for all 44 cases is −6 to +6 and both average and median is 0. When we consider the next part of Questionary D, we find that *S* reports that she is a sound sleeper, that the number of nights during which she dreams is 25% where the average for all *S*s is 18.6%, but that she dreams only 5% of the average night while the average for the 36 *S*s who report any dreaming is 34.5%. She estimates her pleasant dreams at 50%

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[51] W. Stern, *op cit.*, 483 f.

and her unpleasant at 25%. In our search further for evidence of nervousness we find little; she states that she very seldom bites her nails now although she often did between the ages of 4 and 12 years, that she seldom rocks while in a rocking chair, but does much of the time swing her free foot when sitting with her knees crossed. There appears in general to be fairly good agreement throughout the data on this case.

Case 29. M. Experimental scores of 86 and 35 classify *M* as quick-weak; questionary scores of 64 and 86 change the classification to quick-strong. Though the ratings are not usually very much above the average for the trait, she is considered quick in most respects; exceptions are slowness in speech and slowness to anger and impatience. When we consider the strength questions, we find that the only weak points stressed are the ease with which she is persuaded and the mild intensity of her anger. Most of the traits in this group are rated unusually high and picture her as an accurate and steady worker, energetic, industrious, resolute, self-controlled, calm in emergencies, self-confident, enthusiastic, and given to intense likes and dislikes. Optimism and a pleasant feeling tone are assigned to the quick-weak temperament, and this *S* is rated high in them. This characteristic is suggested also by her record in Questionary D, for she gives a value above average to 25 and one below average to only 4 of the activities; she derives the greatest amount of pleasure from 16 activities, and no pleasure at all, or in this case displeasure, from only one. She enjoys both physical and mental activity. *M* reports that she is a light sleeper, dreams about 75% of every night and that half her dreams are pleasant, the other half being equally divided between neutral and unpleasant. Though she often bites her nails she seldom rocks and never swings her foot. Her record at the University Health Department contains a report of "nervous trouble, cried, little things bothered." In the daily questionary she made notes on the first and fourth days that she was "nervous," but her low strength scores were not confined to those days. The data on this case are throughout not in harmony; the ratings on the strength traits are opposed to the evidence gained from all other sources.

Case 33. P. Slow-strong is the classification earned by experimental scores of 21 and 65 and slow-average by questionary scores of 29 and 48. Only in talking and working is *P* considered really quick. She is thought slow in her movements and mental processes, slow to anger and in the arousal of other emotions. The outstanding strength values indicate that she is a steady worker, resolute and courageous, and exerts great self-control in showing her emotions, but that she is inclined to be mentally indolent and inaccurate in her conclusions and opinions, to become only mildly angry and to be indifferent toward people and things. Instead of the tendency toward pessimism and unpleasant feeling tone expected of persons in this group, we find here an inclination to be cheerful and happy, to praise and be optimistic, although in contradiction to this she is also rated as habitually anxious and worried. The values assigned by her to the activities do not give evidence of a definite tendency in either direction; she gives a value above average to 15 but one below average to 13, derives the greatest pleasure from 8, but no pleasure at all, and in one instance displeasure, from 7. She claims to be a sound sleeper, to dream during few nights (5%), and not to any great extent (10%) at those times. Her dreams are chiefly neutral and only 5% are unpleasant. She also seems to be troubled little by nervous habits, for she never bites her nails, though she often did when about 10 years of age; she rocks little of the time that she is seated in a rocking chair, and seldom swings her free foot when her knees are crossed. Again, the data from various sources do not run parallel throughout.

Case 16. F. An experimental rate score of 21 and experimental strength score of 35 classify *F* as slow-weak. Her questionary rate score of 7 agrees with this; her questionary strength score of 48 places her with the average group. Most of the values for the rate traits are well below the average and those for the strength traits close to it. She is considered slow in every way, careful, accurate and steady in work; to feel losses for a long time, to be resolute, perserveing and self-controlled, but to lack force and energy, to have changing likes and dislikes, to be easily persuaded and wanting in self-confidence. We should expect persons in this group to give evidence of pleasant feeling tone. This is true in her case for she is described as good natured, cheerful and happy though inclined to alternate in mood; as given to praising and laughter, and as optimistic. There is further evidence of this tendency in the values assigned to the activities, for she gives a better than average score to 15 and a score below average to only 7. To 11 of the 15 she gives the highest possible score, while she claims to derive no pleasure or even displeasure from only 3. Activities involving either physical or mental exertion are scored high, although her total score for activity in Questionary C is −4; the latter is more in keeping with what one would expect of a person classified as weak and slow. *F* reports that she sleeps soundly, dreams during about one night in 4 and only about 25% of those nights; 50% of her dreams are pleasant and 25% neutral. She apparently does not have any of the common habits suggestive of nervousness, for she very seldom bites her nails, seldom rocks, and very seldom swings her foot. The conservative judgments concerning most of the strength traits weaken an otherwise close agreement between the various sets of data.

IV. CONCLUSIONS

(1) There is a slight indication of the possibility of a common rate factor in the experiments in which speed and accuracy were equally stressed and in those where the rate was voluntary.

(2) A common strength factor is suggested to a slight degree by the results of the experiments in which speed and accuracy were stressed equally and in those where the emphasis was on the quality of the performance.

(3) The 44 cases do not fall into four sharply defined groups on a basis of the experimental scores, but at the same time there do seem to be a few cases who are consistently quick and are either consistently strong or consistently weak, and others consistently slow, who are fairly consistent in their strength or weakness.

(4) The physical measures give practically no evidence of types and do not in general appear to be consistent with the experimental results. An exception to this is the negative relationship between the extreme experimental rate scores, both high and low, and pulse rate. We suggested above that this may be indicative of an excited state which interfered with the performance of the experiments.

(5) The questionary rate and strength scores also do not divide the *S*s into distinct groups, but again, as with the experi-

mental scores, there appear to be some cases who are consistent in the matter of both rate and strength.

(6) The agreement between the experimental and questionary classifications is slight enough to be accounted for by chance.

(7) In general we must conclude that although there may be general rate and strength factors as we have defined them, still, in so far as the questionaries yield evidence concerning temperament, these factors do not seem to be related to temperament.

THE UPPER LIMIT OF AUDITORY LOCALIZATION

By H. M. Halverson, Yale University

Investigators in sound localization agree that high tones, above 800 d.v., are localized with difficulty; *i.e.* that accuracy in judgments of position of a tone decreases with increase in its frequency.[1] There is also general agreement that the tones move over a more limited space with differences of phase.[2] There is, however, little agreement as to the frequency at which the lateral movement of the tone ceases to function.[3]

History of the Problem

Rayleigh[4] (1907) got good right and left displacements of the tonal image with a 512 d.v. tone, fair lateral shifts with a 640 d.v. tone, and rather poor results with a 768 d.v. tone. Wilson and Myers[5] (1908) using the König steel bars were unable to obtain changes in localization in the case of high tones. Bowlker[6] (1908) found for frequencies of 340 d.v. and less that lateral displacements of the tone were as great as 90°. The lateral displacement of a 490 d.v. tone, based on his figures for the so-called "cross-over"[7] angle, was 42.5°. For frequencies greater than approximately 700 d.v. the maximal displacement was "roughly proportional to the wave-length." At 2310 d.v. the displacement was 7°, while at 3100 d.v. he believed the maximal displacement to be 5-6° (not observed). The law broke down for frequencies greater than 3100 d.v.; the displacement for 6770 d.v., for example, was greater than the law would allow. Bowlker's results are shown in Fig. 1. More[8] (1909) stated that 512 d.v. was about his limit for accuracy in localization, that for higher frequencies there was a decrease in accuracy, and that at 1024 d.v. his judgments were not reliable. Brief trials convinced More of the impossibility of localizing direction with a 3000 d.v. tone. Stewart[9] (1920) probably obtained movements at 1280 d.v., but none at 1536 d.v. Hornbostel and Wertheimer (1920)[10] stated that with tones higher than about 800 d.v. the lateral movement no longer extended to 90° right and left but decreased with the frequency. At about 17,000 d.v. the lateral movement should be reduced to nil, *i.e.* the tone should be heard in the median plane of the head. Banister[11] (1925) secured movements for 1040 d.v. and 1345 d.v., although the results were of anomolous character. In substance he agreed with Stewart[12] that movements of the tone occurred for frequencies up to about 1400 d.v.

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[1] See Rayleigh, More, Wilson and Myers, Bowlker, Stewert, Banister, and Boring, referred to below.

[2] Banister takes exception to this rule. He doubts the validity of both Bowlker's and Stewart's conclusions that greater lateral movements of the tone occur for lower frequencies than for higher frequencies. H. Banister, *Brit. J. Psychol.*, 15, 1925, 305.

[3] E. G. Boring, this Journal, 37, 1926, 173.

[4] Lord Rayleigh, *Phil. Mag.*, 6 ser., 13, 1907, 228-229.

[5] H. A. Wilson and C. S. Myers, *Brit. J. Psychol.*, 2, 1908, 368.

[6] T. J. Bowlker, *Phil. Mag.*, 6 ser, 15 ,1908, 323-327.

[7] Bowlker, *op.cit.*, 323.

[8] L. T. More, *Phil. Mag.*, 6 ser., 18, 1909, 314.

[9] G. W. Stewart, *Phys. Rev.*, 2 ser., 15, 1920, 437.

[10] E. M. von Hornbostel and M. Wertheimer, *Sitzungsber. d. preuss. Akad. d. Wissensch.*, 20, 1920, 395.

[11] Banister, *op.cit.*, 291-292.

[12] Banister, *op.cit.*, 305.

Hartley[13] (1919) plotted theoretical curves to show the angular displacement for any given difference of phase for tones of the following frequencies: 310, 620, 930, 1240, and 1860 d.v. According to these curves tones of higher frequency than about 650 should have a maximal movement of less than 90° from the median plane. At 1860 d.v. the movement should be about 27°.[14] He pointed out that his curves corresponded fairly closely with Bowlker's findings.[15]

We are then forced to the conclusion that for frequencies below 600-800 d.v. displacements may extend to the extreme lateral position.[16] Movement becomes more and more restricted for higher frequencies until we reach a point (frequency) above which the sound remains fixed in the median plane. We have seen that Stewart and Banister placed this point between 1200 and 1500 d.v. Hartley and Bowlker refused to commit themselves, but their data placed it above 7000 d.v., and Hornbostel and Wertheimer as a result of their investigations with time differences gave 17,000 d.v. as the approximate frequency at which movement ceased. It was for the purpose of throwing light on this problem that the investigation herewith reported was undertaken.

Apparatus and Procedure

The precise object of the experimentation then consisted in finding the maximal amount of lateral movement which is obtainable from each of a number of tones varying in frequency from 600 d.v. to 17,000 d.v. The method of procedure varied in accordance with the difficulty in making localization.[17] The following persons served as observers: Mr. Harold E. Pressey (P), graduate student in psychology; Mr. John M. McGinnis (M), instructor in psychology; and the writer (H). Neither P nor M had had previous training in auditory localization; H was trained.

For the first three frequencies, 600, 1000, and 1400 d.v., we used electrically driven audio-oscillator forks.[18] The fork was put in a soundproof box and placed in a closet. An iron T-tube, inside diameter of ⅜ in., was placed so that the stem of the T lay perpendicular to the outside face of one of the prongs. A thin piece of tin of the width of the diameter of the T-tube had been driven symmetrically into the stem so that the latter was divided into two equal parts as far back as the top of the T-tube. Finally, the inside of the T-tube was smoothed out by means of melted wax. This preparation insured against interference of sound waves from one tube to the other in the two rubber tubes (½ in. inside diameter) leading from the branches of the T-tube to the stethoscopic binaurals in a second room. We had marked off on a wall-board lying on a table a large semi-circumference of a radius of 70 cm. upon which were erected rectangular wooden posts, which with their appropriate numbers were placed at intervals of 5° on the semi-circumference and served as localizing cues for the *O*, who, with the binaurals in his ears was seated at the center of the circle. One of the rubber tubes in the second room was cut in two, and two glass tubes, each about 125 cm. in length and one telescopically containing the other, were inserted in the gap, so that the length of this conductor could be varied by sliding the inner tube in or out of the larger. The larger tube, with a meter stick fastened parallel to it, was secured to the table. The open end of the inner tube served to indicate the position which was to be used for observation. Eight (for 1400 d.v., seven) positions, previously selected to offer the best opportunity for observing changes in localization accompanying differences in phase, were used in the case of the three frequencies above mentioned. The observer took his position, and, with the fork sounding, the apparatus was set for one of these positions. The *O* placed the binaurals to his ears, localized the sound, removed the binaurals and gave his judgment verbally to *E*. The stimuli were presented in chance order with no repetitions. Under the scheme adopted, *O* was presented with an auditory stimulus and directed to report its position in terms of degrees right or left of the median plane of the head. It developed under practice that while the positions on the wall-board were indicated in multiples of 5's, the *O*s were content to render judgments in terms of these multiples at the extreme lateral positions, but they resorted to finer judgments whenever the localizations were within 45° of the median plane.

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[13] R. V. L. Hartley, *Phys. Rev.*, 2 ser., 13, 1919, 373-385.

[14] Hartley, *op.cit.*, 380. [15] Hartley, *op. cit.*, 384. [16] Boring, *op. cit.*, 173.

[17] Boring, *op. cit.*, 172-173.

[18] Purchased from General Radio Co., Cambridge Mass. These forks emit a remarkably pure tone.

For frequencies above 1400 d.v. we replaced the electrically driven forks by an electro-magnetic ribbon transmitter.[19] This source of sound was a phosphor bronz ribbon under tension longitudinally in the magnetic field of a strong permanent magnet from a Weston ammeter. The transmitter was energized by a Hartley oscillator[20] the parts of which consisted of a Leeds and Northrup Brook's Inductometer with a maximum self inductance of 0.250 henry, a Leeds and Northrup three dial mica condenser with a maximum reading of 1.11 microfarads, and a Western Electric 205-B vacuum tube operated on a plate potential of 120 volts. The transmitter was put in the sound-proof box with the T-tube so situated that its stem rested immediately above the ribbon. We reduced both arms of the tube circuit leading from the T-tube to the binaurals to 40 cm. each because we found that the tubes rapidly absorbed the high tones, and replaced the glass tubes by telescopic rubber tubing (the inside diameters of which were ½ and 9/32 inches), because the shortness of the arms necessitated the use of flexible conductors. For frequencies of 10,000 d.v. and higher the conductors were reduced in length to 25 cm. For frequencies above 10,000 d.v. each arm was but 25 cm. in length. We also replaced the wall-board design by a wall schema consisting of pieces of cardboard appropriately numbered and placed on the wall before *O*. These numbers now served as the localization clues. The positions used for purposes of localization were indicated on the inner of the telescopic tubes, which had previously been marked off in half-centimeters.

For frequencies below 3500 d.v. the *O*s followed the directions outlined above in making their localizations, with the exception that they kept the binaurals in the ears in order that they might better follow the movement of the sound. This is a necessary precaution, for, with all high frequencies, two images are usually present throughout the experimentation. For frequencies of 3500 d.v. and above the *O*s with the binaurals constantly in the ears stated merely the extreme lateral displacement of the sound. This later instruction for the very high frequencies was adopted because median localizations were practically precluded; the intensity of the sound at medial positions was reduced almost to nil.

RESULTS

Preliminary trials with each frequency showed that lengthening or shortening the telescopic arm of the apparatus an amount equal to one wave length of the frequency used produced all possible differences of phase (0°-360°).

While the main problem was not concerned with *O*'s ability to localize sound for various differences of phase, observations on the eight positions used for the three lower frequencies gave the *O*'s excellent training for the experiment and served to show the reliability of judgments of position of sounds at these frequencies. The results for each *O* for the three lower frequencies are shown in Table I.

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[19] The transmitter, designed by Dr. A. L. Fitch, of the Dept. of Physics, University of Maine, will be described in a forthcoming number of the *Physical Review*.

[20] H. J. van der Bȳl, *The thermionic vacuum tube*, 1920, 282-284.

There is fair agreement among the *O*s on their localizations for the three frequencies. The mean variations and the number of judgments show that the difficulty experienced in judging direction increases with the amount of displacement of the tone from the median plane of the head.

To assure ourselves that the judgments recorded in the table are those for the image selected for experimentation we instructed the *O*s to keep the binaurals at the ears and observe the movement of the image while the telescopic tube was lengthened and shortened. The *O*s experienced no difficulty in following the passage of the image across the median plane to its extreme lateral position. It was during this procedure that the *O*s discovered that the image, observed in movement, attained more extreme lateral positions than those positions obtained by observations taken while the image was at rest. For example, the image for 600 d.v. was observed by all *O*s to describe a complete arc of 180° about the head in front. For 1000 d.v. and 1400 d.v. the arcs were respectively about 110° and 70°. Double images were of frequent occurrence and were recorded, but are not shown in the table. For frequencies above 1400 d.v. the main purpose of the experiment was to determine the maximal displacement of the sound image from the median plane of the *O*'s head for varying differences of phase of the frequencies. The *O*s were instructed to keep the binaurals at the ears (removing them only at rest periods) while the experimenter made adjustments of the tubes preceding their judgments of position. This procedure enabled the *O*s to observe the image in motion before it came to rest at the position for which the judgment was given. Inasmuch as the moving image is more easily observed than is the image at rest, the *O*s had greater confidence in the accuracy of their judgments of its position. They caught the image while it was moving, followed it until it reached its most lateral position and then reported this position. In this manner 25 judgments were taken for each side. The quantitative results are shown in Table II.

In general it may be said that as the vibration frequency increased, the tone moved through a more limited region about the median position. This decrease in the amount of lateral displacement of the tone was rapid at first and then less rapid as we approach the very high frequencies, when the amount of displacement remained fairly constant. From our observations

TABLE I

The frequencies are indicated at the top of the table. Each cell contains the localization in degrees for the several positions on the cm. scale (top row of figures) of the inner telescopic tube, the m.v. in degrees, and the number of judgments made on the image out of a total of 40 trials. For each frequency the localizations in the four left columns are all to the *O*'s left; the others are to his right.

		600 d.v. Cm. on scale								1000 d.v. Cm. on scale								1400 d.v. Cm. on scale						
O		60	51	49	40	26	15	6	1	50	45	40	35	30	25	20	15	52	48	44	40	36	32	28
P	Local: deg.	80.1	75.8	39.2	6.0	19.2	36.9	76.0	85.3	36.7	27.3	20.5	8.9	7.0	21.1	32.8	45.5	3.0	8.2	10.1	8.1	4.4	13.8	23.0
	m.v.	8.5	8.2	10.1	4.9	6.8	7.2	8.3	3.5	6.5	5.3	7.1	5.4	3.7	6.3	6.8	8.8	0.0	2.2	2.2	2.5	3.0	3.2	6.8
	No. jdgmts.	31	38	40	40	40	40	36	33	38	37	40	40	40	40	40	31	1	16	34	32	34	38	40
M	Local: deg.	67.8	57.0	55.4	23.1	8.2	33.0	54.5	45.7	48.3	26.9	17.9	4.7	8.5	26.0	33.2	41.5	14.9	16.4	12.0	3.5	6.7	14.0	19.0
	m.v	11.1	7.8	6.7	5.2	3.4	5.6	11.0	8.1	5.4	6.1	4.3	2.4	2.1	5.5	6.4	8.1	4.6	3.7	4.1	1.7	3.8	2.5	2.8
	No jdgmts.	37	40	40	40	40	40	28	30	40	40	40	40	40	40	40	13	39	40	40	40	19	26	38
H	Local: deg.	90.0	82.9	76.3	22.9	0.7	18.6	55.1	72.0	58.4	30.1	23.4	13.2	0.3	12.5	23.3	48.5	20.3	31.4	20.7	12.8	1.7	14.4	21.0
	m.v.	0.0	8.6	5.2	4.7	1.2	3.1	6.9	8.5	4.3	4.1	3.8	4.6	2.2	3.5	3.1	7.7	7.7	4.6	5.8	4.4	4.3	3.4	4.5
	No jdgmts.	36	28	33	40	40	40	39	32	40	40	40	40	40	40	40	37	21	40	39	40	38	40	34

TABLE II

The frequencies used head the columns. The cells immediately below give the maximal displacements from the median plane in degrees. The third and fourth rows of cells show respectively the average maximal displacements and the average m.v.'s for all *O*s.

O	1800	2200	3000	3500	4000	5000	6000	7000	8000	9000	10000	11000	12000	13000
P	22.5	22.5	20.0	12.3	13.4	17.0	13.2	10.5	8.7	8.4	6.4	8.6	9.2	9.5
M	22.0	17.5	17.5	17.7	14.8	15.4	10.2	9.8	3.2	3.1	5.2	5.6	4.5	4.7
H	18.5	18.5	15.0	11.8	12.9	11.1	8.9	8.1	5.9	4.2	5.0	5.0		
Av	21.0	19.5	17.5	13.9	13.7	14.5	10.8	9.5	5.9	5.2	5.5	6.4	6.8	7.1
Av. m.v.	3.2	1.5	2.1	1.5	1.3	1.3	1.2	1.5	1.2	1.2	0.8	1.0	0.9	0.9

it would appear that displacements of the tones should fail to occur only at the upper limits of audibility.

The tones for the various frequencies were remarkably pure. We made no effort to determine the number of partials present or their prominence. When the sound was very intense, one or more partials were observed, but under the conditions of the experiment in which the weakest possible tones were used the observers were able only very occasionally to detect the presence of partials. When partials were noticeably present in the sound, a slight adjustment of the ribbon was sufficient to remedy matters. In any case the partials were so weak as to preclude any possibility of confusing them with the tone in question.

Confidence in the results of the experimentation was substantially increased, first, by the fact that all *O*s experienced very little difficulty in the localization of the tones of any frequency up to the point where the sound was no longer audible through the conducting tubes. For H this point was 12,000 d.v., for P and M, 14,000 d.v. Incidentally, H's upper limit of hearing with open ears by this apparatus was found to be between 14,000 and 15,000 d.v., while the upper limits for P and M were above 18,000 d.v. (Tones of these higher frequencies resembled closely those for very high frequencies as produced by the Galton whistle.) No effort was made to find the exact upper limits of the *O*s. Moreover, all through the experiment *E* was able to obtain every possible difference of phase by lengthening or shortening the telescopic tubes a distance equal to the wavelength of the particular tone being localized.

To show that localization of tones of high frequencies was not particularly difficult, we obtained additional judgments of direction for the frequencies 1800, 2200, and 3000 d.v., such as the median position, 5L, 5R, 10L, 10R, 15L, and 15R. The results, as measured by the cm. scale on the telescopic tube for 25 judgments on each of these positions, show considerable consistency in the judgment of direction both for the individual *O*s and among the three *O*s; the mean variation for any one *O* ranged between 2.6 cm. (for 1800 d.v.) and 0.5 cm.

Fig. I shows graphically for the many frequencies used the maximal amount of angular displacement of the tone from the median plane of the head toward one side; *i.e.* 60° means that the tone will sweep through 120° in the complete phase cycle, and 10° means it will sweep through 20° in the full cycle. The isolated dots give the average angular displacements for all *O*s for the various frequencies and the continuous curve is the best fitting line of the dots. If we assume continuity of function, the successive amounts of angular displacements may be read from the ordinate scale. Thus, the angular displacement at 600 d.v., the lowest frequency used, is 76.4°, that for 1000 d.v. is 47.2°, and so on. On the basis of the results obtained we may regard the smoothed curve as representing in general the form and amount of the function for the angular displacement of tones ranging from 600 to 14,000 d.v.

The dotted curve is the function obtained by Bowlker[21] and is based on the lateral displacements of the sound at the 'double image' point. Bowlker claimed that the displacement was greater[22] than his figures for the "double images" admit, and his curve should on this ground more nearly approximate our curve.

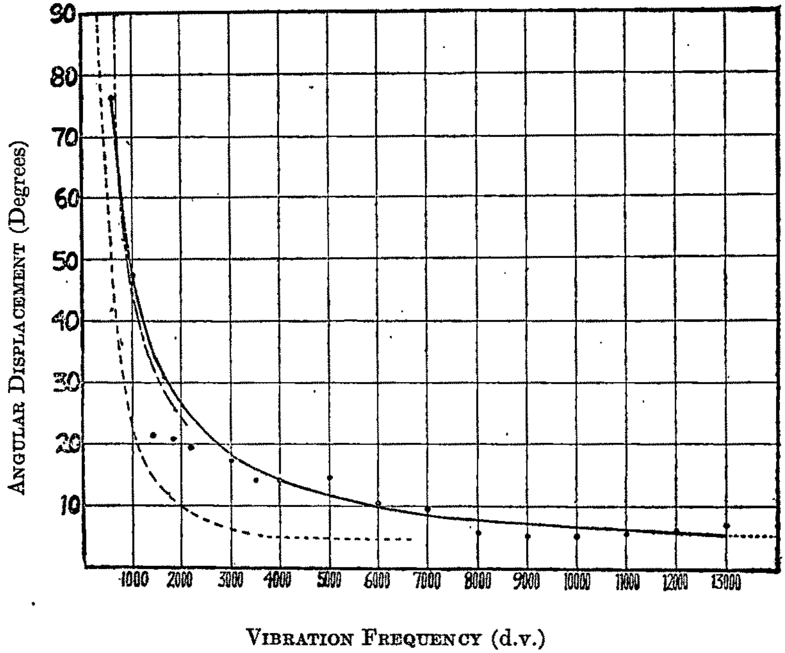

FIG. 1. MAXIMAL ANGULAR DISPLACEMENT OF LOCALIZATION OF DIOTIC TONE, WHEN PHASE RELATION IS ALTERED, AS A FUNCTION OF VIBRATION FREQUENCY OF THE TONE.

The ordinate is the maximal lateral angular displacement from the median plane toward one (or the other) side. The abscissa is the frequency of the displaced diotic tone.

The dot and dash curve is Hartley's function,[23] and is the result of calculations of the position of the acoustic image for varying differences of phase. We have already noted that observations made on the image while it was in motion gave it a greater lateral displacement, than when it was observed at rest. On this basis our curve should, like Hartley's, begin at 90° on the ordinate instead of at 76.4°.

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[21] *Op. cit.* [22] *Op. cit.*, 321-326. [23] *Op. cit.*, 376-385.

Most investigators agree that high tones are localized only with difficulty. In this connection, therefore, it is interesting to review the results and introspections of the observers for the various frequencies with which we experimented. The *O*s experienced no striking difficulties in localizing with the 600, 1000, 1400, 1800, and 2000 d.v. tones. They made fairly consistent judgments for five to eight different positions for those frequencies and their introspective reports indicate the existence of no particular difficulty. The fact that the 1400 d.v. was intense so that the ears 'rang' seems to be the most likely explanation for the slight difficulty experienced for this frequency. At 3000 d.v. the *O*s noted that median localizations were not as easily made as the more lateral ones at 10° and 15°. Four positions were localized at this frequency. From 3500 d.v. up but two positions were localized; that point at either side at which the tone lent itself most clearly for localization purposes. It will be recalled that the *O*s kept the binaurals at the ears for these frequencies and were directed to note the movement of the sound images. The introspections show that the image appeared at one side, became clear and definite, then moved in toward the median plane, where for frequencies between 3500 and 8000 d.v. it almost vanished while crossing the median plane, and then reappeared on the other side, became clear and definite and then gradually diminished in clearness until it disappeared. The diminution in the clearness of the image at the median plane was no doubt due to the decrease in the intensity of the sound at this position, for all *O*s agreed that at this median position the sound was scarcely audible. Localization with the image at rest in the median plane was declared impossible by the *O*s. For frequencies above 8000 d.v. the movements of the image were confined to a small horizontal region at each side of the median plane. Upon one occasion M reported a suggestion of movement across the median plane at 9000 d.v., but there is unanimous agreement by the *O*s that in general the sound was inaudible when the difference of phase of the sound at the ears was 0°. We are then led to believe that it is very difficult to localize in the median plane with tones between 3000 and 8000 d.v., and that median localizations are impossible with tones above 8000 d.v.

All *O*s were able to get right and left localizations for whatever tones were audible through the tubes to both ears. Both P and M got good right and left effects with all frequencies up to and including 13,000 d.v. At 14,000 d.v. the tone for P split up and faded away. It would appear, disappear, reappear, and 'ring' in the ear, but had no definite position. For M the tone was definitely localized, but seemed to spread out over a "number of degrees," and even this 'spread' shifted with a shift in

O's attention. At other times M spoke of the "image as very vague." At 17,000 d.v. neither P nor M could hear the tone through the tubes. H got good right and left localizations for the 10,000 d.v. tone, poorer results with 11,000 d.v., and failed to hear the tone through the conductors at 12,000 d.v. In this connection we must recall that throughout the experimentation weak intensities were used in order to keep the tones as pure as possible.

Double images[24] were observed in the case of all tones by all *O*s. The *O*s observed lateral movements of the image beyond the two-image point[25] when the phase difference was further increased, but inasmuch as these movements also occurred under conditions in which the phase-difference was not altered, it is not certain that fatigue is not a more important factor in producing these more lateral displacements of the tone than is difference of phase itself. Two additional facts seem to support this contention. In work with these very high frequencies, the *O*s were given frequent rest intervals. For instance, after two seconds attention to a tone of 9000 d.v., the image for no apparent physical reason would move either inward or outward slightly and then disappear. It might or might not reappear. Moreover, the tone after reaching the two-image point appeared to expand somewhat, and this expansion gave the impression of greater lateral movement. It appears then that the lateral movement of the tone beyond the two-image point is more illusory than real.

Bowlker reported[26] the presence of as many as three images for some of his higher frequencies, *viz.* 1290, 1675, 2090, 2310 d.v., and four images for 3050 d.v. Our *Os* never have reported more than two distinct images. Occasionally, before the tone was weakened for experimental purposes, the *O*s noted other right and left effects than those usually reported as images These lateral effects always disappeared when the desired intensity was obtained and were probably due to resonance in the tubes dependent on the intensity of the sound. The *O*s observed that the two images, when present simultaneously, at times gave the impression of drawing together and at other times appeared to be connected by an attenuated strand of sound, with the whole sound mass taking a dumb-bell form. It is quite possible that Bowlker by shifting his attention from the two distinct images to a point between these images 'bunched' this complete sound mass and thus gave it the appearance of a single image. As a matter of fact, Bowlker himself questioned the reality[27] of the third image, thinking it might be due to the "result of attention to the two side images."

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[24] Bowlker, *op. cit.*, 323. [25] *Ibid.* [26] *Ibid.*, 325 f. [27] *Ibid.*, 325.

CONCLUSIONS

(1) With changes of phase tones of high frequency move over a more limited area than do low tones. This decrease in the amount of movement is rapid at first and then less rapid for the very high frequencies.

(2) Localization, as far as observable right and left effects are concerned, apparently becomes impossible only at the upper limits of hearing.[28] For frequencies below 1400 d.v. the *O*s gave fairly consistent judgments of direction; for frequencies between 1400 and 3000 d.v. localization offers no particular difficulty, if proper allowance is made for fatigue; for frequencies above 3000 d.v. lateral localization effects are observable, but median localization, if not impossible, is extremely difficult to achieve.

(3) Greater lateral displacement accompanies varying differences of phase when tones are observed in motion than when they are observed at rest.

(4) Fatigue in localization of very high frequencies occurs rapidly.

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[28] It has just come to the writer's notice that C. A. Ruckmick (this JOURNAL, 37, 1926, 466) reports "unequivocal results for phase differences" up to 8000 d.v.

APPARATUS FOR THE CONTROL OF STATIONARY LIGHT STIMULI[1]

By DEANE B. JUDD, Cornell University

In an investigation of the Purkinje after-image, it became necessary to construct a special control apparatus. In the experimental work, which will be reported later, the apparatus proved so convenient and reliable a method of studying, not only the rapidly disappearing Purkinje after-image, but after-images in general, that a complete description of it is thought advisable.

The apparatus (see profile diagram, Fig. 1) is an adaptation of the rotary tachistoscope.[2] Light from source (A) is cut off by a rotating shutter or episcotister (F), and gives a periodic illumination to the lantern-slide (J). Recurrence of the illumination at a too rapid rate, *i.e.* at every revolution of the episcotister, is prevented by the action of the electro-magnetically controlled shutter (D), shown in Fig. 2. This constitutes the essential part of the apparatus; the accessory parts have been added to obtain certain other experimental advantages:

(1) A quick release and termination of the light pulse is obtained by placing the episcotister (F) in the focal plane of the condensing lens system (B). In this plane all of the light flux falling on the slide (J) is passing through an image of the filament of the source lamp (A) which is roughly ¼ in. high and wide. The episcotister (F) has only to travel, then, ¼ in. to make the complete change from darkness to maximum intensity of the stimulus and from maximum intensity to darkness.

(2) The range of intensity required in order adequately to study the Purkinje phase is made possible by the insertion of 5 diffusing screens in series (I), the arrangement being similar to that found in the Nagel adaptometer.[3] Continuous variation within the brightness step caused by the insertion of any single diffusing screen is accomplished by means of a neutral absorbing wedge similar to that used in annulus form by Abney[4] in his work on vision.

(3) The control of the surrounding field intensity independent of the stimulus intensity is accomplished by projecting the

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[1] From the Physical Laboratory, Cornell University.

[2] A. Goldscheider and R. F. Müller, *Zsch. f. klin. Med.*, 23, 1893, 134. Cf. also H. L. F. von Helmholtz, *Physiol. Optik.*, 514.

[3] W. Nagel, Zwei Apparate für die augenärtzliche Funktionsprüfung usw., *Zsch. f. Augenhk.*, 17, 1907, 201. Cf. also Helmholtz, *Physiol. Optik.*, English ed., II, 1924, 315-317.

[4] W. Abney, *Researches in colour vision*, 1913, 70.

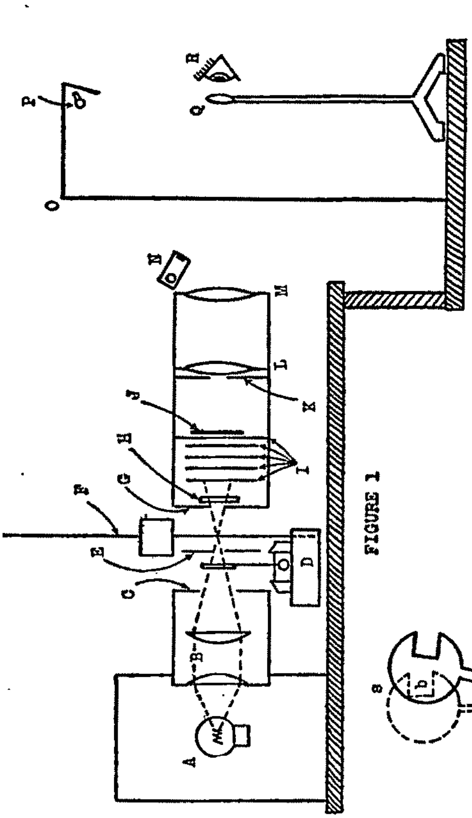

FIGURE 1

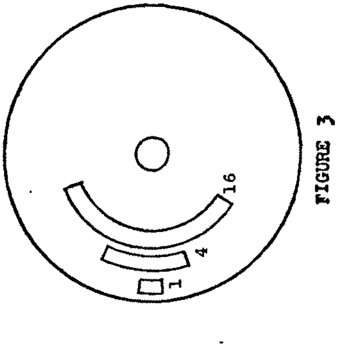

FIGURE 3

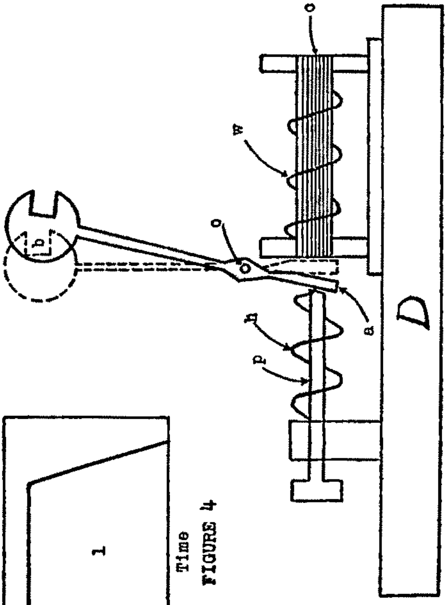

FIGURE 2

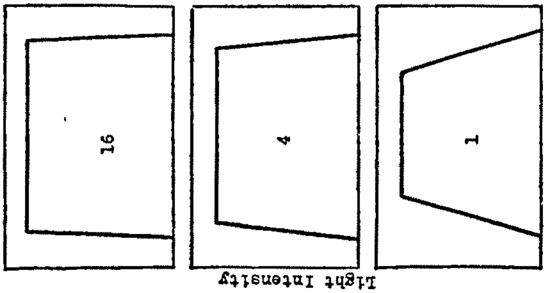

FIGURE 4

image of the slide (J) on the ground glass screen (O) from behind, leaving the surrounding field unilluminated by transmitted light. The surrounding field is, then, illuminated from the front by a series of low-power, incandescent bulbs at (P).

(4) The projection of the slide (J) on the ground glass screen (O) is by means of a system of 2 simple lenses (L and M). Good definition in the projected image is obtained by inserting a diaphragm (K) which cuts off the light from the peripheral portions of the lenses, thus reducing the effects of spherical aberration quite satisfactorily.

DETAILS OF CONSTRUCTION

Production of the Illuminating Beam. The source of light (A) was a 100 watt, 115 volt, concentrated filament, gas-filled lamp (Mazda spotlight), run at 110 volts direct current. This choice was made (1) because 110 volts was at all times available, (2) because direct current is more conveniently measurable than alternating current, and (3) because it is more likely that the initial calibration will hold for several months when the lamp is operated well beneath the rated 115 volts. The voltage was measured by a voltmeter and kept constant by means of a slide resistance of 100 ohms in series. The lamp was enclosed in a light-tight, sheet-iron housing, 17 in. high, 12 in. wide, and 13 in. long. The housing was sufficiently large to prevent overheating.

The condensing system (B) consists of the usual two plano-convex, 6 in. lenses, each having a focal length of about 6½ in.[5] The light (A) was placed at the focus of the first lens, the episcotister (F) at the focus of the second, the lenses mounted 4 in. apart.

The diaphragms (C, E, and G), ⅝ in. diam., were inserted to prevent light leaks. The second diaphragm (E) has the additional function of protecting the rather fragile magnetic shutter (D) from accidental entanglement with the rapidly rotating episcotister (F).

Control of Duration of the Illuminating Beam. The electro-magnetically operated shutter (D) is the only unit of the apparatus that was difficult to design. The function of this shutter is to allow the beam to reach the episcotister (F) before its aperture arrives at the light beam and subsequently to cut it off before that aperture comes around on its next turn. The episcotister was used between the limits of 30 and 150 r. p. m., hence the magnetic shutter (D) must be opened and closed within ⅖ sec. to function adequately. The moving part must therefore be of small inertia. Fig. 1 shows the end of the shutter; Fig. 2, a side view such that the beam to be intercepted lies perpendicular to the page. The movable part of the shutter consists of a light bar of sheet aluminum—an L cross-section to give stiffness—pivoted on a needle suspension at (o), carrying, 5 in. above the suspension, the shade (s), and 1 in. below the suspension, the armature (a). The shade (s) is about 1½ in. in diam. and is made of a double thickness of aluminum foil, which at once gives it durability, opacity, and lightness. The shape of the shade is such that light leakage is reduced to a minimum. The armature (a) of soft iron is ⅛ in. thick by ½ in. square.

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[5] If I were to construct anew, I should take a second lens of longer focal length, say 12 in. With the 6 1/2 in. focus, the axial distance within which all the light flux passes through an area of 1/2 in. diam. is only about one inch. Thus the shutters (D and F) and the diaphragms (E and G) all had to come within the space of a single inch. The diagram (Fig. 1) shows these parts apparently more widely separated, a distortion of the true scale which was introduced for the sake of clearness. The introduction of a second lens of 12 in. focus into the condensing system (B) instead of the present 6 1/2 in. focus lens would eliminate this crowding.

The helical steel spring (h) holds the armature away from the magnet against post (p)—as is shown in Fig. 2 in solid lines—when current is not passing through the coil (w). When the circuit is closed, however, the armature (a) is attracted to the magnet core (c)—as is shown in the dotted lines—thus moving shade (s) ¾ in. to one side and allowing the light beam (b) to pass through. The magnet is wound with 400 turns of No. 24 insulated copper wire on a cylindrical laminated core (c) of soft iron ⅜ in. diam. by 2 in. long. With a current of 2 or 3 amperes, the action of the shutter is unfailing and very rapid. Since the coil (w) itself has a resistance of about 2 ohms, which with the external circuit makes a total of approximately 4 ohms, the shutter operates satisfactorily on a 12 volt storage battery.

The episcotister (F) consists of a 22 in. metal disc belted to a direct-current motor of ⅛ h. p. which drives it at regular rates that may be varied at will continuously from 30 to 150 r. p. m., the variation being accomplished by a rheostat in series with the motor. The disc is used for two simultaneous but distinct purposes: (*1*) the timing of the light pulse over a greater range of durations than that of 5 to 1 given by variation in motor speed alone; and (*2*) the control of the magnetic shutter (D) so that the two operate in mutual coördination.

(*1*) The range of durations was attained by fashioning three sector apertures, ¾ in. wide, in the disc (see Fig. 3); the second (4) is of four times the angular extent of the first (1); the third (16), of four times the angular extent of the second (4). The smallest sector was cut out furthest from the center (½ in. from the edge) and is about 1 in. long, or, roughly, of 5 degrees. The second (4) and third (16) apertures are successively closer to the center of the disc. To understand how the light beam is caused to pass through aperture (16) for example, after adjustments have been made for aperture (1), it must be understood that the disc and driving motor are fastened to the table permanently. The other parts of the apparatus, except the ground glass screen (O) and its immediate accessories, are arranged so that they can be moved horizontally in the plane of the rotating disc; or, stated another way, perpendicularly to the light beam. Although, as indicated in Fig. 1, the center of the disc is slightly higher than the center of the light beam, still the more centrally situated apertures can be reached readily by this horizontal motion. To facilitate the adjustment, all these movable parts, except the eye guide (Q), were mounted on a single board which slides on a grooved support in the required direction. For durations as short as 0.005 sec., the smallest aperture is used at 150 r. p. m.; for durations as long as 2 sec., the rotating shutter is removed from the path of the light beam, the light pulse is then controlled by the magnetic shutter alone, and the disc rotated at 30 r. p. m. Fig. 4 shows the rise and fall of light intensity in comparison to the duration of the light pulse for the 3 apertures—1, 4, and 16.

(*2*) The control of the magnetic shutter is maintained through the action of a 5-pointed starwheel which rotates ⅕ turn with each revolution of the disc. The wiring for the magnetic shutter (D) places in series the 12 volt storage battery, a key which can conveniently be depressed by the observer, the magnetic shutter, and a switch operated by the starwheel. The switch controlled by the starwheel is arranged so that it closes at one of the 5 positions of the starwheel but remains open for the other 4. Thus, when the observer's key is depressed, the magnetic shutter opens—and stays open—for one rotation of the episcotister in every five. The lug on the episcotister disc which engages the starwheel is placed 1 in. from the center of rotation; this arrangement is practicable for high speeds as well as low. The starwheel is so adjusted that the time of engagement with the lug corresponds to the end of the light pulse as determined by the episcotister. Thus, the magnetic shutter does not commence to open until the aperture of the episcotister has been passed and does not commence to close until the same position has been reached on the next turn. In this way a single pulse of the duration expected from the angular aperture and speed of the episcotister is assured.

Control of Intensity of Illuminating Beam. As before stated, continuous variation of the intensity was accomplished by means of a neutral absorbing wedge (H). This neutral wedge, together with a compensating wedge which yields equal transmission over the whole absorbing field, gives transmissions varying from 0.1 to 0.9 depending on what part of the wedge lies in the path of the beam.[6] The wedge is supplied with a scale from which its position may be read.

The combination of diffusing screens (I) used was a milk glass whose transmission in place was approximately 0.01 and four ground glass screens whose transmissions were about 0.2, 0.2, 0.2, 0.4 respectively. The screens were all 5 x 6 in. The inside of the housing containing the diffusing screen was coated with MgO. This served to keep the color of the illuminating beam constant irregardless of the number of diffusing screens used.

To measure the intensity of the image of the lantern slide (J) the wedge and diffusing screens were calibrated in place by determining the intensity of the stimulus image with a photometer pointed through the eye-guide (Q). From this data a calibration curve plotting stimulus intensity against wedge setting was made up for each combination of screens.

Projection of the Slide Image. The two projection lenses (L and M) are 5 in., double-convex lenses of 10 in. focal length. The slide is placed 10 in. from the first lens (L) which is, in turn, 10 in. from the second (M). The ground glass screen (O) is placed to catch a focussed image of the slide which, of course, makes the screen's distance from the second lens (M) again 10 in. To get brightnesses in this image as high as 500 ml. it is necessary to remove all the diffusing screens (I), set the wedge (H) at its highest transmission, and use the entire transmitting surface of the lenses (L and M) without a restricting diaphragm. The resulting image is not perfect on account of spherical aberration. To obtain a uniform field with good definition some intensity must be sacrificed; for, at least, one diffusing screen (I) must be placed in the path of the beam and the diaphragm (K), 1½ in. in diam., must be inserted. The resulting image may be varied in intensity up to about 20 ml. The lower limit of intensity may be extended indefinitely, of course, by the addition of diffusing screens; but if the apparatus is to be used for after-image work alone it seems probable that 0.001 ml.—attainable with the five diffusing screens specified above—is a brightness well below the requirements of any but the most extreme cases.

To make easy the elimination of stray light the absorbing (H) and diffusing (I) systems together with the projecting system (L and M) were enclosed in a light-tight, sheet-iron box. Light enters through diaphragm (G) and leaves through lens (M). The sheet-iron, gauge No. 24, gives a sufficiently rigid construction that the wedge holder (H), the slotted supports for the screens (I), the holder for the slide (J), and for the lenses (L and M) needed no other supporting framework.

Control of Observing Conditions. The screen (O) which forms the field surrounding the stimulus is a piece of window glass, 2 ft. high by 3 ft. wide, ground to an even, diffuse finish on the side toward the observer (R). The grinding was done by hand; a small glass plate was used as a grinding tool with fine emery and water as the abrasive. The source of the surrounding field illumination was a series of four 6 volt, 2 c. p. lamps supplied with current from the A. C. power line through a 100 watt, step-down transformer. The lamps were wired so that one, two, three, or all of them could be lighted as desired. A rheostat served to make possible the continuous variation of intensity between these arbitrary steps, without producing objectionable change in the dominant wave-length of the illuminating beams. In this way the background intensity could be varied continuously between 0.5 and 0.0001 ml. The eye guide (Q) is a 1 in. wire circle mounted on a standard 11 in. in front of the screen. At this observing distance the ground glass screen fills most of the visual field. To illuminate the entire surrounding field with approximate uniformity, a screen of gray cloth one foot wide was hung up, extending out perpendicular to the ground glass (O) along its sides, top and bottom.

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[6] This unit, consisting of a neutral wedge and compensator, is carried in stock by the Eastman Kodak Company, of Rochester, New York.

Behind the screen, but above the beam which projects the stimulus image, is located the unit (N) whose function is to provide a fixation point. This unit consists of a 3 volt, flash-light lamp and converging lens of ½ in. diam. and 1 in. focal length. The lens projects an image of the lamp filament onto any desired part of the ground-glass screen (O). So that no disturbing effects will ensue a red bulb was chosen, and the 3 volt lamp operated at 2 volts or less. The intensity was controlled by a rheostat wired in series with the lamp.[7] When the red image of the lamp filament is reduced in intensity so as to be just definitely above the threshold, it is entirely blotted out by the stimulus and the succeeding after-images, and does not interfere with their observation.

Summary. The principle advantages of the apparatus may be summarized as follows:

(1) A light pulse is produced whose duration is continuously variable between 0.005 and 2.0 sec. The rise from zero to maximum intensity and the subsequent cut-off of this pulse is so rapid as to be negligible in comparison to the duration of the pulse.

(2) The intensity of the light stimulus is continuously variable from 0.001 up to 500 ml.

(3) The surrounding field intensity is continuously variable from 0.0001 up to 0.5 ml.

(4) A sharp image may be projected of any object that may be put on a lantern slide.

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[7] These are the devices used for intensity control of fixation point by Fröhlich in his study of after images, *Zsch. f. Physiol.*, 53, 1921, 81-82.

SOME NEW LABORATORY AND DEMONSTRATIONAL APPARATUS

By EDWIN B. TWITMYER and SAMUEL W. FERNBERGER,
University of Pennsylvania

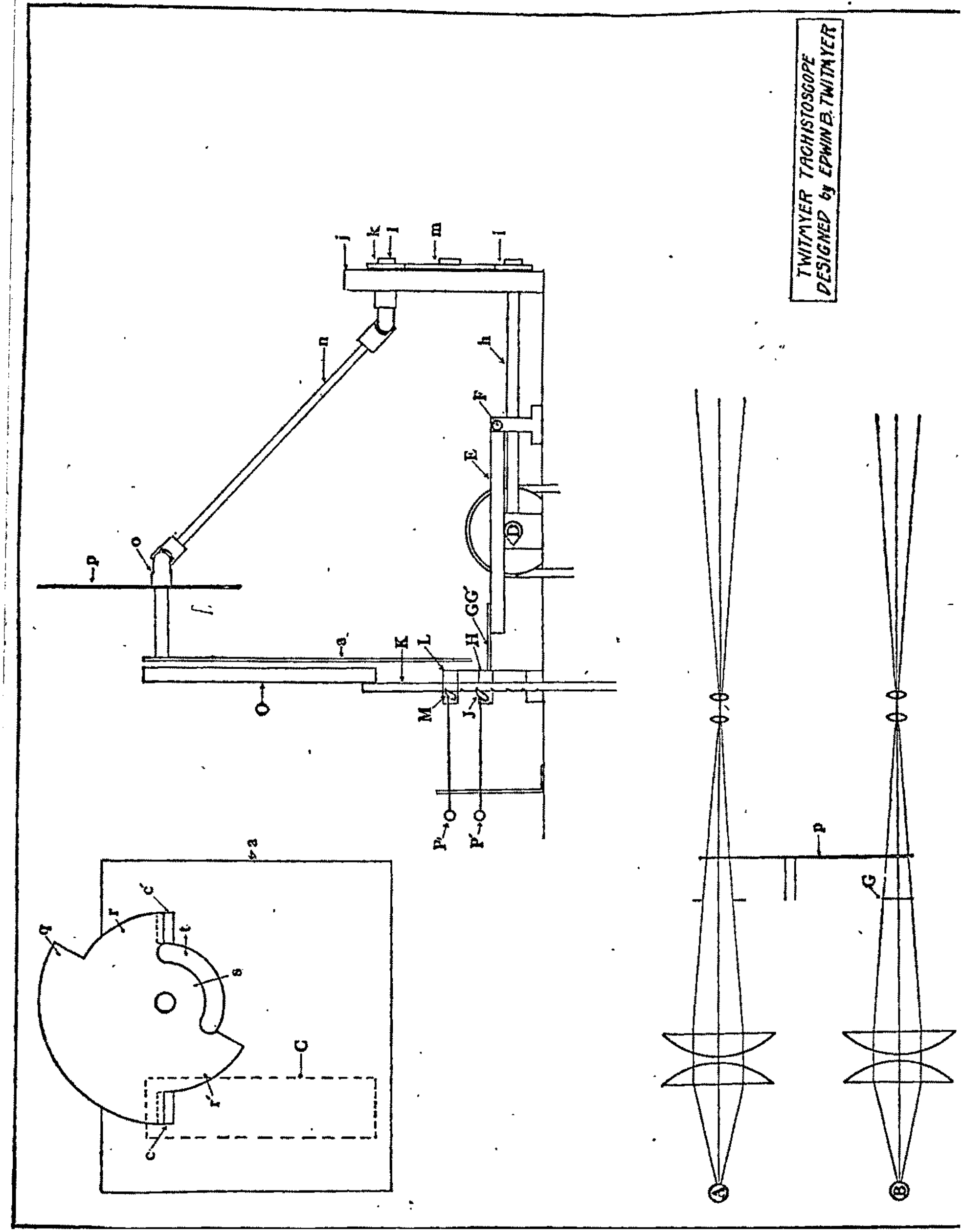
THE TWITMYER DEMONSTRATION TACHISTOSCOPE

The problem of serial exposures for demonstration purposes before a large group has not heretofore been successfully solved although a number of devices have been developed for this purpose. Several factors are essential for such an apparatus. (1) It must produce an image large enough and clear enough to be seen from a distance and so arranged that the subjects sitting far away will have an unobstructed view of the material. (2) The apparatus must provide for regularity of exposure. (3) It must provide for a pre-exposure and a post-exposure field. (4) It must provide for changes in the length of the exposure. (5) It must provide for a sufficient number of regularly timed successive exposures for memory work. (6) It must not disclose the changing of the material.

The apparatus, to be described, provides a clear and intense image which can be seen clearly for a distance of more than 100 feet. It provides for a pre-exposure and a post-exposure field, as well as for 12 equally timed exposures. The times of the exposures can be varied. While the material is being changed between exposures, the subject sees a field similar to the pre-exposure field.

This apparatus is built around two projection lanterns of the usual type. One lantern (A) provides illumination on a screen between exposures (while the material is being changed) and the other lantern (B) provides illumination of the materials. These two fields are focused so that they exactly coincide on the screen and the effect is of a single illuminated exposure field in which materials appear and disappear without spatial movement.

This result is accomplished by a motor and speed reducer which revolves a shaft, at the end of which is a sharp pitched cam (D). When this cam revolves, it pushes up an arm (E) which is pivoted at one end (F) and carries two fingers at the free end (G and G_1). These fingers press up a slotted carrier (H) which moves on two guides set on a central upright. (H) carries a pawl (J) which engages in a slot in the rack (K) and moves this rack up to the extent of the excursion of (H). A second carrier (L) is placed permanently above (H) and carries a second pawl (M) which engages in the slot of the rack (K) just above that engaged by the pawl of the carrier (H). This prevents the rack from falling back to its former position when the carrier (H) returns on its down stroke. This return of the carrier (H) is accomplished by two springs placed between the carriers thus providing a more positive action than could be accomplished by gravity alone.


TWITMYER TACHISTOSCOPE
DESIGNED by EDWIN B. TWITMYER
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Above the rack (K) and fastened to it is a frame (O) which is the holder of the photographic slide (C) holding the material to be shown. By means of the mechanism just described, the materials on the slide are brought successively opposite the focus of the projection lantern (B). In order to return the slide to its first (pre-exposure) position, it is only necessary to disengage the two pawls from the rack and gently lower the rack, frame and slide by hand. The pawls are engaged in the teeth of the rack by two springs. They are disengaged by two levers (P and P′) working against the springs.

A permanent screen (a) is fastened to two uprights. In this screen are cut two rectangular holes (c and c′) before the focus of each lantern. These control the size and shape of the exposure field acting as a diaphram. The horizontal relation between the screen (a) and the frame (O) is permanent. The vertical relations between these two may be altered by moving a screw, which changes the relations between a permanently attached standard and a standard at the top of the screen (a).

The exposure shutter is actuated from the same driving shaft, which actuates the cam (D), which in turn raises the rack (K). Thus the synchronization of the movements of the shutter and the movements of the rack and the frame (O) is accomplished. Fastened to the middle of shaft is a miter gear which engages in another miter gear. The second gear is permanently attached to a shaft (h) which is thus made to revolve. At the other end of shaft (h) is a spur gear (i). An upright (j) carries another spur gear (k), attached to the outer end of another shaft (l). In order to raise gear (k) so it will be more nearly opposite to the center of the shutter, another large spur gear (m) is inserted between (g) and (k) and transmits the power from one to the other. At the end of shaft (l) is a universal joint which is also fastened to another connecting shaft (n). Another universal joint connects this connecting shaft (n) with the shaft (o) which is inserted in the screen (a). On shaft (o) is permanently attached the shutter (p), and the mechanism just described causes it to revolve synchronously with the movements of the rack (K).

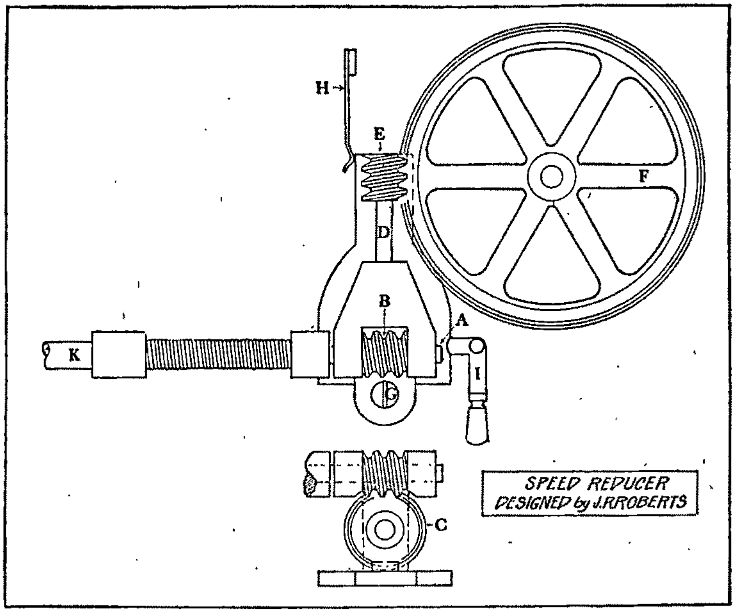
The shutter (p) is cut from sheet metal and is of the shape indicated in the drawing. There are three lengths of radius—(q) is the longest; (s) is the shortest; and (r) and (r′) are intermediate. The circumference for the distance of radius (q) is ⅓ of the total circumference; that for (r) and (r′) combined is another third and that for (s) is the last third. The holes in the screen are so arranged that the beam of light from lantern (A)—the inter-exposure field—is cut off by radii (q), (r) and (r′) and is only exposed while radius (s) is passing in front. The beam of light for lantern (B)—the exposure field—is cut off only by radius (q) and is exposed while (r), (r′) and (s) are passing in front. Hence, when the exposure is being shut off from lantern (B), the inter-exposure field comes on, and vice versa. The ratio of the length of exposure to the time between exposures is obviously 2:1. In our present arrangement, the total revolution of the shutter gives an exposure at the rate of 1 per sec. This means that the material is actually exposed for ⅔ of a second and that the inter-exposure time is ⅓ of a second, without a break in the light of the total exposure field. A counterweight (t) aids in the balance of the shutter.

It has been necessary to have an accessory apparatus for the taking of the slides in order to insure that each stimulus will come exactly in the same place in the exposure field. This device involves the use of an enlarging camera with a slide held in a frame, similar to the frame (O). The slide is mounted on a rack exactly similar to rack (K), which is held in position by a hand operated pawl. The first item to be photographed is projected into the enlarging camera with the slide as far down as possible (leaving room for the pre-exposure field) and is thus photographed. The rack is then pushed up to the second notch and the second material photographed; and so on.

It is possible to use the rack and pawl device for individual exposure in research without the use of the lantern by placing the subject in front of the machine and faced toward it. In such a situation it is possible to dispense with the use of photographic plates and to substitute materials on cardboard.

THE ROBERTS SPEED REDUCER

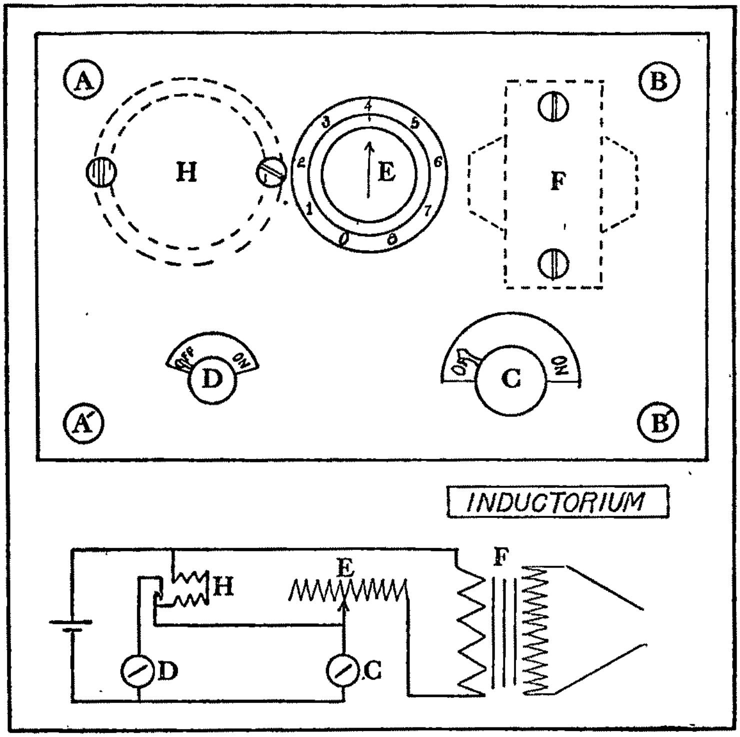
The kymographs used in the laboratory work at the University of Pennsylvania are of the motor driven type. Until recently the motor speed has been reduced by a speed reducer of the belt-wheel type involving a series of eight pairs of wheels. This has been found to be too large, heavy and cumbersome for practical use. It also involves continual adjustment of the belts to avoid slipping. Hence the speed reducer now described was devised to obviate these difficulties.



ROBERTS SPEED REDUCER

The overall size is 4.5 x 2.25 x 2.25 in. The speed reducer is mounted directly on the base of the old kymographs. It consists of a cast brass base, holding two shafts set at right angles to each other. One end of shaft (A) is attached to the motor (at K) by a shaft drive or a flexible spring drive—which is shown in the drawing—and at the other end is a worm (B). The worm (B) engages in a gear of 20 teeth (C) which is attached to the end of the second shaft (D) which, in turn, is set at right angles and below shaft (A). At the other end of shaft (D) is another worm (E), similar to the one (B) already described. The worm (E) engages in the teeth of a large gear (F) with 80 teeth, which is rigidly attached to the upright driving shaft of the kymograph.

The pulley is removed from the motor and the flexible driving shaft is placed directly over the shaft of the motor (K). Thus the speed is reduced from 1,750 revolutions per min. of the motor shaft to about one revolution per min. of the kymograph drum. The speed reducer is fastened to the base of the kymograph by a single screw (G). Worm (E) is held in contact with gear (F) by a spring (H). The entire speed reducer is actuated against spring (H) by a handle (I), so that worm (E) may be disengaged from gear (F), thus stopping the kymograph drum with the motor and speed reducer running. The kymograph drum is again started by pulling the handle (I) and allowing the spring (H) to re-engage worm (E) and gear (F). Grease cups are provided for the bearings of shafts (A) and (D).



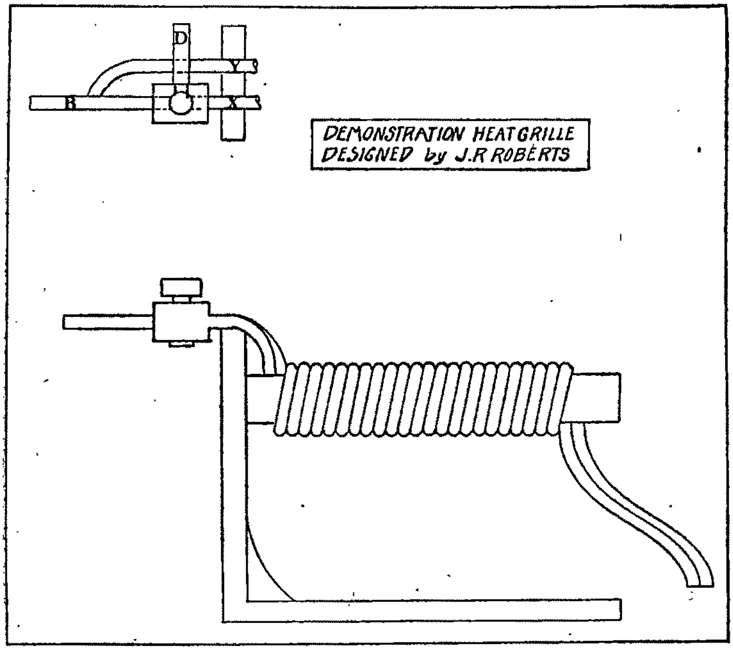
Roberts Inductorium

The worms and gears used in this apparatus are stock parts sold by the Philadelphia Gear Works, Philadelphia, Pa. The gears are Catalogue numbers 835 and 839. Steel worms were used to fit these gears. The price of all gears and worms was $4.50.

This speed reducer recommends itself not only for the advantage in size and weight, but also in that the entire mechanism is a direct drive without the possibility of slowing or stopping as in the older friction driven or belt driven machines.

The Roberts Inductorium

A useful laboratory inductorium is contained in a box 8 x 5.5 x 5.5 in. It is made entirely of stock electrical parts. Mounted on a Bakilite panel are two sets of binding posts (A and A′) leading in from the source of current—a dry cell—and (B and B′) leading out to the electrodes. Two switches are also mounted on the top of the panel—one (C), for simple make-and-break (a Marco snap switch) and one (D), for faradic current (a Yaxley switch). A rheostat knob (E) controls the intensity of the current.



Demonstration Heat grille

The parts consist of a transformer (F), such as is used in a household for ringing a bell, with a ratio of about 1:12; a 25-ohm rheostat (G) such as is used in radio for controlling the filiment of a vacuum tube; and an ordinary commercial buzzer of the nutmeg type (H). The purpose of the transformer is to set up the current or to increase the voltage. The rheostate controls the intensity of the current by introducing more or less resistance. The buzzer is the means for rapidly making and breaking the current. The wiring is indicated in the accompanying diagram.

To obtain simple make-and-break stimulation, one merely throws switch (C). To obtain a faradic current, one merely throws switch (D). The total cost of such an inductorium should be less than $6 at the present prices and the saving in space and cumbersomeness is considerable. This form of inductorium obviates the necessity of adjusting the vibrating hammer of the older type of apparatus. Also the parts are permanently enclosed in a box which materially lessens the chances of injury due to handling.

A Demonstration Heat Grille

A convenient form of demonstration heat grille is made by coiling copper tubing of ⅛ in. bore with walls 1/16 in. thick about a horizontal circular wooden rod of ¾ in. diameter. Two tubes are used and these are coiled alternately for approximately 6 in along the wooden bar. The advantage of this form of heat grille is that there are no sharp shoulders in the tubing with the consequent constriction in the tubes which interferes with the flow of water.

Arrangements are made to connect tube (D) with a tank of cold water and tube (B) with a tank of warm water. The following arrangement of a two-way cock is easily constructed. Warm water flows continually through tube (Y) from the inlet (B). When the cock is in the position (BX) warm water also flows through tube (X). By turning the cock to the position (DX) the warm water is shut off from tube (X) and the cold water is allowed to flow through tube (X) without affecting the flow of warm water through tube (Y). This has the advantage over the older form of heat grille inasmuch as some water flows through both tubes continually. The present form of heat grille is adapted to stimulating the palm of a hand which simply grasps the tubes (X) and (Y). Although there are not as many temperature spots on the palm of the hand as elsewhere on the surface of the body, this apparatus, with the dimensions indicated has never failed in arousing the synthetic experience of 'hot.'

AN IMPROVED APPARATUS FOR ACTUATING TONAL CYLINDERS OF THE KOENIG TYPE

By SAMUEL W. FERNBERGER, University of Pennsylvania

The use of Koenig cylinders for the determination of the upper limen for tone is widespread. Their use in research problems has not been eliminated by the introduction of the radio frequency oscillators more recently devised, and their use in clinical procedure is indicated. The actuation of these cylinders has always been a problem for research. In clinical procedure, the usual method has been to suspend the series of cylinders in a metal frame by dental floss or some other strong but fine thread and to strike the cylinders with a small metal hammer. It is obvious that one cannot control the intensity of impact by such means although the procedure may be adequate for clinical practice.

The earlier method for research was to suspend the cylinders used in a frame similar to the one described for clinical work but to actuate them by the blow of a metal ball released by an electro-magnet. The intensity of the blow was controlled either by the weight of the ball or the height of the fall or by a combination of both of these factors. The difficulty of such a procedure was to bring the cylinder under the ball.

This difficulty was partially eliminated by Möller[1] who mounted the cylinders in a semicircular trough. The electromagnetic release for the actuating ball was attached to a vertical rod mounted in the geometrical center of the circular trough. The electromagnet could thus be swung into any radial position and so could be brought directly over any one of the cylinders. Such an arrangement eliminated many of the difficulties connected with experimentation with the cylinders of the Koenig type. There remains the difficulty, however, of possible spatial localization and hence of judgments on the basis of this secondary criterion. In some early experimentation, we believed that this factor of spatial localization might have a greater influence in a long series of judgments than might have been believed *a priori*. This point was not investigated experimentally but we believed it best to eliminate any possibility of such an influence.

Hence the following apparatus was devised by the author and Mr. John R. Roberts at the University of Pennsylvania. It consists of an adaptation of the turning-top table which has

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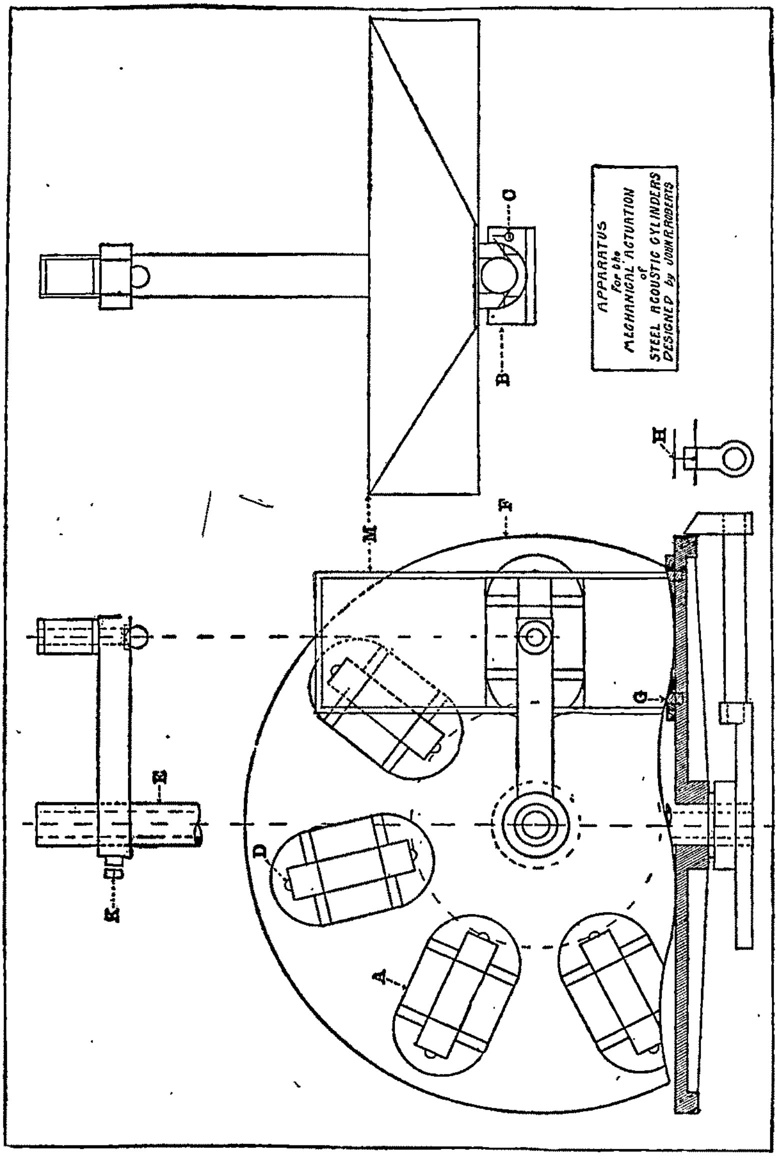
[1] E. F. Möller, The effect of change of intensity upon the upper limen of hearing, this JOURNAL, 33, 1922, 570-577.


APPARATUS
For the
MECHANICAL ACTUATION
of
STEEL ACOUSTIC CYLINDERS
DESIGNED by JOHN R. ROBERTS
A
B
C
D
E
F
G
H
K
M

been used for many years for the presentation of lifted weight stimuli[2] to our present problem. The set of cylinders, for which the apparatus was devised, consists of a series of 40 cylinders ranging from 16,000 d.v. to 24,000 d.v. with intervals of 200 d.v. Our first problem had to do with the handling of the individual cylinders inasmuch as different subjects have different liminal values which obviously necessitates the employment of different cylinders from our total series. In dealing with apparatus such as this, it is advisable to handle the cylinders as infrequently as possible to avoid rusting. Hence we mounted each cylinder in a saddle and it could then be placed in the stimulating apparatus without handling the cylinder itself.

These saddles, indicated in top and end view on the accompanying drawing as (A) and (B) respectively, show the cylinders in position. They were made of aluminum castings, each of a floor plate with two uprights toward either end. These uprights are cast with an almost semicircular hole cutting away most of the metal of the uprights to accommodate the cylinder. Three holes are drilled in the sides of each upright. One of these is tapered to accommodate a taper pin (C) and two small holes are drilled to take each end of the dental floss used to suspend the cylinder. The dental floss is inserted in these holes and knotted at one end. The other end is passed through a hole in the taper pin and fastened.[3] Once the two strings have been adjusted in the saddles in the manner indicated, the grooves in the cylinders were placed over them and the height of the cylinder from the floor of the saddle was adjusted by screwing the taper pin in the same way that one adjusts a violin key. Two holes (D) are drilled in the floor plate of the saddles which may engage in two short pins permanently fixed in the apparatus. Thus the saddle one wishes to employ is readily removed or placed in exactly the proper position by merely lifting it up or setting it down. Inasmuch as the different tones of the cylinders are entirely a function of their length and, inasmuch as the position of the grooves of the cylinders are a function of their total length, it was found necessary to develop saddles with different degrees of separation of the uprights. All of the base plates are of exactly the same dimensions. With our present set of cylinders, which is a rather extended series, it was found that only three saddles of different dimensions were necessary. The inside separation of the uprights in the present set measure 3.6, 3.8 and 4.2 cm.

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[2] Cf. F. M. Urban, *The application of statistical methods to the problems of psychophysics*, 1908, 3-4; S. W. Fernberger, On the relation of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 14, 1913, (Whole No. 61), 10-11.

[3] For the use of taper pins in making fine adjustments of the length of cords as in the case of the Sanford chronoscope, cf. S. W. Fernberger, Improvements on the Sanford vernier chronoscope, this JOURNAL, 37, 1926, 154-155.

The main part of the apparatus consists of a base which rests flatly on the table. Vertically through the center of this is inserted a steel cylindrical tube (E) 2.5 cm. in diameter with a 1.3 cm. hole. Rotating about this tube is the turning table proper (F) which is made from a single aluminum casting, 37 cm. in diam. Seven pairs of short pins are inserted in this casting (G) along equidistant radial lines over which the saddles with the cylinders are placed, as described above. Lines are cut on the edge of the table (F) just on the line of the radial pins and these lines are brought opposite to the sighting line (H) when the table is used. In this fashion, each cylinder may be brought into proper position in relation to the electromagnetic actuator. This sighting line is placed 30′ to the right of the radial lines.

The electromagnetic release is held on a horizontal arm which is attached to the upright (E) by a set screw (K). Thus. it may readily be raised or lowered by a very simple adjustment. In order to insure the correct lateral position of the electromagnet, the point of the set screw is placed in a longitudional slot cut into the upright (E) which is just exactly opposite to the sighting line (H). By this adjustment, one is assured that the ball will hit the proper place on the cylinder, provided the lines on the table opposite each saddle are brought exactly opposite the sighting line (H). A small piece of soft rubber ring is inserted below the electromagnet so that the metal ball is brought into the field without actually touching the magnet proper. The wires from the magnet are run down the hole in the tubular upright (E) and thence to the floor so that the circuit may be broken by a key actuated with the foot. The arrangement leaves both hands free for handling the swinging of the table, for placing balls in the electromagnet and for recording.

If the cylinder is in proper position with regard to the electromagnet, the ball will fall on the cylinder exactly in the middle (from end to end) but a little off center from the top. It will therefore bounce to one side. In the present apparatus, a felt covered metal frame has been mounted on the upright (E) in such a position just above the cylinders that the ball will bounce directly into it entirely noiselessly. This frame is indicated in the drawing by the letter (M).

The apparatus just described is very easily manipulated and the lines on the edge of the turning table are brought to the sighting line with a surprising degree of accuracy and with a small amount of training. The cylinder which is actuated is thus always in the same spatial relation to the subject for all successive stimulations. We have found, for our present purpose, that seven cylinders correctly chosen are sufficient for the determination of either a stimulus limen or of a difference limen. If more cylinders were desired, it would be a very simple matter to enlarge the entire plan of the apparatus to accommodate the additional cylinders.

This apparatus, with the addition of another electromagnetic release device, set along another radial line, and of another sighting line below it would readily lend itself for the attacking of such problems as the effect of intensity on the upper limen for tone. Balls of the same weight would be used and the differences of intensity of the impact would be secured by setting the electromagnets at different heights on the upright (E). Hence the two series of different intensities could be mixed, and the judgments made under exactly similar conditions—a problem which has been difficult of solution heretofore.

THE CONSTRUCTION AND CALIBRATION OF KOENIG CYLINDERS

By EDWIN G. BORING, Harvard University

Mr. A. G. Cox of the Harvard Psychological Laboratory has recently constructed two sets of steel cylinders of the Koenig type. One set is like the set described by Fernberger in the preceding article of this JOURNAL.[1] It consists of 41 cylinders ranging from 16,000 d.v. to 24,000 d.v. by increments of 200 d.v., and is used for the accurate establishment of the psychometric function that determines the upper limit of hearing. The other set consists of 22 cylinders for the notes of the three upper natural diatonic octaves, ranging from $g^4 = 3072$ d.v. to $g^7 = 24576$ d.v. ($c^1 = 256$). This set is useful for demonstration since it extends the musical scale from a region, which is obviously musical and where the tones are loud and well sustained, up to and beyond the limit of hearing. We have thought that with this instrument we might come at a way of discovering whether, where, or just how the tones of the upper audible region lose the musical properties that are involved in fusion, consonance, and the perception of musical interval, although we are not yet ready to assert that the present instrument is adequate to this problem.

At present both of these sets have the cylinders suspended at the nodal points by loops of thread from pins on two diverging bars fixed to a base. It is obvious that for accurate psychophysical work we shall need to construct Fernberger's apparatus.

The main purpose of the present note is to describe the construction and calibration of these cylinders. The frequencies must be calculated and can not readily be measured directly.[2] The formula is: $f l^2 = k$, where f is the frequency, l the length, and k a constant that depends upon the material, the units of f and l, and other factors. The usual practice is to assume that k is constant for a given steel bar of constant diameter, to calibrate a long standard length of this steel, and then to compute the desired cylinders from the standard calibrated bar. If F and L be respectively the frequency and length of the calibrated standard and f and l the frequency and length of any cylinder that it is desired to construct, we have, since both are equal to k, $f l^2 = FL^2$; or, for a given f, the length should be, $l = L\sqrt{F/f}$

Two objections have been offered to this procedure. One is that it is not safe to regard the steel as perfectly homogeneous, that the constant for one section of a rod may not be the same as the constant for another section. The other objection is that theory should not supplant observation and that calculated frequencies can not be trusted in default of direct observation of the actual cylinders.

As bearing upon the first of these objections, we find that difference of kind of steel makes a slight, but not a great, difference. We cut three bars, X, Y, and Z, each 630 mm. in length, from 13/16" round steel. For these bars we determined the frequencies, suspending them by loops of twine at the nodal points (¼ the total length from each end), sounding them by striking with a rubber hammer, and then counting beats against a Stern variator, which was in turn calibrated by beats with a Koenig fork. The error in measurement of the lengths of these rods is not greater than 0.3 mm; we did not seek to be more exact. The frequencies are accurate to within 0.3 d.v., since this is the extreme range of variation for different counts of beats at the rate of about 3 beats per sec. X was hot rolled steel of high carbon content. Y was cold rolled steel with further specifications unknown. Z was cold rolled steel known to have a low carbon content as compared with X. The comparison is as follows:

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[1] S. W. Fernberger, this JOURNAL, 38, 1927, 120-124. Fernberger's set was made to order by the Standard Scientific Co. of New York (cf. C. C. Pratt, this JOURNAL, 30, 1920, 403-406), but this firm has discontinued the manufacture of acoustic apparatus. As far as I know, these cylinders are no longer being made by any firm in America.

[2] The high tones are not sustained; the calibration by beats or by difference tones is therefore difficult and can not of course apply above the limen of the observer. Schwendt's claim that the Kundt dust method is satisfactory does not seem to be substantiated; cf. Pratt, *loc. cit.*

	X	Y	Z
l:mm.	630.	630.	630.
f:d.v.	242.4	239.7	239.7
k: constant ($= fl^2$)	95,136,930	96,208,560	96,208,560

These results were checked by the observations that Y and Z did not noticeably beat with each other, and that X and Y gave on the average 2.9 beats per sec. with each other, whereas their calibrations with the variator would require 2.7 beats per sec. It is not, however, as satisfactory to beat two rods against each other as to beat a rod against the variator.

Thus it appears that two random samples of cold rolled steel, Y and Z, happened to give identical results within the accuracy of these observations, and that two contrasting samples of steel, X and Z, gave results differing by only a little over one percent, a difference probably depending on the character of the steel, since it is several times the error of observation. It would seem, however, that we need not expect a large change of the constant due to heterogeneity of the steel in rods cut from a single bar of high-grade stock advertised by its makers as "homogeneous." Errors in frequencies thus computed should not, on this account, be more than a tenth of one percent and may be much smaller.

The foregoing data also bear on the error in frequency introduced by inaccuracy of machining the length of a bar. With X, 1 mm. = ca. 0.75 d.v. Since there is no difficulty in machining a long rod more accurately than to even millimeters, it is plain that accuracy is easily established to less than one d.v. At about 16,000 d.v., with steel like the steel of Z, 1 mm. = ca. 400 d.v., but 0.1 mm. = ca. 4 d.v. Since it is possible with a micrometer to machine the short cylinders to less than 0.1 mm., the error on this account may be as little as 1/40 of one percent. We cannot, of course, assume that it is actually so small since other sources of error may enter in.

The validity of the formula can be checked in the middle tonal range where computation can be compared with observation. For this purpose we took the rod Z, described above, and cut it down from 630 mm. to 431 mm., a length that by calculation should give 512.8 d.v. By beating this bar against a Stern variator, which was calibrated in turn by a Koenig fork, we determined the frequency of the shortened rod as 512.6 d.v. The difference of 0.2 d.v. is within the error of observation by beats, and we assume the verification of the formula.

It is customary to turn grooves in these cylinders at the points of suspension in order to hold the suspending loops. It is not clear, however, what effect these grooves have upon the calculated frequencies. That the removal of metal may have an effect first appeared when we turned two grooves in rod Y, not at the nodal points, but (as the result of an error) quite near each end. The removal of this metal changed the frequency of the rod from 239.7 to 243.0 d.v. Our most accurate test of the effect of grooving at the nodal points was made with rod X. This rod was first cut to 608 mm. and gave then 257.9 d.v. (thus checking the formula). We then turned two V-shaped, 60° grooves at the nodal points. The grooves were about 1 mm. wide at the top and 1 mm. deep. Recalibration of the rod then gave 256.9 d.v.; the pitch had been lowered about 1 d.v., an amount significant as against the error of observation.

It is not possible to say what the effect of the grooves in the short cylinders would be, but it is plain that the same size of groove (a size usual in the construction of these cylinders) would remove relatively more metal. For this reason we have, in our two sets, made the grooves very light. We have done little more than score the surface. The grooves were made with a standard 60° thread tool, which was let into the surface about 0.01 in. Such a groove keeps the thread suspension in place; the cylinders can swing violently in the longitudinal direction without the loops slipping.

Our actual sets of cylinders were constructed from the cold rolled steel used for rod Z.[3] We used the shortened Z for a standard, *i.e.* the length of the standard was 431.0 mm. We are convinced that this measurement is correct to 0.1 mm. for we measured it, using end-stops and a lens, on a standard steel scale graduated to half-millimeters. We calibrated it by beats against a Koenig fork (256 d.v.), a Western Electric audiometer, type 2A, set for 256 d.v., and the Stern variator, whose adjustment had been altered since the previous calibration. The results, accurate to within a third of a beat per sec., were as follows:

Standard of reference:	Koenig fork	Audiometer	Variator
Indicated frequency of standard rod: d.v.	506.6	510.3	515.0

The differences in calibration by the three standards were checked by observing that the standards beat with each other as indicated. The result from the variator can be discarded since the instrument is adjustable and is not an absolute standard. The audiometer beat with the Koenig fork and it is impossible for us to say which, if either, was correct. We computed our sets by reference to the Koenig fork. If the audiometer is correct, our calibrated frequencies should be multiplied by 1.0073. With the diatonic series this uncertainty is not important, since only the relative frequencies are of primary interest. With the fine series for the determination of limens the error introduced may be more than 100 d.v., *i.e.* more than half the increment between adjacent cylinders. It is less, however, than the probable error of the limen,[4] and is of course relatively as accurate as accepted standards for the middle range, like the Koenig forks.

The constant of this steel stock, based on the Koenig fork, is $k = fl^2 = 94{,}106{,}523$. This gives for the extremes of each series the following lengths:

Diatonic series	Liminal series
g^4, 3072 d.v., 175.0 mm.	16000 d.v., 76.7 mm.
g^7, 24576 d.v., 61.9 mm.	24000 d.v., 62.6 mm.

All the cylinders, except the longest of the diatonic series, were measured with a micrometer caliper and are accurate to less than 0.1 mm.

In the liminal series the function of the lengths deviates so little from linearity, that two diverging straight bars provide adequate suspension. In the diatonic series it was found better to bend each bar slightly in the middle so that the curve of lengths is approximated on each side by two straight lines.

In the use of the cylinders we have found no reason to believe that they are inferior to sets manufactured by professional instrument makers. It is apparent then that anyone can readily have a set made for himself and to his own specifications. All that is necessary is for him to secure lengths of a good cold rolled steel, calibrate a long bar by any standard acceptable to him, measuring its length accurately, compute the desired cylinders from these data, and then have a good machinist cut the cylinders on a lathe, using a micrometer to assure accuracy of length.

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[3] The steel used was cold rolled, open hearth, 13/16″, round, steel shafting, designated as SAE 1020 by Edgar T. Ward Sons Co. of Boston, who supplied it.

[4] Cf. E. F. Möller, this JOURNAL, 33, 1922, 573 f. For example, if the data for all intensities be combined and Urban's procedure be applied, we get for observer M, her most precise *O*, the upper limen = 18,319 d.v.; h = 0.003076; and hence P.E. = 155.d.v. With observer B, if the inversion, although significant, be left in, we get the limen = 18,844 d.v.; h = 0.000956; P.E. = 499.d.v.

PERSONALITY AS REVEALED BY THE GALVANOMETER

By Edwin G. Fleming, Columbia University

In attempting to find an objective measure of personality the galvanometer was suggested as a possible means of obtaining an index. The skin resistance was obtained from 18 *S*s, all of whom were graduate students of at least one year standing in the department of psychology at Columbia University. These values were then correlated, because of the want of a better criterion, with the subjective ratings of four 'judges,' all of whom knew the *S*s intimately. Three of the judges (L, G, and W)—members of the faculty who had come, in the class room, the laboratory, and in private conferences, into close contact with the *S*s—ranked them upon the basis of their 'magnetic personality,' and 'nervous temperament.' The fourth judge (S)—a fellow student of the *S*s—ranked them only upon the basis of their 'nervous temperaments.'

The instructions were given to the judges in writing and were as follows: "Please rank the following students on the basis of 'magnetic personality,' placing the most 'magnetic' at the top; and also rank them on the basis of 'nervous temperament,' placing the most nervous at the top." Then followed the names of the 18 *S*s in random order. None of the judges, at the time of making the ratings, knew the values of the galvanometric readings which had previously been secured.

The initial resistance, measured in ohms, of the skin to the passage of a current from a single dry cell was secured for every *S*. All readings were taken at noon just before lunch. The electrodes were physiological salt solutions into which the *S* placed the index and the second finger of the right hand. The *S*s were ranked upon the basis of these readings—the first position being given to the *S* whose resistance was the highest. Correlations were then computed by the rank method between the resistance ranking and the rankings given by every judge individually and by all combined. The method was to add the rankings of the several judges, and to establish from these totals a new rank order. Correlations were also calculated between the rating of the same judge and of the different judges on 'magnetic personality,' and on 'nervous temperament.' All the correlations were converted into the Pearson r equivalents.

The following tables show the results in terms of Pearson r equivalent of *rho*.

Table I

Correlation between Electrical Resistance and 'Magnetic Personality'

	Judges				
	L	G	W	S	ALL
r	.67	.22	.28	—	.44

Table II

Correlation between Electrical Resistance and 'Nervous Temperament'

	Judges					
	L	G	W	S	ALL	L, G, and W
r	.53	.31	.14	.32	.40	.35

TABLE III

Inter-Correlations between the Rankings of the Judges on 'Magnetic Personality'

Judge	L	G	W
L		.66	.65
G	.66		.66
W	.65	.66	

TABLE IV

Inter-correlations between Rankings of the Judges on 'Nervous Temperament'

Judge	L	G	W	S
L		.66	.65	.27
G	.66		.67	.73
W	.65	.67		.47
S	.27	.73	.47	

TABLE V

Correlations between Ratings of 'Magnetic Personality' and 'Nervous Temperament'

	Judges			
	L	G	W	ALL
r	.47	−.36	−.34	−.18

A comparison of the rankings of the judges with one another and with the ranking in accordance with initial resistance shows, with the exception of the rankings of a few *S*s, a close agreement. The average difference in the final combined ranking of the three judges on 'magnetic personality' and the ranking according to initial resistance is 4.3; one of the differences is 13 and another 9; while eight of the rankings show a difference of not more than 2. It was, therefore, thought possible that a higher correlation might be obtained with the judges' rating of 'magnetic personality' if the *S*s were ranked in accordance with the deviation of their initial resistance from the average. But this method yielded a Pearson r equivalent of *rho* by the rank method of only .30; whereas the correlation when the *S*s were ranked with the highest resistance at the top and the lowest resistance at the bottom was .44.

By the method of partial correlation, when the combined rating of 'nervous temperament' of the three judges, L, G and W, is held constant, the correlation between the initial resistance and the combined estimate of 'magnetic personality' is increased to .56. When 'magnetic personality' is held constant, the correlation with 'nervous temperament' is .53. Using the method of multiple correlation, the correlation between initial resistance and 'magnetic personality' and 'nervous temperament' taken together is .65.

It is needless to say that this study is not conclusive. That is obvious from the fact that so few cases have been used. It is highly suggestive, however, especially since the ratings of the judges show correlations indicating that they understood consistently what was meant by 'magnetic personality' and 'nervous temperament.' The correlation of .44 between estimates of 'magnetic personality' and initial resistance to the passage of an electric current is something of a surprise, and will possibly repay further investigation. As high a multiple correlation as .65 between initial resistance, and 'magnetic personality' and 'nervous temperament' combined, is still more of a surprise, and if corroborated will be quite significant in the study of personality.

MINOR STUDY FROM THE PSYCHOLOGICAL LABORATORY OF PRINCETON UNIVERSITY

Communicated by H. S. Langfeld

A Study of the Phenomenon of Apparent Movement

By Herbert G. Vogt and William Grant, Jr.

History. This experiment is a continuation of the well known work on apparent movement. The history of the experimentation on apparent movement and the so-called phi-phenomenon is given in recent articles by Dimmick, Higginson, and Helson.[1]

Following Wertheimer,[2] Dimmick in 1920 studied the phenomenon of the illusion of movement produced by non-moving stimuli. This investigator characterized the various movements as, "bi-membral," "uni-membral," and "intra-membral." In his analysis of the experience he reports that movement, optimal in all but a few cases, was reported by all *O*s for all colors and backgrounds when the instructions were "to characterize." Under the descriptive attitude there was noted a gray patch covering the space between the two members. This gray flash he reports is the psychological correlate of the perception of movement.[3]

Higginson in a later study attempted to resolve the opposed conclusions of Dimmick and Wertheimer.[4] Among the deductions which Higginson draws on the basis of his own experiments are the following: "It is impossible to set up a significant correlation between any one temporal interval and full-movement." "The fundamental assumption of Wertheimer's physiological explanation: namely, that the *phi-function* requires a determined and specific temporal course, does not stand experimental test." "The significance of a visual gray of a certain durational course, alleged to be the psychological correlate of visual movement, appears in the light of our experiments to have been exaggerated."[5]

The present study of this phenomenon was undertaken with a different apparatus and under somewhat new conditions.

Apparatus and Method. The apparatus used in this experiment was an adaptation of that described by Carmichael.[6] The slide which was used in the apparatus was arranged so that two circles of light 6 mm. in diam. and 5 cm. apart were exposed in immediate succession as the slide was dropped. The slide was actuated by gravity and, since there was very little friction, the speed was sufficiently constant for the purposes of the experiment. Wertheimer's optimal time interval of 60σ between the two exposures was used.[7] This time was determined by means of a tuning fork.

The following filters were used: red, green, red and green, blue and yellow. The colored filters were placed over the openings in the slide and gave the desired colors to the circles of light as they were exposed, *i.e.* top red and bottom green; top green and bottom red; top blue and bottom yellow, top yellow and bottom blue. In another condition of the experiment the filters were not used and the dim yellow light from the box was exposed in two successive circles.

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[1] F. L. Dimmick, An experimental study of visual movement and the phi-phenomenon, this Journal, 31, 1920, 317-320. G. DeV. Higginson, The visual apprehension of movement under successive retinal excitations, this Journal, 37, 1926, 63-75. H. Helson, The psychology of *Gestalt*, this Journal, 36, 1925, 498-519.

[2] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch.f.Psychol.*, 61, 1921, 161-265.

[3] Dimmick, *op.cit.*, 330-332.

[4] Wertheimer's theory is physiological, and Dimmick's psychological.

[5] Higginson, *op.cit.*, 113.

[6] L. Carmichael, A device for the demonstration of apparent movement, this Journal, 36, 1925, 446-448. The slide used was an adaptation of that shown on the right hand side of figures IVa and IVb.

[7] Cf. Wertheimer, *op.cit.*, 178.

The experiment took place in a dark room in which only the fixation-point and the circles of light were visible to *O*. Eye-movements were partially controlled, and the field of vision restricted to a small area, just including the two stimulus dots, by requiring *O* to look through two black tubes, each one of which was 1 in. in diam. and 7 in. in length.[8] A fixation point was attached to the apparatus in the center of the space between the two dots. It consisted of a pin-hole of light. This light became visible when the slide was at its starting point with neither of the circles exposed and was extinguished automatically as the slide was dropped. There was, however, a sufficient interval between the extinguishing of the fixation point and the exposure of the first circle of light to eliminate the possibility of the former light influencing the result of the experiment.

Before every sitting each *O* was given the following instruction: "You will be presented with a series of illuminated dots of different colors and of different color combinations. Before each different presentation 5 preliminary exposures will be given you during which no report is requested; but rather, you are asked to attend to the experience at hand in order to familiarize yourself with what is taking place. After these preliminary exposures, a series of 5 similar ones will follow, after which an introspective account of what you have observed is desired. In order to enable you to fixate the field a small light will be given you before each exposure. This

TABLE I

RESULTS OF SERIES A AND B

O	No M	U′	U″	Bi	De	TPM	FM	Suc	Sim	Str	Intr
C	11	29	7	19	17	72	4	66	4	3	8
N	43	22	0	4	2	28	0	61	9	4	2
H	14	9	19	23	6	57	1	68	2	20	9
L	6	7	9	48	0	64	0	70	0	47	0
Gi	30	29	6	6	6	47	1	64	6	8	21
S	16	28	9	17	8	62	0	69	1	7	14
L	34	20	1	11	2	34	3	66	4	5	5
V	19	10	2	37	0	49	2	68	2	31	15
G	24	10	3	26	2	41	4	65	5	7	1
B	9	32	24	18	1	75	1	69	1	14	14
Totals	206	196	80	209	44	529	16	666	34	146	89
Av.	20.6	19.6	8.0	20.9	4.4	52.9	1.6	66.6	3.4	14.6	8.9
M.V.	8.6	10.6	4.7	11.8		13.2	1.0			7.8	5.7

KEY TO SYMBOLS

No M = No movement.
U′ = Uni-membral movement (movement of upper member down).
U″ = Uni-membral movement (movement of bottom member down).
Bi = Bi-membral movement (movement of both members down).
De = Delta movement (movement of bottom member up).
TPM = Total partial movements.
FM = Optimal or full movements.
Suc = Successive.
Sim = Simultaneous.
Str = Streaks.
Intr = Movement within circles, without other types of movement.

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[8] By the use of this device eye-movements were much restricted. Incidental observations without the tubes suggest, as do certain introspective reports, that the eye-movement factor as a condition of the phi-phenomenon has been too much neglected. Restriction of eye-movement certainly seems to reduce the number of 'full movements' reported.

will be extinguished before the experiment begins. Should you fail to see this fixation point, notify the experimenter. In your introspective report give the nature of the experience."

Following the reading of the instructions the subjects adjusted the tubes to their eyes and the lights were extinguished.

The first series presented (A), consisting of 10 exposures, was as follows: (1) both discs green; (2) both discs red; (3) top disc green, bottom disc red; (4) top disc red, bottom disc green; (5) top disc blue, bottom disc yellow; (6) top disc yellow, bottom disc blue; (7) ordinary light. The second series (B), also with 10 exposures, was as follows: (1) ordinary light; (2) top disc yellow, bottom disc blue; (3) top disc blue, bottom disc yellow; (4) both discs green; (5) both discs red; (6) top disc red, bottom disc green; (7) top disc green, bottom disc red. A total of 140 were thus given to each subject, making 1400 for the group. Only the downward motion of the slide was exposed. When the slide was raised the stimuli were covered.

Observers. The *Os* who acted as subjects for this experiment Dr. H. S. Langfeld (L), Dr. Leonard Carmichael (C), Dr. W. S. Hulin (H), Dr. S. C. Dodd (D), Miss Marion Nagle (N), Mr. C. W. Bray (B), Mr. J. J. Gibson (Gi), Mr. H. Schlosberg (S), Mr. H. G. Vogt (V), and Mr. W. Grant (G).

Results. The two series of observations which are brought together in Table I were also tabulated separately, and the correlation between the rank order of the averages of each sort of report in each series was secured by means of the Pearson product moments method. The result showed a correlation of 0.945. This finding suggests that the sampling was adequate.

In the classification of the various types of movement found in this phenomenon, the terminology of Dimmick and Higginson has been followed. The attempt was made to analyze the constituent elements of the experience into "uni-membral," "bi-membral," "intra-membral," and "delta-movement." The coefficient of correlation was also calculated between the rank order of the subjects according to the number of experiences termed "streaks" and the rank order of the same subjects according to the experiences reported as "bi-membral movement." This correlation was high (+0.89). Most optimal movements were reported when the stimuli were of the same color. The *Os* reported 13 full-movements under these conditions, 5 full-movement experiences with both dots red, 5 with both dots green, and 3 when the color of both dots was of the direct yellowish light from the illuminated lamps. Only 3 experiences of full-movement were reported when the two stimuli were of different colors, 1 in the red-green, 2 in the blue-yellow.

The *Os* described the movements variously as: "streaks," "a connective bar of light," "a brightness between the light," "a filmy pathway," "a downward flash of light," "a connective film," "a vague film," "a flowing pathway." Frequently the lights "did not appear as dots, and there was a mere band of color which started at the top dot and moved downward all or part of the way." In the latter case a second band would commence after a stretch of darkness. The color of the experiences reported as streaks seemed to depend largely on the color of the stimulus circles. In most cases the streak was reported as somewhat lighter in hue, for example, as "a pale green," "a faint red," or "a dim yellow." In a few instances the *Os* reported a "grayish streak." The extent of the streak varied from a "slight downward flash" to "a moving pathway of light from the upper to the lower dot." Usually when the two differently colored stimuli were presented the accompanying streak was only of one color resembling either the upper or the lower dot. Two streaks of different color may be observed and H reported as follows: In the red-green series, "the green was displaced downward in a sort of flare and then directly below the green, but in nowise growing out of the green, there flashed a red streak downward with increasing solidity and brightness ending in a red circle." The intra-membral movement was reported variously as "flaring," "quivering," or "shimmering" within either stimulus.

Conclusions. (1) Under the conditions of this experiment only a very few optimal movements were reported by the *O*s.

(2) The results show that the optimal movement experience when reported occurs more frequently when the two stimuli are of the same color than when they are of different colors.

(3) An experience of a streak between the two stimuli seems to be a consistent factor in the movement phenomenon. The color of this streak depends upon the color of the stimulus.[9]

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[9] Cf. the results of G. D. Higginson, Apparent visual movement and the *Gestalt:* II, The effect upon visual movement of colored stimulus objects, *J. Exp. Psychol.*, 9, 1926, 239-252. In this paper, which appeared after the present study was ready for publication, Higginson presents certain results which are in agreement with those given here.

BOOK REVIEWS

G. Stanley Hall: A Biography of a Mind. By LORINE PRUETTE. New York, D. Appleton & Co., 1926 pp. xi, 267

Any biography of the founder of this JOURNAL and the teacher of so many of its readers is certain to be of interest. But this is no ordinary biography and must therefore be of interest to a much wider circle. As its subtitle indicates it is not so much an account of the life of G. Stanley Hall as it is a biography of his mind. This was rare good judgment. What he wrote needed no amplification, what he did had been recorded, but of the vicissitudes of his mind there was no depiction. This selection of material has resulted in a volume which is strikingly coherent and impressively intense. To the critically minded the wholesome influence of the modern movement in biographic technique as represented by Strachey, Maurois, and Bradford will be approvingly recognized.

The reviewer perused the book with intermingling waves of anger, resentment, sadness and delight, and closed it to wipe away a tear. It brought back to his mind, as it most certainly will to all Clark men of the old days, scenes he had long forgotten or would like to forget, memories which he cherishes, and a renewed consciousness of the sadness and isolation of that youthful spirited, grave, old man in the up-stairs study across the street where the light seemed always burning no matter how late the hour. One can never recall merely the Stanley Hall of the lecture room and the seminary. His mercurial behavior, the skill and the clumsiness with which he handled his student and faculty and scientific contacts makes Hall the man dominate the memories of those who knew him. With such memories it is a question if any one who was ever intimately associated with Hall is fitted either to write his biography or to review it. But it may be likewise questioned if any one who never had the compelling experience of his intimate acqaintance should attempt his biography.

This volume opens with a brief but excellent characterization of his life as a whole. The first chapter is centered about that youthful experience on a hill-top in western Massachusetts with which readers of *Adolescence* and the *Life and Confessions* are already familiar. It is rightly presented as the orienting feature of his life. He always believed that the experiences of youth shaped the course of maturity. Certainly it was true of his own life. Then comes the chapter on "Hall as a Puritan." He was a New Englander proud of his ancestry and in him the sternness, family pride, hardihood, reserve, and essential religiosity of the typical New Englander found expression. But Hall was not entirely true to type, and this is recognized in the chapter entitled "The Conscious Pagan." There was a frank enjoyment of life and a seeking after thrills which more than once pushed him into dubious if not actually dangerous situations. The author has tried to present this, many will think inadequately, but perhaps it is well that it should be minimized because the puritan in Hall always quickly reacted to it and sought to prevent the world from discovering it. Under the caption "Son and Lover" the story of his domestic relations is told. The beauty and tenderness of his love for his mother and the lifelong influence of that love is delicately revealed. The tragedies of his domestic life, the loss by death of his first wife and little daughter and the loss by mental disease of the second wife, are tenderly written. Those who knew him or his work know how thoroughly his was a pioneer soul. *Bahnbrechend* was one of his favorite terms. His writings and researches are here characterized as a work of 'horizon-breaking.' For the psychologist who is first of all interested in systems and interpretations the chapter quaintly entitled

"Cousinship to Peter Whiffle" will be long valued and frequently used. Hall had so often promised a huge book on emotions even up to the last weeks of his life it was but natural to suppose that the nearly completed manuscript would be found among his papers. Then, too, there was that startling course of lectures on the psychology of digestion and nutrition which seemed to indicate a brilliant collection of material deserving publication. But when his papers were examined all that could be found was a vast and bewilderingly chaotic mass of abstracts and random notes. The book on the emotions was not yet on paper in a form which any but Hall could organize and he had gone. The present reviewer was privileged to examine with the hope of publication all the notes used in those lectures on digestion and was forced to come regretfully to the conclusion that there was nothing publishable. Only from the notes of some careful student would it ever be possible to obtain the general scheme which he had in mind for those great topics. In this chapter Pruette has brought together and systematically presented the available notes on those two lecture courses. In other chapters the criticisms of other scientists are summarized, his religious thought is touched upon, and there is a happy application to himself of those measures of a man which he suggested in his *Life and Confessions*. Perhaps the best writing from a literary point of view is to be found in the last two chapters. His attitudes and interests and emphases after retirement from the duties of his chair and his presidency reveal how as an old man he looked at death and was not afraid. The last days are feelingly presented as the "Passing of the Playboy."

Where there is so much to praise one does not like to mention possible defects. Many will undoubtedly say that this is but a feminine view of Hall. But what if it is? There was much in Hall which a woman could best understand and appreciate. The harshness of Hall, the meanness, the selfishness, do not receive the emphasis which they should. While the pathos of his life is admirably presented it is perhaps too well done, because the reader will inevitably find the pathetic giving a sombre cast to all the rest. The humor in Hall, especially the delight in good jokes on himself, is lost sight of. It is also to be regretted that the author did not include a description of the physical appearance of the man. Perhaps it would not have been quite germane but a chapter on his physical appearance in the lecture room, on the street, with his automobile, running up the stairs in his house or in the university when late for a lecture, written by a hand so skilled would have been a lasting pleasure. Many, too, will wonder why the author thought it necessary to have a biography of G. Stanley Hall introduced by Carl Van Doren, a name but vaguely known if known at all to many who will read this book. Has the name of G. Stanley Hall been so far forgotten that it must be so re-introduced? But these are minor points in a book which is certain to live not only in the working libraries of his students and of all psychologists but also in the world's biographic literature for its interpretative and stylistic skill.

University of Oregon EDMUND S. CONKLIN

Behaviorism. By JOHN B. WATSON. New York, People's Institute Publishing Co., 1925, pp. 251.

"This volume," the author states in his Preface, "is designed for the use of undergraduates. While it is written in a somewhat free and easy style, every effort has been made to present facts in unmutilated form and to state theoretical positions with accuracy." It is the reviewer's judgment that the book is too airily off-hand and sketchy, as well as to dogmatic and combative, to be very useful as a textbook. Most of the chapters are much better adapted to the purpose which they originally served, as lectures delivered before a rather popular audience at The People's Institute.

In the first paragraph of Chapter I the author takes what some will think is an unfair advantage of the reader's natural human desire to be up-to-date, by indentifying behaviorism with the "new" psychology and all other psychology as the "old." Behaviorism is defined as holding that "the subject matter of human psychology is the behavior or activities of the human being," while "all schools of psychology except that of behaviorism claim that 'consciousness' is the subject matter of psychology." There is no suggestion of a psychology which would include both consciousness and behavior, or any intimation that there is any other brand of behaviorism than the author's own. This omission seems hardly in harmony with the author's avowed intent "to present the facts in unmutilated form" and "to state theoretical positions with accuracy."

In his discussion of the religious background of current introspective psychology, the author implies that the psychologist who would go beyond the study of behaviorism and concern himself with mental imagery is to be classed with the voodoo doctors or other believers in primitive superstition. The dualistic position of Wundt is characterized as "pseudo-science." Sensation, perception, image, desire, purpose, "and even thinking and emotion as they are subjectively defined," are in the usual militant fashion of behaviorism swept aside as "mediaeval conceptions."

Chapter II gives numerous concrete illustrations, interestingly presented, of how the behaviorist goes about his work. The methods of investigating the conditioning of responses and of stimuli are described. Chapters III and IV describe the human body under the two heads: (a) structures that make behavior possible, and (b) the rôle the glands play in everyday behavior.

Chapters V and VI answer the question whether there are any human instincts. Here the author denies not only the reality of instincts, but also the reality of innate "capacity, talent, temperament, mental constitution and characteristics." The evidence offered by the geneticists is thrown out of court on the ground they "are working under the banner of the old 'faculty' psychology. One need not give much weight to any of their present conclusions." "Give me a dozen healthy infants, well formed, and my own specified world to bring them up in, and I'll guarantee to take any one at random and train him to become any type of specialist I might select—doctor, lawyer, artist, merchant, chief and, yes, even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors." "Let us, then, forever lay the ghosts of inheritance of aptitudes, of 'mental' characteristics, of special aptitudes (not based upon favorable structure such as throat formation in singing, hand formation in playing, structurally sound eyes and ears, etc."

No factual data are presented in support of the above astonishingly dogmatic statements. One may well doubt whether such questions can be settled "forever" by a mere scratch of the pen. However, one can only admire the author's confidence in his particular brand of psychology, and if he can only show us that it is capable of supplying formulas which will manufacture any kind of genius out of any kind of human material, he should have little difficulty in quickly converting the whole world to behaviorism. Think how different our civilization and cultural status would be if our ten million or so dullest adults who have never been able to master eighth grade reading and arithmetic, or even to unravel a spoken sentence with more than thirty words and two clauses, had all been made into Aristotles, Shakespeares, Newtons, Raphaels, Leonardo da Vincis, Michel Angelos, Geothes, Beethovens, Darwins, Galtons, Clerk Maxwells, Pasteurs, and Einsteins!

Chapters VII and VIII are given over to the emotions. They are in the main an informal account of the author's own experiments and observations on the emotions of infants, and for this reason they are the best chapters in the book. Factual data here have their one inning, in contrast to the more "verbal type of response" which characterizes the greater part of the book.

Habit formation and the psychology of learning are disposed of in the twenty pages of Chapter IX, "Our Manual Habits." Chapter X concerns "Talking and Thinking." The title is followed by the words, "which, when rightly understood, go far in breaking down the fiction that there is any such thing as mental life." For Watson, "what the psychologists have hitherto called thought is in short nothing but talking to ourselves." The arguments in support of this thesis are in the main a repetition of those the author has expressed in his other writings. In this chapter is the only spectific citation of experimental literature which the reviewer has found in the entire volume—an article by Miss Thorson on the relation of tongue movements to internal speech. The next chapter "Do We Always Think in Words?" modifies somewhat the extreme position above stated. The thesis is that "one's total organization is brought into the process of thinking,"—that "manual and visceral organizations are operative in thinking even when no verbal responses are present," and that "we could still think in some sort of way even if we had no words." "We think and plan with the whole body." Even the acquisition of manual skills, we learn, is as much a matter of language training and visceral training as it is a matter of muscular training (p. 204). One can only regard it as unfortunate that we have so little experimental data on the possibilities and limitations of training for skill in the case of pathological subjects who have lost the power of speech or defective subjects (*e. g.* untrained deaf mutes) who have never acquired it. On the theory that verbalization is so important in the acquisition of motor skills it would seem a little difficult to explain the extraordinary feats of skill performed by many animals, such as deer, monkeys, birds, bees, etc. The author holds that infantile experiences are not repressed by the subconscious, as the Freudians think, but that they are lost simply because they have not been verbalized. This is an interesting speculation, but one that is difficult to prove or disprove. The reviewer indeed feels that a great many of the tenets of behaviorism, according to the gospel of Watson, resemble Freudian theories in their relative inaccessibility to the experimental approach.

The last Chapter, XII, is devoted to personality. In this realm, as in that of intelligence, the myriad of individual differences which we find are all acquired—"all healthy individuals . . . start out equal." The signers of the declaration of independence "would have been strictly accurate had the clause '*at birth*' been inserted after the word equal." Personality is defined as "the end product of our habit systems," including, of course, the visceral. There are thousands of these habit systems that go to make up a total personality. (Incidentally, 'attention' is merely the "complete dominance of any one habit system.") At thirty years of age the habit systems are pretty well fixed and personality made once for all. Personality is best diagnosed by the study of educational history, achievement records, recreational habits, and emotional responses. Psychological tests are of but limited value for this purpose at present. The chapter closes with an interesting discussion of our inferiorities, our susceptibility to flattery, our strife for dominance, infant carry-overs, etc.

Behaviorism will prove very useful as a reference for acquainting the student with one point of view in psychology. It is lively and challenging, with not a dull page. The language is intelligible even to the average college freshman or the moderately educated layman—something which can be said of but few books on psychology. It can hardly prove serviceable, however, as a general introduction to the science of psychology; it is much too confined to the limited (if important) field which the author has himself cultivated. It gives the reader hardly a hint that work of any importance has been done by any other psychologist, although vague reference is occasionally made to investigations by Lashley.

In view of the extraordinary theoretical and practical importance of the experimental work which Dr. Watson inaugurated at John Hopkins, the reviewer lays down this book with a feeling of profound regret that an investigator so original and resourceful should have been lost to scientific psychology. Had his fruitful experimental work continued he would now in all probability be giving us experimental data of untold significance on mental development and learning instead of verbal fireworks on the merits of behaviorism and the idiocy of other psychological viewpoints.

Stanford University LEWIS M. TERMAN

Science and the Modern World. By A. N. WHITEHEAD. New York, Macmillan Co., 1925, pp. xi, 292.

A review of this book should attract new readers to it, and should at the same time to prepare them for an arduous experience.

There is little question that Whitehead's work is of first class importance. His studies are among those which give a new direction to scientific and philosophic thought. He is remarkable in being not only a most able mathematical physicist but also a serious student of European culture and a lover of the humanities. His theory suggests new prospects for the future of all the sciences, and is boldly constructive.

But there are also some intimidating facts about this book. Large parts of it are written with a most difficult abstractness; the sequence of the argument is not always clear; a fairly large new vocabulary is employed; whole paragraphs and pages, whole chapters, are as oracular and as difficult to grasp as if they came from Hegel. It would be a fortunate reader who did not experience frequent attacks of discouragement and impatience with so hard an exposition, and who did not find some haziness even in his final understanding of Whitehead's theory. Fully half the book is laborious and difficult, nevertheless the labor is profitable. Most readers will feel that Whitehead's thinking is difficult to follow largely because it is in fact an attempt to re-shape the fundamental concepts in modern science. Part of the reader's effort is like that of climbing; and at least part of the book's cloudiness is such as one is likely to find in the view from a height.

Whitehead attacks the materialistic postulates which have ruled in the science of the last three centuries, and sketches an alternative conception of nature, employing a different set of ultimate concepts. He gives grounds for discarding the traditional belief that physical reality can be analysed into substance units of some sort, and that all natural happenings are merely mechanical rearrangements of these. Mutually independent units are abstractions which have been justified heretofore by their usefulness in the development of science, but which are now proving clearly inadequate for the more complete understanding of nature. In their place the primary concepts which Whitehead adopts are those of interdependent events, unlimited fields of influence, organic processes of various degrees of complexity and endurance, "eternal objects" such as the abstract qualities of sensory experience, and an underlying "substantial activity."

Mechanism he rejects everywhere in nature. This was an abstract theoretical construction which earlier scientists developed along with their materialism, and which for a time had a great pragmatic value. But mechanism is quite undeserving of the metaphysical prestige it has acquired even among biologists and psychologists. Configurations and unitary patterns in natural events are given as much objective importance as an *Gestalt* psychologist could wish. The theory of organic evolution is extended in the widest possible manner: it becomes an essential part of every natural science. The modern mathematics of functions and the new relativity physics are drawn upon for evidence that we must revise our beliefs as to the nature of concrete reality. Individual human experiences are brought into a general theory of nature which considers psychological occurrences to be only special cases of a certain fundamental type of event. Vitalism is discarded along with dualism and mechanism and mechanical determinism. The individuality of every part of reality is emphasized, and also the half fictitious character of the scientific generalizations which leave this individuality out of account and deal merely with certain abstracts aspects of things. The full reality itself must be otherwise conceived.

Whitehead writes primarily as a student of physical science, but hardly less as a man who is sensitive to the esthetic and the humanitarian interests of life. In arguing that we ought not to attribute any importance to materialism and mechanism as ultimate truths about nature, he accordingly employs two different sets of observations. There is the fact that a large part of our actual experiences, especially our enjoyments and our valuings, are so obviously unassimilated by that kind of metaphysics. There has been a genuine conflict between the views which thoughtful men have believed science required them to adopt and the other aspects of human life which find expression especially in the arts. To some extent the dominance of the scientific doctrines has been an injurious factor in the social and cultural development of our time. But, quite independently of all this, the recent increase of knowledge in physical science itself has proved the necessity for a new conception of reality. It was Whitehead's work in mathematical physics which convinced him of the final defectiveness of the classical mechanism-theory. A form of relativity physics must be adopted instead, not quite of the Einstein variety, but one with just as difficult a doctrine of space-time and the continuum of events, and just as destructive of the beliefs we have been accustomed to accept from the physicists.

Not substantial particles, not even units such as electrons, but events or 'occasions' are the ultimate concrete facts. Reality itself is indeed one event. By abstraction, however, we can discriminate lesser events which can be considered as its components. Certain trains of these events are, again abstractly, the more or less enduring things which are given so much importance in our ordinary thought and our traditional science. Here the essential fact is that a certain pattern is reproduced in the series, a relatively simple pattern in the case of an electron, and a complex one in the case of a human being. But a relocation of independent pieces is not the final truth about any real occurrence. "We must start with the event as the ultimate unit of natural occurrence." "The event is the unit of things real." Every occurrence, however, is dependent on its relations with all others. The relations of one event to others "are constitutive of what the event is in itself;" they are functional in the mathematical sense, and are "internal."

Even a unitary event is still capable of a different analysis. Here the task of the reviewer becomes difficult. Whitehead considers that into every event there enters a set of factors which he terms "eternal objects," and which he illustrates as "colors, sounds, scents, geometrical characters," and "relationships of these objects among themselves." Each of these remains identical and unaltered, however many events may include it at one time or another; but each of them stands in a set of relationships to all the others. The system of these eternal objects constitutes the realm of possibility. An actual occurrence is a certain finite "prehension" from this system; and, in the large, concrete nature, the real world, is such a selection. "The metaphysical status of an eternal object is that of a possibility for an actuality." With some of these objects, as united into an actual event, we have direct acquaintance. "The psychological field exhibits what an event is in itself." "The private psychological field is merely the event considered from its own standpoint." Individual human lives consist of a set of occurrences, among which some are cognitive; and the possibility of cognition depends on this, that the eternal objects which enter into events are related in certain ways to other eternal objects which compose other events—as for instance a certain color in a particular experience is related to the constituents of those other events which we call a candle burning at the other side of the room. Only because events are systematically inter-related and interdependent can experience be cognitive at all.

Finally the analysis of events requires a still more difficult concept, and one which receives too brief a treatment in Whitehead's book. There is a "substantial activity" which constitutes each real event by unifying the particular set of eternal objects which compose it. This activity is "not an entity in the sense in which occasions or eternal objects are entities." "It is Spinoza's one infinite substance."

The concept of a pattern and organization of events is of special importance, and Whitehead is already being quoted in support of *Gestalt* psychology. Physical science, he believes, has not less need than biology for the concept of organisms, in the sense of patterns of events which are self-repeating and self-maintaining in their interaction with their environment. The discontinuity which recent physics apparently requires in some of its primary objects is also provided, he suggests, in the repetition of simple pattern units, giving a series of wholes which are discrete even though the process within each unit is continuous.

As any part of a human organism is affected by its relations with all the other parts, so similarly any part of any enduring physical structure should be conceived as affected by its place in the larger pattern. It is, he thinks, an unfounded and mistaken supposition that the physical components of an animal body are exactly similar in behavior to those in other environments. This does not give any support to vitalism; but it does lead Whitehead to leave a place in his theory for the possibility that a process in one organism can be directly determined by a certain process in another. He seems to hesitate a little to declare for telepathy, for he avoids using the term in his reference to the subject; but he clearly implies that his organic theory of nature could allow for some phenomena of that sort.

In contrast to the hard sections of the book are the very readable first chapters which sketch the rise and development of science during the last three centuries, and the last chapters on the relation between religion and science. Whitehead draws upon an enviably wide acquaintance with the history of European culture; but one is left somewhat doubtful by his analysis of the psychological origins of the growth of scientific knowledge during the 16th century. Greek tragedy, Roman law, and scholastic theology may have had the essential preparatory importance which he attributes to them, but the argument is at best too brief to be convincing. Psychologists are not likely to approve his repeated remarks that these influences finally produced in many Europeans an "instinctive" belief in the set of doctrines which underlie modern science. In a later chapter, when his reference is really to instincts, he speaks of "the mere impulse of the various senses." But the somewhat casual psychology suggested by these phrases does not after all affect the theory presented in the book, and should be mentioned, even here, only in passing.

Amherst College CHARLES H. TOLL

Science, Religion, and Reality. By LORD BALFOUR, B. MALINOWSKI, C. SINGER, A. ALIOTTA, A. S. EDDINGTON, J. NEEDHAM, J. W. OMAN, W. BROWN, C. C. J. WEBB, and W. R. INGE. New York, The Macmillan Co., 1925, pp. 396.

Not only timely in the light of much recent discussion and legislation on matters of religious and scientific interpretation, but also important for all time in the cross-section that it cuts out of the stream of thought through the ages, is this delightfully conceived series of essays. The editor

(Needham) stipulated that the contributions "should be solid, but not too technical for the general reader," and that they "should not be directly apologetic in tendency." As Dean Inge concludes, the essays have very well met the specifications. They maintain a high level of scholarship revealed not only by ample historical citations but by cautious judgments in the face of controversial opinion or of insufficiently proved points under investigation. Besides two of the essays in particular, one on "The Domain of Physical Science" and the concluding one, display a fine sense of humor as well.

In the introductory essay Lord Balfour persistently searches for the point or area where conflict between science and religion might arise. Collisions do not come so much from the thought processes involved in both as from the emotional attitude engendered, but even when the sheer facts of science jar on our religious sensibilities we must not forget that the glory of the heavens which science reveals is itself born of our being. "If we are nothing, they are nothing." There are some, however, who regard the contradictions not emotional but factual and intellectual. In that case the Bible is often misunderstood as "a textbook of cosmology." The happiest solution of all these problems lies "provisionally in an unresolved dualism;" the world of science and the spiritual world are fostered by a double outlook on our experience; and both are real.

Under the title, "Magic, Science and Religion," the first writer gives a genetic account of the growth of magic and religion among primitive peoples. They are "not merely a doctrine or a philosophy . . . but a special mode of behaviour, a pragmatic attitude built up of reason, feeling, and will alike." Through intimate study of the Melanesians of the Trobriand Archipelago he concludes that the three factors make a three-cornered constellation. Where knowledge is inadequate, primitive emotions and instinctive reactions demand a satisfactory outlet and thus become crystalized into permanent traditional forms in magical rites and beliefs which in turn are fixed by religious faith. Singer in the next contribution traces the historical relations of religion and science. He begins by pointing out that the Greeks made large contributions to science and philosophy without possessing a canon of sacred literature. At another place he makes the startling statement that the Renaissance meant nothing more than a revival of *ancient* science. Roger Bacon was not regarded as a heretic by his contemporaries. It was later that conflicts began to arise. Step by step the author traces the progress of science until the present day. There are then four ways in which the man of science can envisage the interrelationship: he can use his science in the laboratory and study and his religion outside of these boundaries; he can disregard his religious attitude altogether; he can mystically separate the internal from the external experience; and in pantheistic fashion he can square himself with determinism by regarding his soul beyond his ken and yet in transcendental harmony with the world-soul.

The following essay in scholarly analyses outlines the progress of philosophical thought in connection with religion. The intellectual element is throughout an integral part of religion. Although fulfilling different functions in life, religion and science together approach the reality of things. Only an old-fashioned faculty psychology would permit of a relegation of religion to the emotional life and of science to the intellectual life of mankind. The essay on the domain of physical science, by witty illustration as well as by learned discourse, shows the limitations of the 'interventions' of science. Piece by piece the picture-puzzle is put together. Beyond this the scientist can not go. The philosopher is privileged to guess and the man of religion is bound to believe what the picture will look like when completed. Then there is a paper on mechanistic biology and the religious consciousness. Again in historical array the development of biological thought is presented. While" the bio-chemist and the bio-physicist, therefore, can and must be thoroughgoing mechanists," they need not hesitate to affirm on other grounds the existence of religious data. Contrasted with this discussion a paper follows on the sphere of religion wherein the author contends that religion is not reduceable to any mental feeling or attitude but to an outward reference that is dependent in turn on the assumption of a supernatural reality. Just as the natural world derives its reality from factors outside of the evidences of the senses, so the supernatural world, without sensory appeal, is grounded on values that have a similar origin.

In contrast to this the essay on religion and psychology takes the view that religion is a state of mind, a mental attitude with which we face existence in terms of cognition, feeling, and conation. This attitude the author analyses in regard to time and personality. The final essay in the series traces the larger aspects of science, Christianity and modern civilization. Science itself can not produce the values with which the achievements of science are appraised. These are the products of a Christianized world. Christianity in its place has learned much from the achievements of science, and has been made over by them.

The concluding summary by Dean Inge is a masterful writing. Anthropological and historical inquiry are elucidating but limited in scope. "It tells us nothing about Newton to know that he once had a tail. . . . We do not care much how it [religion] began; we want to understand it as it is or may be." Furthermore, while science can never be free from evolution, its main purpose is not to derive these values. Religion, which is not just another field of activity delimited from that of science, studies the essential values of goodness, truth, and beauty. The mystic attitude is not more emotional than intellectual, but a combination of mental abilities in which some individuals are more and others are less gifted.

The whole array of essays is a noteworthy achievement. At a time when scientists are accused of irreligion the writers all reveal not only a wholesome attitude toward the everlasting verities of life but a rational aspect to their convictions. There are minor differences of opinion in the explanations of things, but there is always a choice available even for the sensitive reader. The reviewer wishes that the volume find wide publicity. The format is attractive.

University of Iowa CHRISTIAN A. RUCKMICK

Logic: Part III. The Logical Foundations of Science. By W. E. JOHNSON. Cambridge (England) University Press, 1924, pp. xxxvi, 192.

The two preceding parts of this work deal with topics relevant to Formal Logic. In this volume the author is concerned chiefly with scientific or causal laws and with problematic induction. By 'problematic induction' is intended what is commonly meant by 'induction' merely; but Johnson recognizes other kinds of induction (see Part II).

At the beginning of the discussion the author distinguishes two kinds of universal propositions: those which express universals of fact, and those which express universals of law (chap. I, Fact and Law). If I say, 'Everything that has the character p has the character q,' I express a universal of fact; but if I say, 'With regard to every thing, if it had the character p it would have the character q,' I express a universal of law. The first proposition follows from the second but the second does not follow from the first—a universal of law entails a corresponding universal of fact, but not conversely. Universals of law, Mr. Johnson says, express *nomic* necessity (chap. I, 3d,). It is often held that any law of nature is simply a factual universal. But it is to be noted that we often make such judgments as, 'If this book were released it would fall,' which seem to be inferred from some law of nature, and this proposition does not follow from any merely factual universal like 'All bodies of a certain kind which are released fall.'

Moreover, in the case of universals which are certainly merely factual, such as 'Every book on this table is green,' we do not infer, for example, that 'if *that* book were on this table it would be green' or 'if that red book were on this table it would be green.' Nomic necessity, Johnson holds, is to be distinguished from factual universality.

As a deductive relation, we may have 'p implies q, and p, therefore q,' which is demonstrative; as an inductive relation, we may have 'p implies q, and q, therefore p,' which is problematic (chap. II). Induction is concerned with instantial propositions as evidence for the truth of a universal from which these instantial propositions follow. Now the force of the evidence for a universal is only indirectly dependent upon the number of examined instances. Indeed it is possible that the number of examined instances might be increased indefinitely without increasing the probability of a generalization based on these instances. It is only when the examined instances differ and agree in relevant ways that the probability of the generalization is increased. A new instance strengthens the evidence for a generalization provided it satisfies the generalization and also lacks some character, which all the other instances have in common. The evidence is further increased by the occurrence of two such characters provided the occurrence of neither of the characters necessitates the occurrence of the other, either logically or through some law of nature. It is not the number of characters but the number of independent characters that is important.

The author employs the term *eduction* to denote inference from instances to instances (chap. IV). Inference from a set of instantial data to some unexamined case is always more certain than inference to the corresponding universal, for the unexamined instance follows from the universal, whereas it might be the case that the universal is false and the instantial proposition true. Now it may happen that all of the evidential data are favorable to the truth of the proposition to be inferred, or it may happen that some of the evidence is favorable and some is unfavorable. The former case leads to an inductive generalization which is universal and the latter to an inductive generalization which is class-fractional (p. 47).

This is the kind of thing with which Johnson is concerned. He has also a discussion of the mind-body relation (chap. VIII) in which he applies his analysis of substance and causality.

Harvard University C. H. LANGFORD

Psychology and Education. By R. M. OGDEN. New York, Harcourt, Brace & Co., 1926, pp. ix, 364.

Since the advent of configurationism, especially since the work done by Köhler, most students of education have felt the need of applying the concepts the *Gestalt* school has evolved to some of their problems. Koffka's *The Growth of Mind* was essentially genetic; articles in different journals have been concerned with some one phase or another; no one book had been issued which purported to carry *Gestalt* to education.

The advent of this book by Ogden is an attempt to meet this need. Dedicated to Kurt Koffka, it is a wholesome attempt to read configurationism into most of the problems of educational psychology and thus show a *via media* for avoiding the pitfalls of both behaviorism and teleology. In his preface Ogden expresses the belief that he has been able "to give for the first time to the study of education a psychological background which commends itself as an orderly and systematic development of scientific principles derived from factual observation rather than from neurological tautologies."

The result is a fairly comprehensive study of the field covered in a course in educational psychology, a field which he has enriched by giving the results of his nine years of teaching such a course, and a critical survey of the experimental work bearing on the point of view he adopts. To him mind is not a passive thing transformed into a bundle of habits and pathways, but rather a functioning force capable of creative efforts and achievements, the powers of which are realized only when education liberates the child to follow his own will and intention.

Man's original nature is discussed in the first seven chapters in a manner that shows gleanings from every available source to substantiate the claim, which Ogden consistently makes, that original behavior of any form shows three characteristics: adjustment, differential sensitivity, and variation according to the needs of the organism. Sucking he analyses in detail to illustrate his contention. To him the most primitive form of behavior is an undifferentiated response of the whole organism. In its more developed form, tropistic, reflex, and instinctive behavior are seen centered around some object or situation, and showing always the features of avoidance or acceptance.

A lengthy discussion of the instincts is made on the general basis of River's classification into individual, sexual, and social instincts. Arguments for their existence are based on work done on animals as well as human beings, and apparently equal weight is given to each. The *Gestalt* influence is again apparent in his addition of adornment and cleanliness to the individual instincts, in his distinction between gradual and saltatory development under the sex instinct, and in a few pages devoted to integration under the social instincts. Imitation and play he discusses separately contending that both involve no unique patterns of behavior, yet show their original natures in their attitudes. "It is not the particular action of the others jaws which is imitated, but yawning in a generic sense" (p. 99).

What is really the second part of his book is prefaced by a chapter on "Situation and Experience," where an attempt is made to show the functional unity at the basis of both behavior and situation which makes all experience possible. "Just as the behaviour of an organism is always somehow patterned . . . so the situation which is the counterpart of behavior is also patterned" (p. 109). "The situation . . . is something which participates as a member of the organic pattern of response (p. 111).

Chapters on Perception, Affection, and Sensation follow. Here the influence of the *Gestalt* school is most apparent, although fused with it —especially in the chapter on Affection—the work of behaviorism is pronounced. Perception is not "a complex of sensory experience and associated imagery" but rather "the experience of a closed circuit of events—a satisfying experience—the satisfaction being found by finding an end appropriate to the beginning" (pp. 127 f.). Affection like sensation depends upon the dispositional ground from which a perception emerges. The educational application of these is well thought out in a study of the acquisition of skill under perception, in a repetition of the accepted value of emotion for learning under affection, and in a genetic development of the sensory qualities in the different sense departments.

The last half of the book is given to chapters on memory with a classification of the main laws and some classical experiments bearing on them, on learning in both the process and patterns, and on thinking, reasoning and the measurement of intelligence. Koffka's classification of the process of learning—particularization, differentiation, assimilation, gradation, and redefinition—he adopts, using Köhler's experiments on the chimpanzee to bear out his claim. The patterns of learning comprise for the most part a genetic study of word formation; thinking and reasoning duplicate Creighton's *Logic*; the measurement of intelligence is criticized after the manner of Boring's "What the Tests Test." Yet his using of others' material is only a gleaning from varied sources to substantiate his basic claim that the organism acts as a unified whole to a unified situation, the contours of which are dependent upon the temporary, internal, organic condition.

The book as a whole is a distinct contribution to education, and ought to serve as a good text for students in educational psychology with previous training in psychology. Its defects are of a minor kind; many of the sentences are unnecessarily involved; thoughts that could be expressed in one clear sentence are often juggled about in such a way that one has to read the paragraph many times before getting the meaning; references are mentioned without being indicated; and selections are chosen which confuse rather than simplify the point being discussed. Yet, despite these faults, the book presents a new point of view in a consistent manner, and offers a solution to many of the perplexing problems in education.

Harvard University HERBERT MOORES

Experimental Psychology. By MARY COLLINS and JAMES DREVER. New York, E. P. Dutton, 1926, pp. viii, 315, 27 diagrams.

This book is a combination introductory laboratory manual and textbook of general psychology; tending toward the former in its subject matter, and toward the latter in its popular mode of presentation. Its preface informs the reader that it is meant to serve as an introduction to more advanced textbooks of experimental psychology, but unfortunately the authors seriously detract from its usefulness by not treating adequately their references and bibliography. Only an occasional reference is given to current psychological periodicals despite the fact that the most recent and usually the most significant information is to be found in them rather than in textbooks. All the references that are given are without the initials of the author, the name of the publisher, and the date of publication.

An Introduction of 25 pages gives a brief discussion of the evolution of experimental psychology, the nature of the psychological experiment, the value and application of psychophysical methods, and the statistical treatment of data. This introduction would be improved if reference were made to the stimulus error, as well as to the uncritical use of the normal curve of error.

In the consideration of the senses, Vision is given the customary major portion of space. Occasionally there seems to be an attempt to 'impress' the reader by the use of imposing terms. Together with the three well known color theories it is surprising to find an exposition of the Eldridge-Green theory. In view of the fact that this theory has been so roundly criticized by such men as Parsons and Sherrington it is questionable whether it should be included. Hearing is orthodox except that a disproportionate amount of space is devoted to Watt's theories of consonance and hearing. The Chapter on Cutaneous Sensations is poorly done. Head's theory of 'protopathic' and 'epicritic' sensibility is accepted *in toto*, without mention being made of the experimental results opposed to it. Neither is Abbott's work on physiological zero referred to. Taste and Smell, and Movement and other Organic Sensations are, quite properly, disposed of briefly.

Perception is given only 32 pages. The authors believe firmly in *Gestalt* psychology, and assert that it "bids fair to revolutionize the psychology of perception" (p. 107). From their mention of the valiant fight made by the *Gestalt* psychologists against the error of the 'bundle hypothesis,' the reviewer infers that the authors hold to the doubtful assumption that there are still numerous adherents of strict sensationalistic psychology, and that the 'bundle hypothesis' exists as a controversial issue. The statement that the *Gestalt* psychologists have shown "how, and in what sense, perception is always perception of a single object, that is, of a whole" (p. 108), is ambiguous. What is meant by a 'single object' or a 'whole'? In the next chapter, on Attention, the authors state "we regard a house as a unit, and as a single object; yet it consists of an infinite number of parts, windows, doors, chimneys, etc., each in themselves units. The 'singleness' of the object, therefore, varies according to the purpose at hand.

An object may be one single thing or it may be a number of things grouped together and regarded as a single object" (p. 140). These two statements are incongruous; it is difficult to reconcile them. For a more critical evaluation of the *Gestalt* doctrines, sophisticated readers of Collins and Drever can now consult the articles by Helson which have recently appeared in this JOURNAL.

The authors seem to have included nothing in their chapter on Attention that has appeared since Titchener's *Elementary Psychology of Feeling and Attention*, 1908, in spite of the fact that a number of important experimental articles on the subject have been published since then. The range of apprehension, for example, can no longer, in view of the work of Fernberger and of Dallenbach, be said to be 4 or 5.

The section on Action is brief and on the whole satisfactory. Work and Fatigue, and Suggestion and Suggestibility are fields in which the authors are competent. The chapters of Feeling and Emotion, Imagery and Association, Learning and Memory are adequate. Imagination and the Higher Thought Processes, Language and its Development, and Mental Testing conclude the main body of the book. Considered as brief treatments, there is little fault to be found with these chapters.

From a pedagogical point of view the book has certain virtues. For the most part it is written in a clear, simple, and entertaining style. It stresses and demonstrates the importance of the experimental method; it mentions and describes the chief methods and apparatuses available for the investigation of various psychological problems; and it gives a simple and fairly adequate account of the result in various divisions of the experimental field. For these reasons the book should serve fairly well to assist the average first year laboratory student in becoming acquainted with the scope and technique of his subject.

Harvard University HARRY R. DESILVA

Experimente mit Raphael Schermann. By OSKAR FISCHER. Berlin and Vienna, Urban Schwartzenberg, 1924, pp. 200.

Those who remember the furore that Raphael Schermann's demonstrations in New York some years ago made in psycho-medical circles will be interested in this monograph by Professor Fischer of the German University in Prague.

Dr. Fischer who is a psychiatrist seems to have obtained some remarkable results with this extraordinary man who was able to characterize a person he had never seen from the handwriting, although not after the fashion of the so-called graphologists. Schermann is alleged to have told details about the subject's history, about his incipient ailments, about his circumstances, etc. Many are the experiments which were carried out, and the results cover a wide variety of phenomena, bordering on the veridical.

Perhaps the most puzzling achievement of Schermann is the imitation of the handwriting of a person that he had never seen. Dr. Fischer would visualize one of his acquaintances, and Schermann would produce a fair specimen of that person's chirography.

In all these experiments there were of course a number of failures, but the proportion as a whole is as 27% failures to 65% successes with 8% of uncertain responses.

What conclusion may be gained from these experiments? The author is forced to postulate an "extra-sensual transfer of psychic processes among human beings," which is mediated through some sort of cerebral energy. The *modus operandi* of this process remains a mystery. Furthermore, however, since Schermann could describe conditions which were not thought of by the experimenter—details of the past and present milieu which were not even known to him—when he was concentrating upon the external appearance of the absent individual, there is no alternative but to ascribe to Schermann the gift of clairvoyance or cryptaesthesia. Fischer discusses the question that possibly the transfer is of the subconscious, but counters this by the fact that he was absolutely ignorant of many of the particulars which the observer brought to light.

Clairvoyance or, as it is more technically known, *cryptaesthesia* would be explained on the basis of irradiation of a supra-sensual energy between object and organism. It is curious that although Schermann could give much information (mood of the writer, number of pages) about a letter written, only by fingering it, he could not guess one word of the contents. The author surmises that possibly a certain psychic energy, typical of each individual, attaches itself to the writing, and it is through this energy that Schermann is able to describe the writer.

For a reviewer to attempt to criticize a series of experiments so inaccessible would be folly; and there will always be the tendency on the part of the critical to say, "Since I was not present, I cannot accept the evidence," —an attitude of which the author is keenly conscious. To the reviewer's mind, it does seem an impossible feat for a human being to be able to tell that some one has married for money or is in straitened circumstances, etc., without having had the slightest information about him. Such omniscience would imply that all the experiences of a person, his past motives, and so on, are clinging to him not only in the form of effects (which can readily be understood), but also in their original configuration, so that when his external appearance is visualized, these in some way reveal themselves to the clairvoyant.

These experiments, it may be said, were conducted before Schermann visited the United States; and after 1918. Dr. Fischer was not able to sustain the coöperation of the observer, who had taken an aversion to the experiments. But there is the rub. It is so disconcerting to have to discontinue experimentation because of the attitude of the very person on whom all depends; and here is the great drawback of scientific investigation of the occult. In the physical sciences, results once achieved are held and allow of the possibility of reproduction, duplication, and expansion. Subsequent workers can always build on the foundations of their predecessors. Until we get to that stage in the region of the occult, investigators will always encounter the justifiable skepticism of those who are unable to verify the conclusions because of the resistance of the medium or other person crucial to the test.

Cambridge, Mass. A. A. ROBACK

The Ways of the Mind. By H. F. ADAMS. New York, Charles Scribner's Sons, 1925, pp. 336.

The chapters in this book are what one has come to expect in an elementary text in psychology. Emphasis, however, has been placed on the dynamic side as shown by separate chapters on Interest, Desire, Will, and Motives and Incentives. A promise of the Preface to give separate chapters to Originality and Intuition is not fulfilled; the first of these two topics is considered only in the discussion on Thinking, and the latter is entirely neglected—it is not even mentioned in the Index.

Though reproducing a large number of good cuts, 12 of which are taken from Howell's *Textbook of Physiology*, Adams has presented much detail that he does not make use of in the text. The elementary nature of the book appears in the over-simplification of the treatment of some of the psychological processes, arbitrary presentation of certain theories without indicating that there are rival theories, and a marked tendency to define.

The tendency to define has lead Adams into many inconsistencies. For example: association is described as "the most fundamental of all psychological principles" (81), and later he says, "it would appear that instead of attention being the unit of association the associative process is the unit of attention" (150). Incentives are "external conditions" which affect response (298). Primary attention "cannot be interesting, for it is a mere passive awareness without meaning, purpose, or future reference;" "attention both analyzes and synthesizes the parts of which a complex whole is made" (124). "Images are necessary in all thinking" (232). "Pleasure results from the activity of well nourished nerve cells (194). Strangely enough 'assent' is postulated as essential to all movements; yet "in the case of automatic, reflex, instinctive, and random movements 'assent' is understood, and the movement takes place in a purely mechanial way" (274).

The verbalism of the text is best seen perhaps in the chapter on Interest. A sample passage begins, "Interest, we see, is essentially a mental examination of objects . . . in which the self is not completely identified." Emotions "are called out by objects and situations having immediate and personal value" (218); but "Value . . . [is] an ideal as compounded with emotion."

The subject matter in most of the twenty-one chapters is presented in one, two, three order; consequently they are easy to outline. The chapters on the Nervous System, Sensation, Memory, Attention, Perception, and Fatigue are clear and adequate for such a book. Adams, however, seems most occupied with the question of the nature of interest. Textbooks of this kind make one question whether it is worth while to keep on re-writing the older theoretical psychology for students who should be getting a fresh glance at the living science.

Louisiana State University PAUL C. YOUNG

Arthur Schopenhauer als Psychologe. By RICHARD HOHENEMSER. Leipzig, J. A. Barth, 1924, pp. 438.

In a system of thought so highly integrated as is Schopenhauer's philosophy it is no easy matter to find the catalytic agent that will precipitate out effectively and clearly the psychological elements that are dissolved therein. The author seems happily to have discovered the agent. In a very comprehensive way he not only analyzes but expertly weighs the contributions made by Schopenhauer to modern psychology. The four chapters deal with each of the larger subdivisions: the doctrine of recognition, including apprehension and comprehension, judgment, association, memory, the comic, delirium, and dream; the doctrine of will which involves questions of instinct, mental inheritance, and magic; the doctrine of beauty as applied to architecture, painting, poetry, and music; and the doctrine of moral relations, with especial reference to the freedom of the will, to conscience, punishment, and the activities of society and the state.

Because Hume first gave a thorough-going mental interpretation to human understanding, he is recognized as the first great psychologist. Then comes Kant. He proved the *a priori* character not only of all perceptual activity but also of conation and pointed out their manifestation in art and ethics. The contributions of Fichte, Schelling, and Hegel are of little account psychologically since they either lost their psychological bearings in their search for the metaphysical or else followed courses that were parallel to but far removed from psychology.

But Schopenhauer hewed close to the phrase, "the world is my idea." Perceptual presentations were conditioned by objective data and by sensory limitations, but they were undoubtedly also at the mercy of the past experience of the *self*. At once we notice the dynamic power of conation and its influence on the mental life. Man is not to be regarded as a microcosmos, he thinks, but the world at large can be better understood as a macranthropos. So like Descartes and Berkeley before him, Schopenhauer studied in detail the processes and principles of human perception and thought in order the more adequately to comprehend the world at large.

In the reviewer's judgment, however, the author does not show clearly the danger of anthropomorphizing the universe without seriously trespassing on the intrinsic meaning of such concepts as *conation* or *will*. The author quite rightly defends Schopenhauer's notion of unrecognized "motives" in instinctive action, but fails to account for the magnification of the effort or drive of the human mind into worldly or universal dimensions. While Schopenhauer did psychology good service by portraying passive observation as active and dynamic at a time when logic still ruled with an iron hand in psychology, he seems to have overreached himself when he tried to convert all mental processes, and perhaps physical events as well, into a supreme voluntarism—even in the face of an outworn intellectualism.

The monograph is well organized and the material is handled with sympathetic insight into Schopenhauer's philosophy and with keen understanding of many modern tendencies. Where many treatises of this type become arid by meandering through plans of abstract phrases, the author sweetens his task with apt illustration and epigram.

University of Iowa CHRISTIAN A. RUCKMICK

Emotion and Insanity. By S. THALBITZER, translated by M. G. Beard. The International Library of Psychology, Philosophy and Scientific Method. New York, Harcourt, Brace and Co., 1926, pp. 127.

This book consists of a preface by Harold Hoeffding and four chapters devoted respectively to Introduction, Psychology, Psychiatry and Psychology, and Physiology.

The author treats the subject comprehensively but not exhaustively. The book is written in a style that can be understood by the average student of psychology.

The chief contribution to the subject is found in the chapter of Psychiatry and Psychology, where the author gives a lucid and comprehensive description of the various mental types found in insanity.

Professor Hoeffding very ably depicts the merits of the chapter in his preface where he says: "In two respects in particular the author has, I feel, made important contributions to psychology. In the first place there is the general conclusion at which he has been enabled to arrive by a study of the manic-depressive psychosis. He shows that within this we find every kind of feeling represented, even the most complicated, and on a more elaborate scale, so that each particular feature appears with greater clearness. We thus become aware of features which are not easily discovered in the observation of normal processes.

"Secondly there is his description and analysis of mixed forms at various levels of those mental states in which different feeling elements are in operation simultaneously and with opposite and contrasted effects, so that the elements are set off one against another."

Harvard University C. A. SELZER

La Psychologie Organique. By PIERRE JEAN. Paris, Félix Alcan, 1925, pp. 297.

This, the first of three proposed volumes under the general title *Theorie de la Vie*, takes up the reactions of vegetables and of animals without a nervous system. The second volume will deal with "neural psychology," the third with "heredity." For the author all the functions of the most highly developed nervous system are not only potentially present in organisms without a nervous system, but are actually present for such organisms, in perfect form. Neural psychology is simply a specialized form of the psychology (organic psychology) which deals with the reactions of all the cells which are not nerve cells.

The main thesis of the book is that 'will' and 'choice,' in the sense of making a selection of possible responses in order to attain certain ends, are fundamental categories of a genuine psychological determinism. This 'will,' however, is not free but is determined by the needs of the organism. "Et le besoin, c'est la psychologie" (p. 271). In the author's opinion physical-chemical theories of tropisms, 'repetition,' functional excitation, tension, irritability, etc., either covertly imply sensitivity and orientation both of which are purely psychological or they patently controvert the facts.

A great many references to experimental results, some evidently original, ranging in complexity from Ray's culture of mushrooms in an agitated *milieu* to the effects of castration on horned animals, are cited to show that since the adaptations made by the organisms in such unusual conditions could not have been prepared for by natural selection they must be the result of volition.

The following typical quotations may serve to show the author's point of view: "The stalks, leaves, and grains of chlorophyll seek the light when it is useful, flee it when it is harmful. If organic psychology exists, the organs feel their needs and respond to them" (p. 54). "The chemotropisms and all the tropisms are instincts" (p. 71). "In case of an unusual need, vegetables look ahead (prevoient) and calculate" (p. 220). The action of leucocytes, the coagulation of the blood, the development of instincts, the metamorphosis of plants and animals, the regeneration of lost parts, the action of hormones, are all cited as cases which are inexplicable on any hypothesis that denies choice and orientation of the organism according to its own needs. "The ensemble of cells which by their multiplication and differentiation concur in the formation of an organ, constitutes a colony of living beings obeying a common instinct, just as a colony of termites construct their nest" (288).

Louisiana State University PAUL CAMPBELL YOUNG

The Psychology of Religious Mysticism. By JAMES H. LEUBA. New York, Harcourt, Brace & Co., 1925, pp. xii, 336.

Professor Leuba applies the term mystical to "any experience taken by the experiencer to be a contact (not through the senses, but 'immediate, intuitive') or union of the self with a larger-than-self." In the three forms of religious mysticism studied—savage intoxication, the Yoga system of India, and Christian mysticism—he has found emphasis on knowledge as an instrument for the suppression or enlargement of the self.

The impression of free, perfected life produced by certain drugs is pronounced comparable to the buoyant experiences of exaltation described by the Christian mystics. Ecstatic states and periods of dryness are traced through the lives of Christian saints, but their alleged oscillation is affirmed to be determined by physiological and psychological factors. The experience of Mlle. Vé, a modern mystic who recognized the sexual aspect of her revelations, illustrates the connection of mystic states with sex activity, particularly in persons deprived of normal satisfaction. In a survey of the lives of St. Catherine, Mme. Guyon, Santa Theresa and St. Marguerite Marie, passivity and asceticism are discovered to be methods for the furtherance of ecstasy. These mystics suffered from hysteria, but "moderate mysticism . . . is entirely free from that disease." Many mystics claim an Ascent of the Soul toward the highest stage, that of the trance, during which supernatural understanding, distortion of time and space, the appearance of brilliant light, an illusion of floating, and certainty of belief are experienced. Religious mysticism, it is contended, is a revelation not of God, but of man.

A statistical study shows that belief in the God of the established Christian religion is still widely prevalent among intellectual leaders in this country, but that there is correlation between eminence and disbelief among these leaders. Historians and physical scientists profess greater belief, while biologists, sociologists and psychologists have less.

The book, following Professor Leuba's series of publications on the psychology of religion, is of especial significance. Its facts are drawn from reliable sources, including authoritative biographies of the saints. Quotations from religious and psychological documents support the statements made. Aside from its informative value, the volume merits commendation, for it brings mystical religion into the sphere of scientific research.

Cornell University FLORENCE B. WOOLSEY

L'Enfant Turbulent. By H. WALLON. Paris, Félix Alcan, 1925, pp. 649.

This book is based principally on two hundred or so observations taken by the author as the assistant to Dr. Nageotte, physician at Bicêtre, an asylum near Paris (boys), and at the Fondation Vallée (girls), from the records of children who consulted the author at the Dispensary for Nervous Diseases and at the Laboratory of Pedology, and from records obtained by Dr. Nageotte from his non-resident work at La Salpêtrière. The interpretation of the data reflects the author's experiences with cases of psychical and nervous troubles resulting from injuries to the skull and emotional upsets due to the World War.

The title of Dr. Wallon's book refers to a class of children displaying an excess of activity more or less pathological. Since the loss of a function by an adult does not reduce him to a condition equivalent to that in which he was previous to the acquisition of the function, Dr. Wallon considers it to be impossible for the most part to apply to the psychical troubles of children the psychopathic classifications used for adults. At most, in his opinion, the children would fall under the heading of psychopathic personality. Therefore he employs a psycho-motor classification for dealing with his child cases.

Dr. Wallon builds up composite pictures of the biological and psychical equipment of these children and treats their types of reaction as of primary importance on the basis that movement is indissolubly bound up with excitation in the elementary reactions and that the foundation of the higher processes of perception and ideation is the psycho-motor experience. He believes that associations, such as an idiot forms between his organic, affective, sensory impressions and his modes of reactions, are those retained incidentally by the normal child, but that in the case of the latter they do not constitute his total equipment nor do they attain the equivalent degree of refinement.

The book is divided into three parts: (1) The Periods of Psycho-motor Development; (2) Psycho-motor Symptoms; and (3) Observations. It also includes discussions of various investigations made by others and the application of their theories to the data of the author.

Harvard University ROXANNA BEEBE-CENTER

L'Activité psychique. By EDME TASSY. Paris, F. Alcan, 1925, pp. xvi, 126.

The book treats of a function which is the third of the activities designated by the author as constituting the whole behavior of the human organism. There is first organic activity, then mental activity, and lastly psychical activity. Psychical activity, as contrasted with the purely organic or with the sensory (mental), has to do with the reactions which the individual as a conscious personality makes to those excitations which he receives. The three chapters deal respectively with psychical sensibility, action of the self, and action among selves.

Harvard University M. B. PRATT

Clinical Psychology. By Louis E. Bisch. Baltimore, Williams and Wilkins, 1925, pp. xiv, 346, 17 pl.

The primary object of this book is, as the author states in the preface, "to give the teacher a working basis by means of which he or she may be able to recognize an atypical child in the classroom and to know how best to handle the situation." The book should be most valuable to all students of the subject.

The clinical field is comprehensively dealt with from this standpoint, and although the author puts forward no claims for originality, yet the background of his own experience is evident throughout. In the appendix (pp. 219-396) are 58 highly instructive case records selected as illustrative of the various deviations that may be encountered. The records cover the usual fields of examination and are well assorted, although it is to be regretted that the old Binet-Simon scale was used instead of the Stanford revision.

University of Arkansas A. Rendle Stone

Fechner und Lotze (Geschichte der Philosophie in Einzeldarstellungen). By Max Wentscher. Munich, Ernst Reinhardt, 1925, pp. 207, 2 pl.

The author confines himself rather strictly to summaries in chronological order of the theoretical content of all the principal works of Fechner and Lotze and to a short account of their lives; there is little effort shown to give more than just this. The book will be found quite useful, especially by those who, intending to study one or all of the works of Fechner or Lotze, wish a preliminary orientation in the philosophical ideas of these men. The psychologist will be disappointed, however, by the lack of attention given to the empirical and scientific rather than the speculative aspects of their works; the *Psychophysik* is treated in elementary fashion in five pages, and the *Medizinische Psychologie* has only six, which are devoted to metaphysical questions. Of the book 55 pages are given to Fechner and 129 to Lotze; the two articles are independent. There are two portraits and a bibliographical guide.

Harvard University Frank A. Pattie, Jr.

Business Organization and Management. By Henry P. Dutton. Chicago and New York, A. W. Shaw Co., 1925, pp. xi, 545.

This is primarily a textbook for business managers. The value of psychology in training for business leadership is emphasized. McDougall's classification of instincts and modern psychopathology receive special and favorable attention.

Harvard University J. C. Kelson

Southern Pioneers. By Howard W. Odum. Chapel Hill, N. C., The University of North Carolina Press, 1925, pp. iii, 202.

The content of this book, comprising the biographies of Woodrow Wilson, Walter Hines Page, Joel Chandler Harris, Booker T. Washington, and other southern notables of international repute, is primarily literary. Its incidental sociological and psychological import is, however, already being utilized by statistics-seekers in the field of leadership and genius. This incidental function affords its only scientific aspect, as there is no pretense at correlation with such anthropological, biological, eugenical, or even historical information such as the tilte might imply.

University of Michigan. Martha Guernsey

NOTES

Systematic and Scientific Study of Death in Man

It is a curious fact that as yet there seems to have been no systematic and scientific study of human death. There have been more or less sporadic efforts to make such investigations in exceptional and interesting pathological cases, but the regular order or the average manner of death is not known. Some of the main reasons for such study are:

(1) To gain trustworthy and general knowledge of what most often takes place in the dying hour.

(2) To learn the manner of death for different diseases, and the percent of those conscious and unconscious.

(3) To find in what percent of cases there is severe pain, slight pain, and no pain, and the nature of the pain if any.

(4) To note details in exceptional and special cases, the causes of these phenomena, and the length of dying time.

(5) To determine the characteristics and differences in dying of infants, children, adolescents, adults before middle life, during middle life, and in old age.

(6) To present eventually a general picture of the dying time, based upon a sufficient number of observations, and with instruments of precision when possible.

(7) That with such knowledge, the treatment of patients during the dying time may be more humane and delicately done.

(8) That in cases liable to be painful, proper precautions may be taken in advance, so that pain may be avoided or greatly lessened.

(9) That finally the knowledge of the dying time may be so increased that physical death will cease to be feared or dreaded, since through such knowledge pain may be practically eliminated and death may be made easy, gentle, and placid.

I am seeking knowledge upon death and shall consider it a great favor if any of the readers of the Journal, who makes observations and studies of cases during the dying time, will send me a copy of his notes and results. Due credit will, of course, be given when sufficient data are received to justify publication.

100 E. Capitol St. — Arthur MacDonald
Washington, D. C.

Dr. Schoen on "The Elementary Courses in Psychology"

Having read with interest and in general approval, Dr. Schoen's article on the content and method of the elementary course in psychology,[1] I wish to offer a few comments upon what he said, and to add a criticism to those that he mentioned.

I am not so optimistic as Dr. Schoen about making psychology 'function' in the lives of the elementary students—assuming that the future continues to give us in our classes the same cross-section of humanity that we have been receiving. Any textbook which presents psychology scientifically must be difficult. It must demand thought and persistent intelligent study. Now, I feel, and I think many will agree with me, that at least half our students are incapable of perceiving the subtle relationships or formulations which any closely knit scientific work emphasizes. Indeed, if twenty-five percent of the beginning students obtain a real grasp of the subject and make it a part of their thinking-action habits, I feel that the instructor has made a marked success. The sooner we cease fretting about the lower half the better will be the scientific content of our courses. If a book were proclaimed "as a book which easily makes all things in psychology clear to the average student" I should have the suspicion that the book was superficial. As for the bright students—well, they will get a good deal out of a course in spite of the teacher or the text.

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[1] Max Schoen, The elementary courses in psychology, this Journal, 37, 1926, 593-600.

I advocate the giving of a brief and 'surface-study' course in psychology in the high school which may serve as an introduction to the college work. If this were done we could then, as in physics and chemistry, turn in college to be serious and laborious business of presenting technical and scientific courses. But even if this were done, I should not feel optimistic about making psychology 'function' in the lives of our students. In order that this ideal may be accomplished, two conditions are necessary: first, the student must have a good mind; and secondly, he must be willing to pay the price by hard persistent study. Unfortunately, both of these conditions are fairly rare.

A criticism which Dr. Schoen neglected, and which is the chief point of this note, deals with the general practice in our colleges and universities of making the laboratory work in psychology optional. In chemistry, physics, zoöloyy there is no choice; the student must take the laboratory in conjunction with the lecture course. Even the poorest colleges uphold this scientific ideal in respect to the teaching of these courses. But when we come to psychology how different is the case! Few departments of psychology require the laboratory work. I for one am firmly convinced that it is a mistake to grant the students this option; and in the institutions where psychylogy is counted as science credit the omission of the laboratory requirement is more than a mistake, it is a breach of faith, it is a violation of the well established scientific ideal.

Unfortunately there are many institutions of higher learning—not limited to the poor benighted colleges—where the student secures a 'soft science credit' by attending one lecture and two 'quiz' sections a week in psychology. I should like to see the day come when the American Psychological Association will go on record as recommending that no science credit be given in psychology unless the laboratory work accompanies the text-book course.

College of the City of Detroit E. B. Skaggs

The Eighth International Congress of Psychology

The eighth International Congress of Psychology, under the presidency of Professor G. Heymans, was held at the University of Groningen, Groningen, Holland, from Sept. 6-11. There were 242 names listed as members with about three-quarters of this number present. The meetings were held in the University buildings which were most admirably adapted to the needs of the Congress.

The Congress was in every sense an international one. The following is a listof the more prominent psychologist who were present: N. Ach, F. C. Bartlett, L. Binswanger, Ch. and K. Bühler, E. Claparède, O. Decroly, G. Ewald, A. Fauville, M. V. Gopalaswami, G. Heymans, E. Jaensch, P. Janet, D. Katz, F. Kiesow, O. Klemm, K. Koffka, W. Köhler, Mrs. C. Ladd-Franklin, G. R. Lafora, H. S. Langfeld, J. H. Leuba, A. Ley, M. Levy-Bruhl, J. Lindworsky, W. McDougall, A. Michotte, A. Meyer, E. Minkowski, C. S. Meyers, H. Piéron, W. B. Pillsbury, E. Rignano, F. Roels, E. Rubin, F. Sander, O. Selz, C. Spearman, E. Spranger, W. Stern, H. C. Warren, M. Wertheimer, H. Zwaardemaker.

The Congress opened on Monday, Sept. 6, with a reception in the University. The presidential address was delivered on Tuesday morning by the president, Professor G. Heymans. The program of the Congress was so arranged that every morning was given over to a symposium. In the afternoons, the Congress was divided into two or three sections for the presentation of special papers.

The first symposium was on Intensity Differences of Sensations with papers by C. S. Meyers and H. Werner. The Psychology of Religion was the second of the symposia participated in by Pierre Janet, Ernest Jones, J. H. Leuba, and R. H. Thouless. The subject under discussion in the third symposium was *Verstehen und Erklären* with papers by L. Binswanger, Th. Erismann, E. Ewald, and Eduard Spranger. Kurt Koffka, A. Michotte, E. Rubin, F. Sander and C. Spearman contributed papers to the symposium on *Gestaltwahrnehmung*. The last symposium was on the subject, The Psychology of the Lower Races with topics by F. C. Bartlett, M. Levy-Bruhl, W. Mayer-Gross, and A. Storch. Of the symposia, the one upon *Gestaltwahrnehmung* seemed, as at the seventh congress at Oxford, to arouse the greatest interest, and, at the same time, the greatest antagonism. This meeting had to adjourn without giving opportunity to all those who wished to speak upon the subject.

The papers in the afternoon sections were devoted largely to theoretical and experimental topics. There seemed to be an absence of papers upon subjects in applied and educational psychology. In general, it may be said that many of the papers were of such a general nature, dealing with particular theories or with particular controversies, that the discussion was of a very general sort and very limited in extent. The question might well be raised regarding the programs of the International Congress whether it would not be better to have a selection made of the papers offered for the Congress, and thus materially reduce the number of topics presented. At this Congress there was an average of about twenty papers each day. With a fewer number of papers, and more time for each, it might be possible to discuss psychological problems without indulging in vague generalizations and personal antagonisms.

The Congress was organized and conducted in a most excellent manner under the direction of the first secretary, Professor F. Roels. The government of Holland and the city of Groningen coöperated in the arrangements so that from the time the border was crossed, one was graciously received and assisted. It was a true hospitality on the part of the national government, the municipality and residents of Groningen, and the members of the University.

A number of very pleasing social affairs were arranged by the local committee. On Friday evening, the municipality of Groningen tendered a reception to the members of the Congress at the *Stadspark* which was most pleasant, and to the American visitors at least, most memorable. The dinner on Saturday evening, with speeches from several members, brought the Congress to a close.

The following were present from the United States: Miss Olga Bridgman, Miss Psyche Cattell, David Fryer, H. M. Kallen, Mrs. Christine Ladd-Franklin, H. S. Langfield, J. H. Leuba, William McDougall, Adolph Meyer, L. J. O'Rourke, W. B. Pillsbury, Rudolf Pintner, M. S. Viteless, Seth Wakeman, H. C. Warren, and K. Zener.

Smith College SETH WAKEMAN

ON BLUFFING IN EXAMINATIONS

In the Summer School of 1926, at the University of Pennsylvania, a class of 29 showed evidence of more than the usual bluffing in a quiz. A trap was set for them in the next examination in an attempt to determine just how much and how frequently the college student would bluff. To this end, the class was asked to discuss ten terms devoting not less than one page to each. The seventh term in the list was made up out of the whole cloth—*Psychoterminality*.

Of the group of 29 men and women, only 2 made the positive statement that they did not know what the term meant. Six others left the question blank without a definite negative statement. The other 21 students made some sort of an answer ranging from one-half to three pages in length. Many of these answers showed a good deal of originality and most of them were based on an attempt to get at the etymology of the word. A brief analysis of the types of answers may be of interest: automatism, 1; vitalism, 3; prediction (of end), 4; to what end does mind exist, 2; resumé of psychological terms, 3; hypnosis, 2; behavior of lower organisms (lower end of animal scale), 2; the limits of psychology, 2; reaction time, 1; and causes, 1.

Do these results indicate that bluffing is as prevalent as this in all college groups or is it a beautiful example of what W. H. Burnham, of Clark University, would call a lack of 'intelligent ignorance.'

University of Pennsylvania S. W. FERNBERGER

INDEX OF THE FIRST THIRTY VOLUMES OF THE AMERICAN JOURNAL OF PSYCHOLOGY

An Index of the first thirty volumes of THE AMERICAN JOURNAL OF PSYCHOLOGY has just been issued (Dec., 1926). The Index is divided into four parts: I. List of volumes with their publication dates and editors; II. Original articles; III. Books reviewed; and IV. Subject matter. The contents of these volumes are thus made readily available.

Now that this beginning has been made the editors of the JOURNAL hope to issue a similar comprehensive index after the completion of every succeeding ten volumes. The burden of the present undertaking has been borne by many psychologists thereby making possible the preparation and publication of the Index at a moderate cost.

The price of the Index is low. Orders should be sent to D. R. Knight, Morrill Hall, Cornell University, Ithaca, N. Y.

K. M. D.

THE INDIAN JOURNAL OF PSYCHOLOGY

Since the publication of the October issue of the JOURNAL we have received the first and second numbers (dated January and April, 1926) of the *Indian Journal of Psychology*, which is published quarterly by the Indian Psychological Association.

The *Journal* is edited by a board of editors consisting of Owen Berkeley-Hill, European Mental Hospital, Ranchi; Haridas Bhattacharya, Dacca University; G. C. Chatterjee, Government College, Lahore; J. M. Sen, D. H. Training College, Calcutta; and N. N. Sen-Gupta, University of Calcutta. It is published in English so the work of our Indian confreres will be readily accessible. The articles in the first two numbers—which will be reviewed elsewhere in the JOURNAL—are of a quality that deserves attention.

K. M. D.

ERRATUM

Through the misreading of a sentence in some correspondence on the subject, I unwittingly misrepresented the amount of psychological apparatus manufactured annually by the C. H. Stoelting Company as stated on p. 583 of my recent article on the "Development of Laboratory Equipment in Psychology in the United States" (this JOURNAL, 37, 1926, 582-592). It turns out that the figure $500,000 covers apparatus from all sources, including foreign markets, and also test-material published by book concerns in this country. Of this business the Stoelting Company receives, of course, its share.

University of Iowa CHRISTIAN A. RUCKMICK

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THE AMERICAN
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STUDIES IN AFFECTIVE PSYCHOLOGY

By PAUL THOMAS YOUNG, University of Illinois

I. THE LOCALIZATION AND SPATIAL CHARACTER OF PLEASANTNESS AND UNPLEASANTNESS

The aim of the present study is to throw light upon the following questions: (1) Are pleasantness (*P*) and unpleasantness (*U*) capable of localization in space? (2) Do pleasantness and unpleasantness possess a spatial character of volume, similar, possibly, to the volumic attribute of tones?

Method and Procedure. The experimenter sought to arouse a moderately intensive *P* or *U* in a direct but non-mechanical manner through the presentation of olfactory, gustatory, auditory, or tactual objects. When he judged that *P* or *U* had been aroused in *O*, he pressed a button which gave a rap upon a sounder—the prearranged signal for *O*'s report.[1] Sometimes there were bodily indications that an affective process had been evoked, as an incipient smile, a movement of avoidance or the involuntary twitching of facial muscles. When there was no hint of bodily movement the experimenter waited until he concluded that a feeling had been aroused.

O was seated in a Morris chair, his eyes closed. The chair was in an enclosure, 7×9 ft., curtained from the rest of the room. The stimuli were arranged on tables outside the enclosure. In an actual experiment E took a stimulus-object into the enclosure and stimulated *O*. When he thought that a feeling had been aroused he pressed the button and retired to copy down the report.

Stimulus-objects. The following objects and forms of stimulation were used in the main experiment:

Olfactory: white rose perfume, anise, stale cheese, sweet pea perfume, oil of cloves, heliotrope, lemon, oil of bergamot, H_2S, white locust perfume, camphor, oil of lavender, putrid fish, honey-suckle perfume, oil of juniper, nutmeg.

Gustatory: candy, citric acid, sarsaparilla syrup, quinine.

Auditory: crumpling paper near ear, friction of sandpaper held near ear, clangs from tuning forks—e g c′ and e g c′ a, noise from clapping pieces of wood together, from filing glass near *O*'s ear, and from falling metal.

Tactual: sandpapering of chin, cotton on cheek, removal of foot rest, tickling nose with camel's hair brush, rapping on knee or forehead with small hammer, pressing needle through stocking, snapping rubber band on face, dashing water on face, pinching nose with fingers, slapping face warming with blackened carbon light, holding lighted match near face.

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[1] The present method should be compared with that of Nafe who instructed his *O*s to judge when the feeling was at its maximum and then to break off the observation. In a later experiment, repeating Nafe, one of my *O*s reported difficulty in determining when the feeling was at its maximum while at the same time he observed the feeling. J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 1924, 35, 510.

Instructions. The following instructions was used: "Put yourself so far as possible in an attitude of indifference. Odors, sounds, tactual impressions, and tastes will be presented to you from time to time. When you hear the tap of the sound-hammer please make a report. You are instructed to describe the quality and the spatial characteristics of the feeling, noting especially the place where it is located."

At the close of the experiment the following questions were asked: (1) Judging from your observation in this experiment, are *P* and *U* experienced at some place? (2) How do you understand the instruction to describe "the spatial characteristics of the feeling, noting especially the place where it is located?"

The answers which the *O*s gave to the questions are presented below with the data.

Observers. The experiment was conducted during the fall of the year 1922 in the Psychological Laboratory of the University of Illinois. Five *O*s took part in the experiment, all of them connected with the Department; C. R. Griffith (G), G. D. Higginson (H), H. L. Kingsley (K), Alice H. Sullivan (S), Dorothea E. Wickham (W).

No fixed number of observations were taken on any day; the number varied with the length of report. Every *O* made a total of 65 reports during the experiment, except W who made 55. The total number of observations in the experiment is 315.

SUMMARY OF REPORTS

Observer H: There is no case of localized *P* and *U* in the reports of H. Furthermore, there are positive statements to the effect that affective processes are not localized, for example:

"I seem to localize the odor but I don't seem to localize the *P* of it."

"There was no (affective) localization so far as I could see. The sound, of course, was localized."

"Definite localization of cutaneous and kinesthetic qualities, but I could observe no localization of *U* or *P*."

"I did not localize the *P* in any sense, but the relaxation was quite clear."

Sometimes H reports that feelings are general to the entire body, as:

"A *P* feeling seemed to be general to my entire body."

"The feeling seemed to be general through my body. It was not localized."

At the close of the experiment H answered the two questions about localization and the instructions as follows:

(1) "In the light of my observations during this series of experiments, I am quite sure that I can state with emphasis that pleasantness and unpleasantness, for me, are not experienced at any particular place with reference to my body." (2) "I understand the instruction 'note the place where the feeling is located' to mean any particular region to which the particular feeling under consideration is referred or found or sensed, as on any general area of the body. 'The spatial characteristics of the feeling'. I took to mean the relative spread-out-ness or diffuseness, or the general limits of a feeling as it is observed at or in any particular region or place."

Thus, so far as H is concerned, the evidence for the localization of *P* and *U* is completely negative, and the reports furnish no data on the spatial character of *P* and *U* except a few statements that these feelings may be general, throughout the body.

Observer S: S reports localized feelings, for example:

"That was very *U* and nauseating and that feeling was localized in my chest and around my stomach. The feeling seemed to be made up of sensations—little pressures and things like that in my stomach."

"The localization of the *P* feeling seemed to be very diffuse. It was all over me and enveloped me. The localization of the *U* feeling was right within me—chest and stomach—and it was small compared to the *P* feeling."

"*U* was located around my mouth somewhere; not in my mouth. It must be a sense-feeling because the *U* is right there with the sweetness."

"A feeling of *P*, widely diffused, which became more widespread. The feeling began somewhere in the region of the chest and spread over the body and outside of me."

"A feeling of *U* which increased in intensity and in extent. It was localized partly in the chest and partly outside of me, and as the stimulus continued the *U* shot back and forth in streaks. It was rough in texture and had no definite boundary line."

"Very intense *U*. It was spread all over the body as if I were immersed in it. The feeling was soft."

"*P* seemed to be localized around the stimulus out in space. It was soft and fluffy."

"Moderate *U* which was localized in the nose."

"Very intense *U* which seemed to be localized in my head."

The reader will have noticed in reading the above quotations that *P* and *U* are sometimes described as extended, surrounding the object, having a voluminous out-spread. The following excerpts from other of her reports illustrate further this spatial character of the affective processes:

"A great expansive feeling which surrounded the stimulus. The expansive feeling seemed to emanate from the stimulus outside of me. It seemed as though the most *U* part was near the stimulus and then it got less and less intensely *U*. I couldn't say where it ended."

"*P* was soft and filmy and much brighter than *U*. It had rays of *P*, more intense, scattered around. In between the rays there was weak *P* and the rays were stronger *P*. It was quite extended but it wasn't in me. It was outside of me surrounding the stimulus. It was as if the stimulus were in the center and then these little rays extended out from it."

"The *U* seems to begin in the mouth and to spread out around. It spreads out in rays. It goes out in one plane. It is a flat thing. Some of it goes outside of me and some of it is in me. These rays seem bits of more intense *U*."

"That was *P* and it was spread out all around the odor. It was grayish with just a little tinge of lavender in it (S has synaesthetic imagery). It seemed to be wrinkled. It didn't have rays. It spread out in a circle with wrinkles of more intense *P*."

If we take the above reports at face value, we may say in general that the affective processes have a spatial attribute. *P* and *U* are extended in a spatial pattern which may have gradations in intensity, the feelings being more intense in the center and less at the periphery. The spatial pattern may have streaks, rays or wrinkles, differences in texture. The affections are large or small in volume; boundaries are not clearly marked off.

Sometimes the feelings are very voluminous, diffuse, widespread, as in the following examples:

"The feeling seemed to spread out all over me. It was as if I were within the experience and it were all around me. The feeling was diffuse."

"The *P* was like a cloud which enveloped me. It wasn't localized within me; I seemed to be localized within it."

"That was *P*; it was diffuse, large. It seemed to envelop me and was not localized within me."

"*P* seemed to be a cloud-like thing that surrounded me and was outside of me. I couldn't tell where it ended or where it began."

S answered the two questions as follows:

(1) "Pleasantness was, for me, never experienced at any place within me. In most cases I think it is non-localizable; in some cases, however, it seems to be localized around the stimulus. But even in the latter case the localization is not clear, for I can not say where it begins or ends. Unpleasantness was quite often fused with organic sensation and was then localized where the sensation was. I think it is also non-localizable where experienced alone. By non-localizable I mean that I can not locate it in space any more than I can locate it within me." (2) 'I understand the instruction 'to describe the spatial characteristics of the feeling, etc.,' to mean that I am to tell how extended the feeling is and whether or not it has a boundary line, and its pattern. Sometimes I can describe the pattern of a feeling and yet I can not localize it. I understand the instruction about noting 'the place where the feeling is located' to mean that I am to note whether it is localized within the body as an organic or kinesthetic sensation is located or outside the body, around me, in front of me, etc."

If I understand correctly the answer to the first question, *P* and *U*, in themselves considered, are not localized, or possibly they are very vaguely localized around the stimulus-object. S's reports contain a good many statements to the effect that *P* and *U* are localized, but there is some doubt about the place and the fact of localization.[2]

S explains that the local character is due to the sensory process rather than to the affective process. In other words it is a sense-feeling, a fusion of sensory and affective processes, which is localized. The sensory rather than the affective component is local.

Observer G: G sometimes reports localized feelings, as for example:

"Decidedly *P*. The tickle itself was *P*, and the *P* quality seemed to be located in the nostrils and at least about the face."

"The auditory quality was *U*. It was localized there about my ear. It was just a *U* sound; it was that kind of a sound."

"The tactual quality itself was *U*. It was a *U* right there at my forehead."

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[2] There are at least a dozen statements in the 65 reports expressing doubt about the localization: "I don't know where the feeling is." "I cannot say where it is localized." "I am in doubt about the localization."

"Withdrawal and a facial configuration meaning disgust. That taken with organic qualities gave a nausea. The whole experience was *U* in general and in particular the odor was *U* and the organic qualities were *U*. In a certain sense, then, the *U* was localized at the nostrils and in the viscera."

"*U* located with taste qualities on the tongue."

"Auditory qualities increasingly *P*. The *P* feeling was an integral part of the tonal experience, located with the tone, a character which the tone possessed."

"The *U* is part of the odor and located with the odor in the nostrils."

"The *P* was localized along with the tactual qualities and a resident part of them. A more diffuse and obscure feeling of *P*, a feeling of comfort, carried in the total character of the organic and kinesthetic processes, an obscure background of *P* feeling."

P and *U* are sometimes described as diffuse, widespread, voluminous:

"There was a momentary *U*, diffuse and not localized anywhere; a general feeling of *U*."

"An organic and kinesthetic pattern, diffuse *U*."

"Slightly *U*, widespread over the body."

"An organic and kinesthetic pattern, widespread, unlocalized, *U*."

At the close of the experiment G answered the two questions as follows:

(1) "A considerable number of my affective experiences appeared to be very definitely localized. Frequently they were localized along with the perceptual experience itself and less often they appeared to be located along with organic processes. Occasionally the affective experience was unlocalizable but almost always (in this case) the related organic processes were diffuse and obscure." (2) "I understand the instruction to mean the localization of affective experiences in the same sense as other objects are located. The tones from the tuning fork out there are pleasant 'out there.' The organic sensations aroused by a nauseating odor are near or in the diaphragm or stomach or viscera and the affective quality is similarly placed."

At the start of the experiment G did not localize *P* and *U* and there are a number of definite statements to the effect that *P* and *U* are not localized. As the experiment progressed *P* and *U* feelings were localized along with the sensory processes and at the place of the sensory processes. G's answers to the questions confirm the view that *P* and *U* feelings are definitely localized.

Sometimes the localization is not exact; *P* and *U* feelings are diffused over the whole body or in some general region of the body.

The reports show unmistakably that G regards *P* and *U* as characteristics of the sensory processes or of the total experience. *P* and *U* go along with, depend upon, attach to, belong to, are tied up with, are referred to, are characteristics of, some sensory pattern. The sensation *is* *P* or *U*; it is 'that kind of a thing.'

G is skeptical about the existence of affective processes and speaks of *P* and *U* as meanings carried by sensory processes.

Altogether it appears from G's reports that sensory feeling are localized, and that so far as *P* and *U* are reported to b localized the local character is derived from localized sensor processes. The reports furnish no evidence that the affectiv processes, *P* and *U*, considered in their own right apart from sensory processes, are localized.

I hazard the guess that the spatial character of diffusenes or volume is also referable to sensory processes.

Observer K: Some examples of localized *P* and *U* by K are given below:

"As far as the *U* was localized it was all through the upper part of m body. Not localized at any point."

"That was a *P* sensation and it seemed to be localized entirely in th nose region."

"That was *P*. In a general way it seemed localized in the region of m head."

"At first the *U* was felt in the region of the sensation, as it seems Later it seemed to be diffuse and general."

P and *U* may be diffuse and extended, as for example:

"The effect was spread out, not localized. The sensation was sprea out and that gave me the impression of the *U* being rather diffuse."

"That was a *P* odor. It was rather spread out, diffuse, without limitations. It seems to me as if the feeling is spread out in the upper part o the nose, in my throat and back of my mouth."

"That was *P*. It was diffused all through the upper part of the bod and arms with a more intense form around the nostrils where I got the odor It was in the upper part of my body as far down as the stomach. It seeme to come to a head being more intense, thicker, in the region of the nostril and less intense as it spreads through my body. I almost visualize it i this way."

"*P* feeling. It seemed to be sort of a general muscular response, ver diffuse."

"I cannot localize the feeling. The experience in general was in th region of the nasal cavity and the upper throat. Attention was directe there and what I observed there seemed to be the agreeable, and pleasure giving thing."

Some of K's reports indicate an uncertainty about the distinction between sensory and affective processes and consequen difficulty in reporting upon localization and spatial characte of *P* and *U*:

"That was a *P* odor and I seemed to feel it part way through my throat I am confused about the localization; I don't know whether I am to distinguish between feeling and sensation. I feel the sensation; that's a naïv way of saying it. I felt that one as far down as the neck. The agreeabl sensation was in the throat. It seemed to be very diffuse and general.'

"The stimulus attracts my attention and it seems as though the feelin is concentrated there. With more careful observation I don't think could say the feeling is located there because of other experiences tha come with it."

"That was *U* and the experience was pretty well spread over my body The muscles of my nose tended to pucker. What I localized seems to b sensory experience."

"That was *P* and I seemed to feel it in the nasal cavity and down the throat and even in the lungs. At least I refer the feelings to these regions. It is very hard for me to say whether that is feeling or sensation. As nearly as I could tell the feeling was there and even outside of the face. I localized it there."

At the close of the experiment K answered the questions as follows:

(1) "Judging from my observations in this experiment, *P* and *U* are experienced at some place—if we are to understand 'place' as referring to rather extensive and indefinite regions of the body. They, *U* and *P*, were not localized as sensations of taste and touch are localized, at some definite point, but more as general bodily kinesthesis is localized, vaguely within some region of the body." (2) "I understand by the instruction 'to describe the spatial characteristics of the feeling, noting where it is located,' that I was to observe its size and shape and location, meaning by location the place within or without the body where it seemed to exist. I understand that I must try to observe whether I localize the feeling as I do sensations, as when I experience pain *in* the end of my finger."

I have quoted above examples of localized feelings. It would be equally possible to quote examples where localization was doubtful or impossible. For illustrative purposes a few examples are given below:

"I don't know whether I can speak of the localization of the *U*. Of course the sensation was clearly localized."

"Moderately *U*. I am unable to say anything about the localization of it." "The *P* which I felt couldn't be localized or described spatially at all."

"I can't say that *P* and *U* are localized, but attention is towards the stimulus and the experience seems to be there."

"*P*; it seemed to me that the stimulus took my attention so that I couldn't make any localization."

If we take K's reports at face value, we may conclude that *P* and *U* are sometimes localized in space and sometimes diffused in a general region of the body, over the entire body, or even outside, enveloping the body.

The main trouble with this conclusion is that it is ambiguous. K is not clear about the distinction between sensory and affective processes. The localized feelings are observed as sensory processes, or as sense-feelings, and it is probable, judging from the reports, that the local character rests directly upon sensory experience. K attends to sensory processes which *are P* (or *U*) and it seems as if the *P* (or *U*) feeling is there.

Observer W: A few quotations from W's reports bearing upon the problems of affective localization and volume are given below:

"A strong feeling of *U*; a desire to draw away from it. The feeling seemed to be largely in the region of the head and shoulders and arms. It was not diffused throughout the body."

"I enjoyed the candy very much. I had the quality of *P* so far as the sensation of taste was concerned. It seemed to be localized in the region of the mouth and throat. I didn't seem to be able to locate the *P* any place other than in my mouth or in my throat."

"I seemed to feel a certain feeling of *P* spreading through me. A general bodily contentment. I should like to have that sensation for some time. The localization probably took a region from the nostrils to my ears."

"A vague *U* which seemed centered around my ear drum."

W's type of report is quite naïve and, as it happens, her data do not throw much light on the experimental problems. Sense-feelings are localized in the region of the sensory processes. The distinction between sensation and affection is not clearly drawn, so nothing can be said definitely about the localization of *P* and *U* as affective processes.

General Summary

If we depend upon statements made by the *O*s at the close of the experiment, we find lack of agreement upon the matter of affective localization. Two *O*s (G and K) state that *P* and *U* were localized; two (H and S) state that *P* and *U* were not localized. One (W) was not given the questions, but nevertheless localized feelings occur in her reports.

I am not willing to infer that this disagreement reflects any genuine difference in the experience of the *O*s—although this is a possibility. It seems rather that the disagreement rests upon different understanding of certain critical words, specifically:

(1) The term 'feeling' is used by the *O*s to indicate (a) clear sensory processes, especially organic and kinesthetic processes, (b) sense-feelings as, for example, an odor which has the character of *P* or which means *U*, or which possibly is a fusion of sensory process, and (c) non-sensory, non-clear affective processes, *P* and *U*, differing in quality, intensity and in temporal course.

When the observers used the term 'feeling' in the sense of (a) or (b), localization in perceptual space was sometimes reported.[3] In the sense of (c), localization was not reported.

(2) The term 'localize' is also ambiguous. This word may signify: (a) to *refer* *P*, *U*, or other 'feelings' to some place in perceptual space. K, for example, used the term in this sense. Localization meant reference to the sensory object—odor, taste, sound, etc.; or to the bodily processes secondarily evoked by the sensory object—nausea, relaxation, etc. From this point of view all of the *O*s 'localize' feeling. (b) 'Feelings' are said to run their course in a region of the body without very precise spatial localization, or they may be general to perceptual space, diffuse, voluminous. The term 'localize' sometimes means merely the noting of the space where the 'feelings' are given and the description of the spatial characteristics of the 'feeling' ex-

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[3] The instruction used the term 'feeling' without defining it, whereas question (1) asked specifically about the localization of *P* and *U*. The *O*s usually took the term 'feeling' to refer to *P* and *U* feelings, but if the terms of the instruction had been precisely defined, possibly the ambiguity would not have occurred.

perience. (c) The term 'localize' may mean to place affective processes within the phenomenal field as components of it in a non-spatial, non-perceptual sense. I have no evidence that this third possibility occurred to any *O*.[4]

In the sense of (b) there is a good deal of evidence that *P* and *U* 'feelings' have the spatial attribute of volume—bigness, diffuseness, outspread. *P* and *U* may be restricted to some gross anatomical region or they may be general, unlimited, everywhere. *P* and *U* may spread out around the sensory object. *P*, as Nafe has noted,[5] is generally more voluminous than *U*. The evidence from the present observations seems to indicate that this voluminous outspread is sensory in nature.

CONCLUSION

The *O*s disagree upon the matter of localization of *P* and *U*. It is not likely that the differences in the reports reflect genuine differences in affective experience. It is more probable that the disagreement depends upon the ambiguity of certain words, specifically the terms 'feeling' and 'localize.' These terms may be understood in a manner justifying the conclusion that '*P* and *U* are not localizable;' in another manner the conclusion follows that '*P* and *U* are localizable.'

The experiment brings to clear light the question of the existence and nature of the traditional affective processes, and a serious difficulty with the introspective technique.

RELATION TO OTHER EXPERIMENTS

In a previous brief paper upon localization I concluded that "(1) *P* and *U* are not localizable. Localization and extent are characteristics of the unanalysed object-feeling of common-sense. (2) Localization and also qualitative differences of feeling depend upon sensory components in the total feeling experience. The popular term 'feeling' includes localized sensation as well as meaning."[6] Also in criticising Wohlgemuth I have defended this position.[7]

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[4] Cf. E. B. Titchener, *Lectures on the elementary psychology of feeling and attention*, 1908, 43 f.

[5] *Op. cit.*, 523.

[6] P. T. Young, The localization of feeling, this JOURNAL, 29, 1918, 430.

[7] P. T. Young, The coexistence and localization of feelings, *Brit. J. Psychol.*, 15, 1925, 356-362.

Wohlgemuth has published a rejoinder which does not seem to advance the problems in question. The interested reader should refer to the original papers in this controversy and then form his own conclusions.

The net outcome of the discussion seems to be the drawing of a distinction between (1) the cognitive awareness of a pleasant or unpleasant object and (2) an immediately felt pleasantness or unpleasantness. This distinction can be drawn on the basis of Wohlgemuth's published introspections, for example: "I seemed at the same time to know that there was a certain quality, a sort of whistling quality, connected with the tone that was disagreeable without feeling it to be so." (A. Wohlgemuth, Pleasure-Unpleasure: An experimental investigation on the feeling-elements. *Brit. J. Psychol., Monog. Suppl.*, 6, 1919, 95.) In the sense of (1) there is ample evidence that two or more pleasant and unpleasant objects may be cognitively apprehended at the same time. If by 'feeling' one mean the awareness of a pleasant or unpleasant object, then 'feelings' are localized and they may coexist. If by 'feeling' one mean pleasantness and unpleasantness reported as felt and as having a temporal course in experience during which qualitative shifts and intensive changes may occur, then the possibility of coexistence and localization of the affective processes has yet to be demonstrated for the evidence is mainly negative.

At the close of his rejoinder Wohlgemuth refers in a foot-note to Phelan's recent monograph upon feeling. I fail to see how this study advances the problems of coexistence and

I believe that the positions I have taken in these studies of localization presuppose a certain systematic position regarding the nature of affective processes—the position which was in the air at Cornell University in 1917-18. Starting from the view that there exist non-clear, non-sensory affective processes which vary in quality (*P* and *U*), in intensity, and in temporal course, the papers I wrote have a certain validity. The validity is relative to the postulates which underlie these studies. Given another set of definitions and psychological postulates the results might be reversed. The answer to the question of localization may be 'yes' or 'no' according to what one understands by 'feeling' and by 'localization.'[8]

The Effects of Practice in Observing. Nafe has written: "Many of the *O*s, in their earlier reports, attempt to localize the affection, and often mention a more or less definite region where it is felt. Later, however, they speak of a 'reference' to a part of the body and deny the localization. All the *O*s finally report affection to be unlocalizable, but they may still refer it to their whole body, and the affection may include the body or be projected out from it."[9] This statement implies that as the experiment progressed and as the *O*s became more practiced in observing, the tendency to report localized feelings disappeared. In other words, practice made them 'better.'

With Wohlgemuth the 'improvement' was in the reverse direction. Wohlgemuth writes: "Subjects P and J are unable to localize the feeling-tone at the beginning of the investigation, but as the training proceeds statements to that effect became more frequent."[10] Also in another study Wohlgemuth concludes that "The ability to localize the feeling-elements improves greatly with practice."[11]

In my present experiment repeated observations under outwardly controlled conditions (up to 65 reports) did not bring the observers into agreement and there is little indication that further practice would bring any unity. Had I educated my *O*s in the precise use of certain critical terms,

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localization of *P* and *U*; but the following quotation is suggestive: "There is no *a priori* reason why we should conclude that an individual *feels* a pleasant or an unpleasant response when he simply states that the stimulus-object is pleasant or unpleasant. Futhermore, there is ample *a posteriori* evidence to show that judgments of value, whether of hedonic or aesthetic value, are clearly differentiated, by the subjects themselves, from the actual experience of affective phenomena. As a matter of fact, we have reports of experiments in which it is explicitly asserted that no feeling was experienced, yet the subjects formulate judgments of value in terms of pleasantness and unpleasantness. On the other hand, there are numerous cases in which the affective character of the experience is actually *felt*, and, in addition, a judgment of value is made, which is, on the subjects' own statements, quite distinct from the feeling itself, and, sometimes, quite contrary to it." G. B. Phelan, *Feeling experience and its modalities*, 1925, 64.

It is essential, therefore, to distinguish between the affective judgment which characterizes an experience as *P* or *U*, and the actual *felt* *P* or *U* upon which the judgment may or may not depend. Wohlgemuth has not eliminated the affective judgment of objects in reporting his own 'mixed feeling' in which "pleasant daffodils" and "unpleasant foot-sensations" were present simultaneously in consciousness. A. Wohlgemuth, The coexistence and localization of feeling, *Brit. J. Psychol.*, 16, 1925, 116-122.

[8] Titchener has stated his belief that "affections are always coextensive with consciousness, diffused over all the sensory contents present at the time; and that, if the pleasantness of a taste is localized in the mouth, that is simply because consciousness itself, under the experimental conditions, has been narrowed down to a taste-consciousness." E. B. Titchener, *A text-book of psychology*, 1915, 234-235. Cf. also *Lectures on the elementary psychology of feeling and attention*, 1908, 43 f.

[9] *Op. cit.*, 535.

[10] A. Wohlgemuth, On the feelings and their neural correlate, with an examination of the nature of pain, *Brit. J. Psychol.*, 8, 1917, 450.

[11] A. Wohlgemuth, Pleasure-unpleasure, etc., *Brit. J. Psychol.*, *Monog. Suppl.*, 6, 1919, 242.

had I laid down the definition of these terms and the specific task of observation, I might then have found uniformity. It would, however, be a *predetermined* uniformity more than a *discovered* uniformity.

II. The Case for the Affective Processes

The present experiment, dealing with the nature of the affective processes, was conducted immediately after the preceding experiment upon localization.[12] The experimental setting, the stimuli and other details were the same as in the earlier experiment. Three of the *O*s were retained.

Instructions. Throughout the experiment a general instruction was used as follows:

"You will be presented with simple odors, tastes, sounds and tactual impressions. Report when you hear the rap of the sound hammer."

Along with the general instructions were used in turn two specific instructions which, it was hoped, would throw some light upon the nature of the affective processes. They are:

(1) "Describe your feeling experience, mentioning temporal relations precisely. Put direct description of the observable content-processes of experience outside of parentheses.[13] Put all else inside."

(2) "The aim of this experiment is to determine the fundamental nature of feeling. Whenever you use terms which refer to feeling state exactly what these terms mean. Analyse the parts or phases of experience included under the term 'feeling.' "

Summary of reports. The present experiment comprises 153 reports distributed as follows:

Os	Instruction 1	Instruction 2	Total
S	30	32	62
G	30	24	54
K	30	7	37

Results. The experiment was in the nature of an exploration; the results could not be predicted. I have taken the data as a whole and asked this question: What do the data show about the nature of *P* and *U*? I take up in turn the different attempts made by the *O*s to describe them.

(1) *Bright and dull pressure.* Some of the reports of S appear to confirm a view recently expressed by Nafe that "the affective qualities, pleasantness and unpleasantness, turn out, under direct observation, to be modes of pressure: *pleasantness is a bright pressure, and unpleasantness is a dull pressure.*"[14]

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[12] In the Psychological Laboratory, University of Illinois, from January to May, 1923.

[13] The use of the parentheses was suggested by Jacobson's work (E. Jacobson, On meaning and understanding, this JOURNAL, 22, 1911, 553-578). This technique proved futile in the present case, so the parentheses have been disregarded in treating the data.

[14] Nafe writes: "The *O*s agree that the affective experience comes as a unit, and that only when the pressure described as bright, etc., is present can such an experience be described as *P*, and only when the pressure described as dull etc., is present can an experience be characterized as *U*" (*op. cit.*, 508, 511). This is equivalent to saying that bright pressure is a *sine qua non* of *P* and dull pressure a *sine qua non* of *U*. If Nafe were correct, an analytical attitude should reveal bright pressure every time an *O* feels *P* and dull pressure

"The *P* was spread out from the ticklish sensation. It was bright and thin."

"Later a more intense *P* which was localized around the odor. It spread out in rays and was fluffy and bright."

"*P*; the quality was like weak, light pressure and perhaps such a pressure was at the core of the experience."

"*P* was localized all over the body and was made up mostly of a very weak pressure as I relaxed. There was something besides the relaxation there—a certain smooth, soft texture which seemed to belong to the *P*."

"Moderately *P*. The *P* was soft, fluffy, cloud-like and bright. It was not localized; seemed to be spread out in rays. It was a little like weak pressure or contact."

"The *P* was entirely separate from the cutaneous sensation. It was not localized, but it was very large in extent, very soft and light and thin. It seemed to be in two dimensions only and was bright, almost white in color. It was similar to weak pressure or contact, but it was a disembodied contact."

"*U* which was mixed with actual sensation—dull pressure in the region of the chest and stomach."

"The *U* seemed to be located in the chest and was fused with dull pressure in the chest."

"Very intense *U*, there were meanings carried by dull pressures in the throat which meant that I would hold my breath."

"*U*; then the feeling changed to a different kind of *U* and I thought I should like to cry. This was localized in the chest, and was a dull, heavy pressure and the meaning."

"Moderately *U*; the feeling itself seemed to be made up of a diffuse, unclear pressure or tension all over the body."

P is described as bright, light, fluffy, cloud-like, soft, thin, active, arousing. *U* is described as rough in texture, soft, fused with dull pressure, hard and dark.

It should be noted also that *P* is sometimes reported to be associated with dull pressure, and that there is one 'lively' *U*.

"In the *U* and dull pressure there is something hard and dark; in the *P* and dull pressure there is something moderately soft and bright."

"Feeling of *P* which had its core in a mixture of *P* and dull pressure in the chest."

"The nervousness was made up of a readiness to jump at the least sound or feeling and was muscular tension and a certain kind of *U*—a lively sort."

My reports do not justify the assertion that *P is* a bright pressure and that *U is* a dull pressure. I might conclude that *P* and *U resemble* bright and dull pressures respectively; or else that *P* is *associated with* bright pressure and *U* with dull pressure more frequently than the reverse.

(2) *U and P described as bodily attitudes, especially attitudes of avoiding and accepting.*

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every time an *O* feels *U*; but this is not the case. Further, the associations '*P*-bright pressure' and '*U*-dull pressure' are sometimes reversed. Nafe's failure to consider negative cases may be regarded as a serious methodological defect.

It is interesting to note that Boring, observing in Yokoyama's experiments, reports: "I can't describe *U*. I think it is some sort of organic pattern in my body. I actually believe that it is as much like the dull pressure-aches of nausea as *P* is like the bright contacts of tickle; but it is a far cry from *P* and *U* to these simple sensory things" (M. Yokoyama, The nature of the affective judgment in the method of paired comparisons, this JOURNAL, 32, 1921, 362).

Observer K: "Very *U*. Immediately there arose kinesthetic processes from the face, neck, meaning turning head and screwing up the face to get away from this tickling *U* thing." "The odor was *P*; I liked it. I tried to get more of it and had an attitude which meant 'I wish it were not so weak.'" "*U*; I felt a drawing of the muscles in the face and a sort of jerk of the head to one side away from the stimulus."

Observer G: "A new pattern of organic qualities. A change in the kinesthetic qualities in the chest meaning a *P* odor to be inhaled as much as one can." "Faint kinesthetic or organic processes which meant turning away from the odor." "Perhaps the organic and kinesthetic processes were not *U* but they meant withdrawal, avoidance, expectoration, etc." "A diffuse organic and kinesthetic accompaniment which meant withdrawal, holding of the breath, etc."

(3) *P and U as biological meaning.* The attitudes of avoiding and accepting are sometimes described as kinesthetic-organic patterns (see above), and sometimes the significance of these attitudes is merely stated without analytical description. The two types of report are intertwined; the following excerpts are intended to illustrate chiefly the *meaning* side of the reports.

Observer K: "The *U* meant a desire to have it stopped. There were muscular reactions of avoidance and escape." "When I say this stimulus gave me a *U* feeling I mean that I was affected in such a way that I wanted it to stop. I either wanted to get away from it or have it get away or stop."

"The experience was that of nausea; the feeling was one of repulsion. I wanted to get the thing away, or to get away from it."

Observer G: "Very diffuse and obscure organic and kinesthetic processes in the viscera and chest meaning comfort. An equally obscure and diffuse, very faint, feeling of bodily *P*." "The general bodily set of aversion and withdrawal carried the meaning of *U* more clearly, more intensely." "*P* might be the meaning given by the kinesthetic and organic set which meant 'I'd just as soon continue smelling it.'" "Along with the olfactory quality there was plenty of kinesthesis about the face and chest which meant withdrawal, or a dislike, or dissatisfaction, or discomfort. Whether all those things carried the meaning of *U*, or whether there was a *U* to the odor itself I cannot say."

Observer S: "This *U* may be nothing but the sensations resulting from puckering up the mouth and frowning, plus the meaning that accompanied these." "All the experience this time was organic sensation plus meaning as far as I could observe."

(4) *P and U as mere verbal statement of meaning.*

Observer S: "Part of what I call *U* seemed to be meaning, as if I said to myself 'I don't like that' although I didn't say it or think it, but there was a kind of 'short circuited' meaning having the same reference as if I had said it." "This was tantalizing and interesting and nice, so it must have been *P*."

Observer K: "I had what was probably a conscious attitude meaning perhaps 'Oh, I like this.' There were some fleeting, scrappy images of spicy foods." "This experience might be called *U*. I can't see that there is any special feeling to it." "All that I mean when I say that this is *U* is that I do not especially enjoy bitter tastes. I should rather taste sweet."

(5) *P and U as characteristics of the object.* In the foregoing reports the observer, in a sense, has looked inward for a description of the affective processes. *P* and *U* are regarded as components of individual experience which resemble pressure,

or as the kinesthetic-organic equivalents of the attitudes of accepting and avoiding, or as the meaning of these attitudes. It is a question whether the following reports really characterize *P* and *U* as such. They refer *P* and *U* to the object and regard *P* and *U* as though they were bound up with the object.[15]

Observer G: "The *P* seemed to be part of the total character of the cutaneous pressure and localized with it." "The *P* quality is a part of the perception of the odor." "The *P* feeling was an integral part of the tone experience and located with the tone." "I could not detect two things, the pain and the *U*. The experience was just that kind of an experience, *viz.* *U*." "The odor was distinctly *P* and the *P* seemed to belong to the odor as part of its total character." "The *U* was a distinct part of the total perception." "The *P* was a genuine part of the odor so far as I could tell." "The *U* characterized the odor itself." "The *P* seemed to be a part of the perception, a characteristic of the tone itself." "A *P* odor. The *P* was a part of the perception."

Observer K: "It seemed to me that the *U* quality was inherent in the sensation of bitterness. I just did not like the bitter taste and that is about all I could observe." "This olfactory sensation gave me a feeling of *P*. I seem to identify the feeling with the sensation. It just seems that the sensation *is* *P*. It is the kind of a sensation that I should like to have more of." "There was pain which was *U*, localized on the forehead. It seems in this case that the feeling of *U* was tied to the sensation as an attribute."

General Discussion

(*A*) *The sensorial attitude.* In the physical sciences observation involves the direct stimulation of a receptor organ. It involves a predisposition, a set of the *O* which is equivalent to the purpose of the observation.[16] It involves or implies the presence of a phenomenon in the *O*'s field. Frequently observation is followed by report, in words or in other symbols.

In such sciences as chemistry, geology, astronomy, etc., the essential conditions of a particular observation lie outside and beyond the organs of sense. Consequently the physical sciences have regarded the *O* as a troublesome source of error and the physical methods have heretofore aimed at his elimination. The analytical psychology of datum has regarded sensory processes—pressures, reds, tones, bitters, etc.—as directly dependent upon the stimulation of receptor organs and the bodily structures behind them. Observation in sensory psychology has been essentially the same as observation in chemistry. The chief difference between a psychological observation and a chemical observation lies in the problem which is presupposed. In the study of mental imagery the sensorial type of observation has been found to be fruitful of results.

In approaching affective psychology one may take up the sensorial attitude and ask the question 'What?' 'What is *P* as given?' 'What is *U* as given?' The 'What?' attitude, consistently maintained, should yield all the 'Thats.'

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[15] Cf. Nafe, *op. cit.*, 538 f.

[16] P. T. Young, The phenomena of organic set, *Psychol. Rev.*, 32, 1925, 472-8.

One difficulty in answering the question 'What?' appears as soon as one attempts to observe directly and independently those characteristics of experience called 'pleasant' and 'unpleasant.' It is commonly maintained among psychologists that *P* and *U* lack clearness. When one attempts to observe directly a *P* or a *U* one finds only sensory processes. *P* and *U* are elusive; they are not directly observed.

The reason for this is doubtless the fact that the essential conditions of *P* and *U* are not in the stimulus nor in the receptor organ, but functionally behind the receptors. As evidence for this statement I note briefly: (1) The stimulation of any receptor or any group of receptors may yield an experience characterized as *P* or as *U*. No single sensory structure appears essential to the existence of affective processes. (2) With the pattern of stimulation remaining approximately constant experience may shift from *P* to *U*, and sometimes the shifts are rapid. (3) Experiences which are generally believed to be centrally conditioned—the experiences of memory, imagination, thought—may evoke intense affective processes.

The attempt to approach affective psychology from the sensorial point of view has this consequence: whatever is observed is sensory. Thus Nafe, assuming *P* and *U* to be 'palpable' found bright and dull pressures.[17] The sensorial attitude makes these pressures sensory in nature, however obscurely localized and ill-defined they may appear. The sensorial attitude yields something sensory, or nothing.

In the present experiment when the *O*s took the analytical point of view towards experience they observed qualities in the cutaneous-kinesthetic-organic group. They observed sensory patterns which *meant* avoiding or accepting, etc. Also they observed gross objects and characterized them as *P* or *U*.

(*B*) *The functional report.* My *O*s regarded *U* as the meaning of avoidance and *P* as the meaning of acceptance. Such statements are not analytical observations but functional reports. Following Bentley's terminology, we may distinguish the analytical *inspection* from the making of comments.[18] The *commentary* report may be illustrated as follows:

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[17] Nafe might object that the term 'palpable' is not restricted to sensorial observation. To this I reply, (1) if the term means more than sensorial observation, it is introducing still further confusion to affective psychology; and (2) as far as I can see the *O*s actually did take a sensorial attitude and they described in terms of pressure which is essentially a sensory process. The sensorial attitude in affective psychology has been discussed by Yokoyama. Cf. M. Yokoyama, Affective tendency as conditioned by color and form, this JOURNAL, 32, 1921, 88 f.

[18] M. Bentley, *The field of psychology*, 1924, 41-46, 197-199; The major categories of psychology, *Psychol. Rev.*, 33, 1926, 88-91.

After smelling a bottle of putrified fish the *O* replied: "I don't like the odor; it is very unpleasant; take it away." During the experiment I noticed that he was holding his breath and that he turned his head away from the bottle.

When the *O* reported "I don't like it" was he observing? The discussion has made it clear that he was not analysing, not inspecting, data from the sensorial point of view. His comment is descriptive of something; it is easily and generally understood; it is psychologically significant. The report 'I don't like it; it was *U*' is just as significant as the statement 'I feel a bright pressure.' The latter statement implies a sensory process; the former implies a dislike which is not directly observed as a sensory process, but which is merely reported unanalytically.

It is not only legitimate but scientifically worth while to assume the sensorial attitude towards the problems of affective psychology and to go as far as one can go with it. There are numerous problems which may well be approached in this way. I believe, however, that psychologists should also accept at face value the functional, commentary, unanalytical type of report. To accept this type of report as of genuine psychological importance opens the door to a non-sensory psychology wide in its scope.

The functional type of report furnishes the only evidence I know for the existence of affective processes. *P* and *U* are not directly observed from the sensorial point of view. Perhaps the most that can be said is that *P* and *U* are directly *felt*, that they are ultimate non-sensory data, and that the comment 'I like it; it is *P*' is a direct expression of the *felt* experience.

That pleasant and unpleasant experiences exist can readily be demonstrated. I believe that the average person upon smelling the putrid fish would express dislike in some way or other. The terms *pleasant* and *unpleasant* significally characterize gross experiences.

The functional report, it should be remembered, is not sensorial, not even analytical. If the question 'What?' must be answered, we may either (1) accept whatever a sensorial attitude yields, or (2) accept the functional report at face value and *infer* the existence of non-sensory processes of experience.

INCIDENTAL RESULTS

The following incidental results have come out of the experiments upon affective localization and the nature of the affective processes.

(*A*) *Results from a synaesthetic O.* S describes *P* as bright, light, almost white; and *U* as dark, dark gray, black. In the same context S also describes *P* as light yellow; and *U* as greenish yellow, slightly reddish, dark brown, brick red. The clearest examples are given below.[19]

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[19] A similar case has been described by Cutsforth. "In summary, pleasantness as an accompaniment to vision consists of reds, blues and yellows, and as an accompaniment to audition of rose-reds, blues and brighter yellows. Unpleasantness, as a visual accompaniment, consists of dirty browns and dull greens; as an auditory accompaniment it consists of dirty browns and less saturated, dead greens." (T. D. Cutsforth, The rôle of emotion in a synaesthetic subject, this JOURNAL, 36, 1925, 536 f.)

"The *U* increased in intensity and extent. It was a dark greenish yellow color with a smooth compact texture."

"The *P* was soft and fluffy, loosely woven in texture and very light in color, a sort of light yellow. The core was a little darker."

"Either the pain or the *U* was dark gray, almost black in color."

"The *U* was decidedly black in color."

"There was a tendency to withdraw and the tendency plus a blackness seemed to constitute the feeling."

"This *U* was followed by anger. This was localized in the chest and was like dull pressure and warmth. It seemed to well up, increasing in intensity and extent. It wasn't dark like *U*, but was brick red in color."

"*P* was bright, almost white."

(B) Waves and other movement of sensory feelings.[20] Three *Os* (K, S, W) report waves of feeling which move through the body, especially in jar, shock, or surprise. K and W are very sensitive to these moving experiences, and K reports them frequently.

Observer K: "What I felt was a wave of sensation which seemed to start about the ear and to spread as far as the belt. The wave was on the order of a succession of weak thrills" (noise). "I'd say that was *U*. I felt a thrill or wave going all through my body, legs, and arms. It was much farther reaching than any so far. I felt the thrill in my finger and even down to my legs" (sandpapered chin). "I felt a wave go all over me, down to the toes and hands—a wave of *U*, a *U* shock or jar. It was down to the toes and fingers before I realized it. It originated somewhere in the upper part of the body. I observed it going down the trunk and legs. It traveled fast and with a motion outward toward the toes and fingers" (odor of old cheese). "That may be characterized as *U*. What I felt was in the nature of a shock. A thrill or pulse of sensation spread through my body. I not only felt the wave or shock but after the first violent shock there was a tingling sensation in my legs. This was noticed in my knees and arms" (snap on face with rubber-band). "I felt a thrill go clear through me, down to the legs, a catching of the breath, a jerk of my body. It was *U*" (slap on the face).

The 'waves' move downward and outward, *i.e.* in the peripheral direction. Their localization is not very precise but their movement can readily be observed. The 'waves' move through the gross regions of the body and can frequently be followed to the fingers and toes. They are called forth by auditory, olfactory, tactual, and other stimulation. They are observed as sensory in nature but as regards definiteness of localization, movement, periodic character (tingling, etc.) they differ from the more familiar sensory processes.

In the above examples the experience is reported as *U* whenever the affective quality is specifically mentioned. The existence of *P* waves, however, is suggested by the following report:

"A tingle in my legs—more in the nature of a tingle than in the soft waves, as with *P* stimuli."

Observer S: "The feeling was very *U* and was accompanied by waves of dull pressure that travelled over the body, and muscular tension in the head and back."[21] "*P*; it was very much like waves of weak pressure travelling over the body, but the waves were not localized in the body but out in space." "*P*; there were waves of pressure beginning in my nose and running over my mouth and chest to my stomach." "Moderately *U*; then waves of pressure ran down the body and it became very *U*."

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[20] So far as I am aware these spatially moving feelings have never been adequately described. I have previously noted their existence, this JOURNAL, 29, 1918, 427.

[21] In view of Nafe's conclusion it is interesting to note that S describes *U* 'waves' in terms of dull pressure.

Observer W: This *O* is extremely sensitive. She reports a good many feelings which come in waves, sweep through the body, run through the body in a definite direction, spread, move up or down the arms and back, moving especially in waves or shivering. Examples are: "I had a distinct feeling of nausea which came in waves and was quite extensive. It didn't seem to be localized. It came in waves and receded." "Beginning in the region of my stomach and sweeping upward to the throat, as nearly as I could locate it, was a feeling which I could characterize as neither *P* nor *U*." "A quick jump with a certain amount of hand movements. A sharp feeling running through, almost a pain running from right shoulder in a diagonal way to my heart. Feeling takes on the characteristics of a line, a small area and a straight line shooting down. I can't catch enough of it to know its quality." "In the region of my stomach very quick feelings of nausea. Chills up and down my arms and back. They moved down from the throat regions in waves; they didn't come back." "I felt waves of something up and down my arms, not exactly chills, waves of feeling and I don't know how to describe that feeling." "A distinct *U* sensation. It sent all sorts of little waves of feeling rapidly all through my body. My right foot was tapping in annoyance while I was trying to swallow the liquid. A general bodily disturbance, the most marked of any I have had. I think there was a coldness in the creepy feeling, a difference in temperature from the temperature of my skin. I wasn't sure whether the creepy feeling was colder or what." "A definite feeling down my throat and into the stomach. Waves of nausea. Shivers down back and arms. I puckered my mouth, shook my head. Uncontrolled shivers through arms and spine and in a straight line through stomach. I didn't seem concerned that the taste was in my mouth."

The complete description of these moving bodily feelings would constitute an interesting problem in analytical psychology.[22]

(*C*) *Strain and relaxation in relation to P and U.* In a previous paper, I pointed out an association of *U* and strain, and of *P* and relaxation.[23] The present data confirm the association.

Association of P and relaxation. Examples of the association of *P* and relaxation are:

Observer K: "The experience was restful, quieting, soothing, relaxing —*P*. There was some kinesthesis in thoracic viscera and throat (meaning effort to draw in the odor to get more of it)." "This experience was *P* to me. In this I experienced a tension of the muscles just before the stimulus was given (this was suspense and expectancy). With the stimulation came a relaxation, a comfortable and agreeable feeling. It was an experience to be desired, something I should like more of." "Very *P*; there was something soothing and relaxing in the experience. It seemed soft and comfortable and there was something in the quality of the odor which I liked and which seemed to set me in a relaxed and surrendered bodily state. The agreeableness of the bodily state was observed throughout the thoracic viscera especially. There was some vague and obscure imagery which meant flowers and beautiful things in general." "I felt myself relax as the tones continued and as this went on the *P* seemed to increase and I observed then some light pressures in the chest which seemed to add to the agreeableness of the total situation."

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[22] If any one contemplates making such a study, it should be remembered that individuals differ greatly in the frequency with which they report these experiences. The investigation should be carried on from the sensorial point of view.

[23] P. T. Young, Pleasantness and unpleasantness in relation to organic response, this JOURNAL, 32, 1921, 40-44.

Observer G: "The odor was soft—*P*. A sudden dropping out of organic and kinesthetic processes. General bodily relaxation, diffuse feeling of comfort."

Observer S: "I experienced a distinct relaxation and this was or meant *P*."

Association of U and strain. Examples of the association of *U* and strain are:

Observer K: "This experience was *U* to me. I was in a fairly relaxed condition just before the stimulus was given. When the stimulus was started there was a sort of tightening of muscles, a sort of an attempt to brace myself against this noise." "The *U*, as far as I am able to discover, seemed to lie in the muscular response. I felt a very decided contraction of the muscles of the face and especially of the eyes and region about the forehead."

Observer S: "Very *U*. The *U* was fused with pressures which shot back and forth through the chest and head. A great deal of tension in the muscles which meant that I wanted to draw away from the stimulus." "Very intense *U*, the kind that seems unbearable. Accompanied by muscular tensions all over the body." "A strong desire to pull away which was carried by muscular tensions." "The *U* was made up of shrinking or muscular tension." "A great deal of tension of the muscles which meant that I wanted to draw away from the stimulus. There were streaks of greater *U*, which appeared at times in the complex of *U* and pressure."

I might point out, as I have previously done, that the correlation in question is by no means perfect. Frequently there are strains—especially those involved in surprise, attentive observation, anxiety, fear and other attitudes—present in indifferent and in *P* experiences. There may also be *U* relaxations, although I can find no evidence for them in my data.

III. The 'Trained' Observer in Affective Psychology

In view of the present confusion in affective psychology it was deemed advisable to make a minor experiment bearing upon the methodology of analytical psychology in the sphere of affection. If one control the stimuli and other important factors in an experimental situation, and if one control the instructions and suggestions presented to the *O*s, will practice (in the sense of repeated observation) bring a group of *O*s into approximate agreement regarding the critical point of the experiment?

I decided to put the question to a test using Nafe's recent experiment and limiting the problem to his chief conclusion, namely that: "The affective qualities, pleasantness and unpleasantness, turn out, under direct observation, to be modes of pressure: *pleasantness is a bright pressure, and unpleasantness is a dull pressure*."[24]

In the following experiment I have endeavored to duplicate Nafe's experiment, using his stimuli, his instructions, and reproducing so far as possible the total experimental situation.

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[24] J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 508.

Observers. I selected 3 *Os*: Dr. Elizabeth Möller (M), instructor in psychology; Dr. E. A. Esper (E), assistant professor of psychology; and Miss Lillian Nilson (N), undergraduate student majoring in psychology.[25] M, who holds the doctoral degree in psychology from Cornell University, has been impressed by the Titchenerian creed. E completed his doctorate in psychology in Ohio State University. He is of the behaviorist's faith. His views upon the aims, problems, and methods of psychology differ from the views of M as widely as black differs from white. N is approximately neutral as regards psychological education. She is a junior at the University of Illinois, a normal, competent individual who is quite innocent of theoretical views upon introspection, feeling, and other psychological matters. All the *Os* took the work seriously and faithfully.

Stimuli. The following stimuli were used in the experiment. The 'catch-word' which represents them appears in parentheses.

Visual: (colored strip) a series of colors painted on a strip of paper, mounted on a card, representing the complete series of hues, exposed in moderately bright artificial light for 5 sec.

Auditory: (chord) four forks, C, F, A, C′, struck in the order given with a very light blow. The chord was allowed to continue about 3 sec., and the forks were then damped in the same order; (loose fork) a fork mounted loosely to the resonator so that when struck rather violently it produced a rattling, noisy tone. It was sounded from 1 to 3 secs. In addition to the above, which were used by Nafe, I also used the chord C, E, G, C′, and the noise of filing glass close to *O*'s ear.

Olfactory: (presented in bottles) oil of peppermint, old fish, caryophyllorum, stale cheese, menthol, nutmeg, fish oil, white rose perfume, anethole, phenol, pyridine, camphor, methyl salicylate, ethyl cinnamate, geraniol, oil of mace, napthaline, nitrobenzole, carbon disulphide. The *Os* were instructed to breathe normally. The time of exposure was the time of one inhalation, but occasionally two or three inhalations were permitted. These odors are in large part different from those used by Nafe; but for purposes of the experiment they are equivalent in that they are capable of arousing affective experiences of moderate intensity.

Gustatory: vinegar, salt solution, concentrated sugar solution, sarsaparilla. A few drops of the latter were placed on the tongue by means of a spoon. One drop of the other solutions was placed on the tongue by means of a camel's-hair brush.

Tactual: (hot steel) a piece of steel, about 1 in. in diam. at the contact surface, heated to 50°C or above; (cold steel) a similar piece of steel cooled to about 10°C. These stimuli were applied to the volar surface of the forearm for 3-5 sec.; (hair) a moderate-sized camel's hair brush drawn two or three times across the forehead or lips; (glass) a smooth piece of glass moved over the tips of the fingers; (sandpaper) a piece of No. O sandpaper moved over the tips of the fingers, the chin or the nose; (eraser) a felt eraser, filled with chalk dust, moved across the tips of the fingers; (nail) a sharp nail pushed slightly into the volar surface of the forearm to give moderate pain; (string) a piece of heavy string with frayed end which was moved across the closed lips of *O*.

A comparison of the above list with that of Nafe reveals that, apart from the olfactory group, my stimuli are almost identical with those used by him.[26] In the olfactory group and in the case of a few additional stimuli the two sets, Nafe's and my own, are equivalent for purposes of evoking moderately intense affective experiences.

The method of presenting the stimuli was the same in both experiments except for the following inconsequential differences: Nafe did not hold his

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[25] Nafe also employed an undergraduate as a control (*op. cit.*, 510).

[26] *Op. cit.*, 509 f.

Os to any fixed number of reports at a particular sitting, whereas I obtained a series of 8 reports at every experimental sitting regardless of the time consumed. Every series began with 2 odors and ended with 2 odors, and the remaining 4 stimuli were selected from the tactual, auditory, gustatory, and visual groups. I made much use of odors on account of their well-known capacity to evoke *P* and *U*. No stimulus was used twice during a single experimental sitting.

Procedure. The experiment was done during the fall of 1925. Every *O* observed twice a week for 5 weeks. As 8 reports were taken at every observational sitting, a total of 80 reports were thus obtained from every *O*. The work for a given *O* was done at the same hour of the day throughout the period. The time of the sittings varied from 20 min. to 1 hr., according to the length of the reports.

The *O* was seated in a comfortable chair which was enclosed in a 7×9 ft. booth curtained off for the purposes of the experiment. The stimuli were kept on tables outside of the enclosure. The sound of *E*'s approach served as a warning signal. *O* kept his eyes closed during the stimulation, except when visual stimuli were used in which case he opened his eyes at *E*'s request. The verbal reports were recorded literally as given, and are given below unedited as they appear in the protocols.

Instructions. At the start and also at the close of every experimental sitting *O* was asked to read the instructions. This was done to impress *O* with his observational task and to make certain that the instructions would be a determining condition of his reports. The instructions were taken from Nafe.[27] They are:

"I shall give you a stimulus which is intended to arouse a moderately pleasant or a moderately unpleasant sensory experience. You are to direct your attention as exclusively as possible to the affective side of the experience. When you judge that the feeling is at its maximum, break off your observation and describe the feeling itself as accurately as you can."

This instruction was supplemented according to Nafe by the following:

"Continue the effort to get affection palpable under observation for itself. Look for concomitant organic sensations with a view to determining whether affection is simply a meaning laid over these organics."[28]

The first instruction was used for the first 24 reports. Both instructions were used together for the next 16 reports, and the second instruction was used alone for the last 40 reports. I am not able to discover exactly how Nafe made use of the two instructions.

RESULTS

For illustrative purposes I have selected one report from the protocols of every *O* for every day of the experiment. I selected the fullest and most complete, and present them unedited, just as they appear. The order in which they are presented is that in which they were given. The number in parentheses indicates the serial position, the 'catch-word,' gives the stimulus used.

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[27] *Op. cit.*, 510.

[28] When handed these instructions N asked to have certain words explained. My explanation was off-hand as the question was not anticipated. The words and the explanations as given are here recorded: (a) *Affection*: this term refers to pleasantness and unpleasantness and possibly to other feelings. (b) *Palpable*: this literally means 'touchable,' *i.e.* observable. The point is whether you can observe feelings of *P* and *U* directly *as* feelings. (c) *Concomitant*: this means present at the same time. (d) *Organic sensation*: the term refers to pressures, aches, warmths and other sensations localized in or referred to the body. (e) *Meaning*: I can't fully explain this term. It is the same as 'significance,' and the word is used here as in every day life.

For the sake of uniformity I asked M and E whether they understood the instructions and they replied that they did; consequently no explanation was given. Both, however, complained of difficulty in carrying out the instructions: M about the instruction to "break off the observation when feeling is maximum;" and E about the instruction to "look for concomitant organic sensations."

Observer M

Reports: (3—camel's hair brush) The experience was *P* and that *P* seems to be a pressure. It isn't a pressure on anything, and it's a pressure that is light gray. I don't like to say it is gray and yet gray is the nearest thing it is like. This pressure is unlocalized. That's what I mean when I say it is not on anything and it seems to be just all of experience for an instant or two. I can't point any place and say, 'That's where it is' because it isn't that kind of an experience.

(13—noise) That was *U*. There seemed to be a big gray band of experience, and when the *U* comes in it comes against this background. In this case the *U* part was rather heavy, compact, a dense thing that is much like pressure. It has nothing visual about it and yet I want to say it is almost black. The pressure is just everywhere, not localized any place and the black doesn't appear any place.

(22—C, F, A, C′) When *P* appeared it seemed to be like a light, steady pressure. It is something that is big or that has volume as a cloud has. This wasn't a volatile, dancing pressure, but one that was smooth and soft and steady. It isn't a pressure at any place, but it exists and seems to exist as air does. There was a definite coloring in this one. This pressure seemed to be in a gray band. When I'm talking about the background I am never there as looking at it. It goes on.

(30—noise) That was *P* but it should have been *U*. The *P* part was bright; it was springy, buoyant, and the little pressures went up. Made up of lots of light pressures; they were in motion all the time. They were bubbling up, and yet they don't bubble any place.

(35—sandpaper nose) Slightly *U*. The *U* was sort of heavy, like a dull pressure as opposed to a bright pressure, a dead heaviness. That was noticeable in this case because the experience was rough and yet the *U* wasn't rough itself. It was smooth. I didn't notice any organics. The *U* seems to be the whole bit of experience. There doesn't seem to be anything else there except the carrier of the experience.

(44—camel's-hair on palm from wrist to finger tips) Slightly *U*; the *U* is dull. I think that is the most striking difference between *P* and *U*. The *U* ones are all so dull. It wasn't like a heavy pressure. More like a dull pressure than anything else. There is a kind of solidity about it which the *P* ones don't have. The *P* ones are like film but this one is more like a surface. Yet the film and surface aren't any place. No organics noticed. In this case it was quite obvious that the *U* didn't arise till after the cutaneous experience had gone on a little while.

(55—geraniol) Moderately *U*. The *U* is something that goes down. It was dense and dead, most like a dull pressure although it wasn't a pressure any place. It was unlocalized except that it seemed to be everywhere, but yet there was a background for it. I didn't notice any organics. As soon as the odor went the affection was gone too.

(59—C, F, A, C′) That was *P*. The affective experience in this case ran a certain course. The first auditory experience was itself *P* and the *P* was springy, volatile, something like gray and something like pressure, without being either gray or pressure. As the other auditory experiences (tones) came they somehow mingled with the first *P* and the experience was then more uniform. It wasn't quite so bubbly and springy. It was, however, something light that pressed up rather than down, and it was gray. Somehow it was a light gray. After the experience had gone on for a while I broke it off to look for organics. Of course I found some but they weren't related in any way to the affective experience.

(68—noise) That was moderately *U*. The *U* is just as palpable when it comes as the auditory experience itself. It rises to a maximum and seems to dominate all of experience so that other things drop back to a low level.

The *U* was heavy, dull, inert. I think I get its dark meaning from the heavy clouds of smoke and from dull, black bits of metal. It is really like a dull pressure only it isn't a pressure any place.

(75—colored band) That was moderately *P*. The *P* is soft and it seems to be light and to push up instead of down. It is more like a film than anything else. It is misty and there is something bright about it. I didn't notice any organics.

Questionary. At the close of the experiment M was asked a few questions, a record of which appears below:

Q: Were you an observer in Nafe's experiment?

A: Yes.

Q: Have you read Nafe's published report of his work?

A: I am not sure whether I have read it or not. [Note: as this appeared only one month prior to the present experiment it is not likely that M had read the report. If read, it did not leave much of an impression.]

Q: Will you give me briefly your theoretical views regarding *P* and *U*?

A: I think they are real qualities which are just as observable as visual or auditory qualities, and that they can't be described adequately because they are ultimates. They are alike in lots of ways. For example, both *P* and *U* seem to be like pressures and sometimes they have a reference to gray. *U* is smaller than *P* experience and it is more compact while *P* is diffuse. *U* is like a pressure down and *P* like a pressure that is springing up. *U* is dark when it has a color quality there. *U* is like a dark gray and *P* is like a light gray.

Q: Has the experiment changed your theoretical views?

A: My view has been changed in that I am uncertain about that color business, where it is or if it is. I did have a bias at the start because I think that ultimately everything will reduce to pressure.

Q: What, in your view, is the purpose of this experiment?

A: I think it is first of all to determine whether affection can be observed and then to get a description of affection.

Q: Do you think this experiment is comparable to Nafe's, or do you notice any differences?

A: The first instruction "break off your observation and describe the feeling" bothered me all through because I couldn't observe affection and at the same time note when it reached a maximum. Outside of this I don't remember any difference between the two experiments. [Note: The present instruction is word for word the same as indicated by Nafe.]

Q: What do you think about the instructions?

A: I think they are all right except for the above point. In looking for concomitant organics you can't break off the experience and be set to notice organics.

Q: Do you have any criticisms on the experimental technique?

A: No.

Q: Where did you get the hint that *P* and *U* are like pressure?

A: It just came out of a clear sky. All of a sudden I had this peculiar experience, this pressure that wasn't any place, which seemed to exist absolutely independently of me. I don't think it was suggested because I remember the shock I had when I observed it first and from that moment dates my bias towards pressures.

Discussion. An impartial reader of M's reports would gain the impression that M is struggling with words in an endeavor to describe *P* and *U*. *P* is something bright, light (in the sense of not heavy), bubbling, rising up, billowy, sparkling, volatile, airy, springy, diffuse, alive, dancing, smooth, soft, buoyant, like clouds or like smoke, made up of lots of light pressures, penetrable, misty, like film, active. *U*, on the other hand, is something dull, dark, dead, heavy, small, compact, black, rough, grainy, like gray, dense, thick, pushing down, like dark or heavy smoke, inert.

The contrast between *P* and *U* is expressed in the frequently used pairs: bright-dull; alive-dead; rising up-sinking down; active-inert.

The affective processes together are said to be 'like pressure' or 'like gray.' I doubt whether M's reports justify the assertion that *P is* a bright pressure and *U is* a dull pressure; but there is no doubt that the reports are fairly consistent in stating that *P is like* a bright pressure and *U is like* a dull pressure. Also *P* and *U* are similar to visual brightnesses.

If the list of terms, descriptive of *P* and *U*, be compared with Nafe's list,[29] and especially if M's former and present reports be compared, then it appears that the present experiment is a rough confirmation of Nafe. Further, M. herself stated, as shown above, that she noticed no marked difference between the experiments.

OBSERVER E

At the close of every sitting, except the first, E was asked: Are there any comments you would like to make about to-day's experiment, or about the experiment as a whole? These comments are the most illuminating part of the record and appear below as 16A, 24A, 32A, 40A, 48A, 56A, 64A, 72A.

Reports. (6—C, F, A, C′) I was awaiting with pleasurable anticipation the addition of further tones. I derived considerable *P* when I heard them all together. I was disappointed when they didn't drop off in the same uniform manner as they had been added.

(9—carbon disulphide) I was expecting that glue smell, so the first thing I got out of it was that it wasn't so bad as I expected. As you went away 'cooked onions' occurred to me. [Note: O turned his head away from the bottle.]

(16A) It isn't clear to me what problem I am to set myself. If I ask myself, 'Is this *P* or *U*?' I could probably answer it one way or the other each time. If I took Wundt's system, I could probably answer in the six ways. I myself am not convinced that there is anything but momentary significance to it. There is practically nothing I can say about the experience as such.

(18—phenol) Again rather *P*. Weighted with a lot of associations of childhood experiences, various occasions in which carbolic acid entered.

(24A) I find I don't have much success in carrying out the instructions. There doesn't seem to be much affection to turn my attention to.

(26—stale cheese) Again the response released, in spite of instructions, is an identifying response. I am quite unable to report any affection other than avoidance. Imagery of decayed cheese. [Note: O turned his head away.]

(32A) It appears to me that it is quite impossible to carry out instructions because the stimuli are so indifferent. The only behavior released is an identifying behavior and possibly then a withdrawing. I don't find anything to report except in some cases *P* or *U*, but I am not convinced that those reports mean much more than a recording of my avoiding reactions or non-avoiding reactions. The terms *P* and *U* don't stand for something more than those avoiding reactions. It is a bare verbal report which means nothing more.

(39—fish oil) Well, that was perhaps more interesting than the others. If I had been left to myself, I would have continued smelling it longer to identify it. I wasn't sure whether it was agreeable or disagreeable.

(40A) When I say 'pleasant' it doesn't stand for anything more than 'I would smell it more if I could.' I am not able to discriminate anything beside that in most cases.

(43—salt) Merely identification as salty. I might add, perhaps *P*, but that would be merely a verbalism. It doesn't stand for anything else.

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[29] *Op. cit.*, 517.

The response 'pleasant' pops out as in an association experiment. Organic sensations simply don't enter in at all. I seem to be doing business exclusively at the upper end of my body.

(48A) I find this difficulty. The instruction asks me to watch for organic sensations. That means that I have a tendency to respond to various conditions in the abdomen which are present all the time but to which I don't ordinarily respond. When I read the instructions I begin responding to them. I have difficulty in knowing whether I am reporting something as from the stimulus or from other conditions after stimulation. An utter blankness as to what to report. Usually there is nothing except stimulus is so-and-so or suggests so-and-so.

(52—colored band) Well, I identified it as a spectrum and perhaps found some pleasure in inspecting the colors although I don't know what that means when I make use of that expression except that it is the verbal equivalent for continuing to look.

(56A) The judgments of *P* and *U* are merely verbal symbols for movements towards or away from the stimulus, continuing or avoiding. Such questions as 'Why is it *P*?' or 'What is it?' release no further response than, for example, 'The odor is *U* because it stinks.'

(63—anethole) A suggestion first of an association with some sort of medicine. Perhaps a slight nausea of the sickly sweet type again, although I am not at all certain of that. At the present instant I can discriminate nausea. I think the sequence is a verbalism 'sickly sweet' that reminds me of the instruction about organic sensation, and that leads me to make abdominal discriminations which are likely to be of the order of nausea. It happens that my digestive processes are not perhaps what they should be to-day.

(64A) I find the instructions very vague. When you ask me to report on my affections, or my feelings, it isn't at all clear to me what that means specifically except perhaps that I can verbally report that this or that is *P* or *U*, or that I can report that one thing is more *P* than another.

(71—nutmeg) Very interesting and *P* odor. Probably I would continue investigation of it for some time if left to myself. Vague spicy suggestion and other factors which I didn't have time to analyse out.

(72A) It has occurred to me that in most of these stimuli if you consider my probable responses under ordinary conditions, none of them last long enough to be sure whether I would avoid or continue listening. I don't get past the point of finding out what the situation is in most of them. Take, for example, those cheese odors. One whiff is enough; I would probably get away. Even in case of a given stimulus when I report *P* possibly a little bit more of it would make me respond by getting away. I hardly get past the stage of a hasty identification of the stimulus which usually consists of a few associations.

(75—colored band) Well again subvocal *P*, warm, cheerful. A rather active examination of the stimulus field and had the stimulus continued I'd have settled back and looked at it with interest. I had just about got adjusted to it when the stimulus was removed. I was at first looking too high. It took me an instant to find the stimulus.

Questionary. At the close of the experiment E was asked a few questions. A complete record of questions and answers is given below:

Q: Are you familiar with Nafe's experimental study of the affective qualities?

A: No.

Q: Are you acquainted with Titchener's touch pyramid?

A: No.

Q: Will you state briefly your theoretical views regarding pleasantness and unpleasantness.

A: My views on *P* and *U* would coincide with those of Max Meyer in the *Psychological Review* about 20 years ago in which *P* is regarded as facilitation of behavior, and *U* as interference.[30] With respect to the present experiment, if anything, I have been confirmed in my belief in the adequacy of that view, although as a matter of fact that didn't occur to me till to-day.

Q: What do you think is the aim of this experiment?

A: My answer would be colored by my knowledge of the subjects. You told me who your subjects were.[31] I assume that the experiment has as its object the comparison of data obtained from individuals with different types of training. Perhaps this is a study of the validity of introspective reports in connection with feeling experiments. I have assumed that if you wanted a mass of introspective data on feeling you would get some introspective experts rather than myself and one other naïve individual.

Q: Do you think your suspicions about the nature of the experiment influenced your reports?

A: No. I was trying to follow instructions.

Q: What do you think of the instructions?

A: The part about 'organic sensation' annoyed me very much. I seemed to be doing business with the upper part of my body and I had to identify and this took most of the time. There wasn't time for other types of discrimination such as organic. I was also disturbed by paying attention to the abdominal region rather than to the stimulus. When I was discriminating what was going on in the abdomen I wasn't responding to the stimulus.

Q: Do you have any criticisms of the experimental technique?

A: No, because I don't know its purpose.

Q: Is there anything further, in general, to add?

A: No.

Discussion. E's reports do not give a hint of anything comparable to the statements made by Nafe's *O*s. Not once are *P* and *U* described as pressures, as similar to pressures, or as associated with pressures. Not once do the terms 'bright,' 'dull,' or their equivalents, occur. Why has E failed so completely to confirm Nafe's conclusion?

Taking E's reports at face value, one finds therein a fairly consistent view of *P* and *U*. 'Pleasant' and 'unpleasant' are verbal responses, and as such *P* is the equivalent of letting the reaction continue and *U* is the equivalent of an avoiding reaction. At the close of the experiment E stated his theoretical views upon *P* and *U*. So far as I can see, E's reports demonstrate or 'prove' his view to be 'correct.' The view in question can be referred to E's training in psychology at Ohio State University.

M also, at the close of the experiment, expressed her views upon affective processes. So far as I can see, M's reports and her theoretical views are consistent. M's views are derived from psychological education at Cornell University.

It appears, therefore, that within the limits of practice set down in the present experiment, mere repetition of observation under controlled conditions has failed to bring two *O*s with radically different psychological education into agreement. Each *O* reports in a manner which is fairly consistent with his own theoretical views.

At this point let me anticipate certain objections which some of my readers will raise.

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[30] M. Meyer, The nervous correlate of pleasantness and unpleasantness, *Psychol. Rev.*, 15, 1908, 201-216, 292-322.

[31] At the close of the experiment on Nov. 19 (after report 48A) E asked: "How many subjects do you have?" I replied, "Three—Miss M, yourself, and a student." E then said, "It is a clever idea to use such different subjects; you are really working on observation, then, and not on feeling. I replied, "This is a study in feeling." E added: "Excuse me for anticipating; we'll talk it over when you are through!"

Although E had guessed the purpose of the experiment at this date, I doubt whether this guess influenced his reports. His data are uniform throughout.

(1) E's reports are 'common sense,' '*Kundgabe*,' 'mere casual comments.' If such terms be used to describe a specific type of report and to distinguish it from other types, I see no objection to them. If such terms be used for purposes of wholesale condemnation—as they frequently are—I object to this method of clearing up the situation. To throw out all of E's reports as '*Kundgabe*,' or as something else equally terrible, is to avoid the problem. One would be equally justified in throwing out all of M's reports on the ground that they are 'poetic,' 'literary rather than scientific,' 'over-imaginative,' 'biased by psychological preconceptions,' etc. Either we should discard both sets of reports and thus avoid the problem, or else we should face the problem squarely.

Every report of E, without exception, is straightforward, understandable, clear-cut. Literary style may be criticised but this is wholly beside the point; all printed reports are raw data, just as they were dictated in the laboratory.

(2) It may be objected that E failed to carry out the instructions.

The fact is that E understood the instructions and with good will carried them out to the best of his ability. There is good evidence that the instructions essentially determined the nature of E's reports.

It is true that E did not talk at great length about *P* and *U* as qualities of experience, but in this particular experiment the question at issue is whether *P* and *U* are 'palpable.' Possibly E did not observe *P* and *U* for the simple reason that he found nothing affective which was observable; he found only reactions, etc. If E *failed*, the same argument shows that M *failed*.

(3) If E had had more practice in observation, he would then agree with M and with Nafe's other *O*s.

It is true that M had had more training in introspective psychology at the start of the experiment than E had at its close. M observed throughout a major experiment quite like the present one; E, on the other hand, was serving his first term in this type of experiment.

It is impossible to predict what the situation might be after 800 reports; but after 80 it is clear that E's reports are not in any way approaching M's. E's type of report is fairly stable and I guess that further repetition would give only further habituation to a type of report in the case of E and also in the case of M.

Fundamentally I object to taking M's reports as a standard and asking 'Why did E *fail* to report as M did?' It is equally straightforward to ask, 'How much longer would the experiment have to continue for M's reports to 'fall in line' with E's?' I think it is just as likely that further practice would modify M's report as E's.

If it be true, as I am claiming, that the theoretical views of an *O* are essential conditions of his report, then a change of 'heart' rather than further practice is needed to bring about uniformity in a group. Why should we assume that one report is better than another?

OBSERVER N

At the close of three of the series N made comments which are reported below as 16A, 48A, and 56A.

Reports. (7—caryophyllorum) That was nice. It was a spicy smell. I'd say it was *P*. It would be more *P* if it hadn't been quite so intense. Didn't bring up any gustatory imagery.

(9—carbon disulphide) That was *U*. It was half-way between nauseating and sickening. It had something in it that smelled a little bit like ether. It was moderately intense. I just now get an after-image of it. I still have it faintly.

(16A) The ones you gave me are chiefly *U*. I believe you gave me only *U* ones in the series. Most of them were very *U* and especially the ones like decaying food matter. I got after-images of the most intensely *U* ones and not of the mild ones.

(23—pyridine) It's the worst one I've had so far. Very *U*. I get a very strong after-image of it. I still have the after-image and I still have the taste in my mouth. It is a nauseating odor, very much so. It left a terribly bitter taste in my mouth. Now the image of the smell itself has left but I still get the taste.

(29—C, E, G, C′) First two notes were *P*. The third note made a noise, so did the fourth. Towards the end of the vibration the whole became *U*. No after-image and no organic stimulation. Each note in itself was *P* but when the last note was combined with the others it gave the *U*.

(37—vinegar) That was *U*. It had a very sharp taste. It gave a peculiar *U* sensation in the stomach. I still taste it. It has lost its sharpness but it is still *U*.

(41—oil mace) That was slightly *U*. I guess it was *U*. If it had been a little milder, it would have been *P*. It was a strong spicy smell.

(48A) I haven't yet found *U* as organic except in one or two instances—in a smell and in one taste there were organics. I did get organic sensations in drawing my hand away in the pain. I drew away from the sound a few days ago.

(52—colored band) I'd call that *P*. I like the brighter colors toward the center best. I thought most of the colors were good colors.

(56A) There is some organic reaction with the *U*. Odors make you pull your head aside or shudder. Mild stimulations don't seem to give the organic reactions that the strong ones do, or if there is any organic reaction, it is so slight that I didn't notice it.

(58—phenol) That was mildly *U*. It smelled like strong carbonated vaseline and it brought up associations about the sick room.

(70—hot steel) That was *U*. It was hot and it made me unconsciously try to draw my hand away. It left a pain sensation. I felt heat while the heated object was pressed on it. Now I feel pain.

(77—sarsaparilla) That's very *U*. It gives me a slight shuddering feeling. Taste is still in my mouth.

Questionary. After the experiment N was asked the following questions. A complete record of questions and answers is given.

Q: Are you familiar with a study of feeling by Nafe?

A: No.

Q: Are you acquainted with Titchener's touch pyramid?

A: No.

Q: Will you tell me briefly your theoretical views about *P* and *U*.

A: My opinion is entirely unbiased. I don't think I had any views at the start and I don't have any views now. I don't think *P* and *U* are much connected with organic sensation. It is only in extreme *U* that organics appear. I haven't had much *P* so I haven't formed any view about that.

Q: Have you read any psychological papers on feeling?

A: No.

Q: What courses in psychology have you taken?

A: I have had Psychology I (introduction) and II (general), and I am now taking III (elementary laboratory) and VII (mental imagery). Feeling has not been stressed in these courses and I think I am really unbiased.

Q: What do you think the experiment is about ?

A: I think you are trying to find out whether the sensations which you give (odors, etc.) are connected in any way with bodily sensations.

Q: What do you think about the instructions ?

A: I think they are all right. I think they are clear. I wasn't quite sure of how much you wanted in the introspection.

Q: What did you understand your task of observation to be ?

A: First whether it was *P* or *U* and to describe the feeling if I could and to tell whether it was connected with organic sensations. I gave that about after-images but I don't know whether you wanted it or not.

Q: Are there any criticisms of the experiment?

A: No. Only one thing. I don't see where the *P* comes in. I think it was an experiment chiefly on *U*.

Q: Is there anything further you should like to add?

A: Nothing now.

Discussion. The terms 'bright pressure,' 'dull pressure' or their equivalents are not to be found in the reports of N. There is no hint that *P* and *U are* pressures of any kind, or that they are associated in any way with pressures.

N makes use of such psychological terms as she has acquired from her courses of study. The frequent reference to after-images should be related to the fact that N, during the experiment, was registered in a course upon mental imagery. In reports (not printed) describing auditory experiences N uses such words as 'partial tones,' 'volume,' 'beats,' 'brightness quality,' 'intensity,' etc. These descriptive categories are most certainly derived from Psychology I and III. Also in reports of smell qualities and associations the influence of elementary courses in psychology is apparent. Had N been acquainted with Titchener's touch pyramid[32] as were *all* of Nafe's *O*s,[33] the terms 'bright pressure,' and 'dull pressure' might have appeared in N's reports.

The reports of E and N are similar in that both are relatively brief and also contain a good deal of material beside the point of the experiment. Both are naïve. M's reports, on the other hand, are longer, possibly more to the point of the experiment, and certainly more sophisticated than those of E and N.

SUMMARY

Nafe's experiment was repeated using one of his original *O*s. The present reports of this *O* duplicate her former efforts. A second *O*, who had had a radically different type of psychological education, failed completely to confirm Nafe's result but gave reports consistent with his theoretical views upon *P* and *U*. A third *O*, relatively unsophisticated in psychology, also failed to confirm Nafe's result; her records show the dependence of report upon psychological vocabulary.

Practice in the sense of mere repetition of observation under controlled conditions is not sufficient to duplicate Nafe's result. Practice was limited to 80 reports, 8 a day for 10 days, and at the close of the experiment there is no hint that further practice would change the result. It is just as likely that M would change her type of report with further practice as that E and N would modify theirs.

A 'trained' *O* is not necessarily one practiced in psychological observation. The important factor in training is the psychological education or indoctrination to which the *O* has been exposed. The terms of description which an *O* has acquired, the psychological facts he knows, and his theoretical views very largely determine the type of report.

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[32] E. B. Titchener, this JOURNAL, 31, 1920, 213.

[33] *Op. cit.*, foot-note 517 f.

IV. THE LOGIC OF AFFECTIVE PSYCHOLOGY[34]

The confusion and contradiction found to-day within affective psychology are notorious. Upon the most fundamental matters there is little agreement among psychologists.

If one consider the problem of the basic nature of affective processes, one finds at least three contending views. (1) There is the traditional view of Titchener and others that *P* and *U* are unique, non-sensory, non-clear mental processes which vary in duration, quality and intensity. This view was tacitly accepted by myself in 1917-18 when working on the affective processes at Cornell, and the view seemed to work very well in the laboratory.[35] (2) There is the view expressed by Yokoyama in 1921 that *P* and *U* are statable as meanings.[36] Two of Yokoyama's *O*s, confessing to be biased in favor of this view, confirm it in their observations. All of the *O*s may report *P* and *U* as meanings. (3) There is a view most recently stated by Nafe that the affective qualities under direct observation are modes of pressure: *P* is a bright pressure and *U* is a dull pressure.[37] This is a special variety of the sensational views of affection.

In less fundamental matters a similar state of turmoil prevails. I have previously pointed out that the uncertainty about the localization of affective processes rests upon ambiguity of the terms 'feeling' and 'localize.'[38] Confusion about the antagonism and coexistence of *P* and *U* can be traced to the common use of ill-defined terms such as 'cognitive awareness,' 'meaning,' 'affective process,' etc.

In Section III it was demonstrated that experimental data in affective psychology depend largely upon the theoretical views of the *O*.[39] This consideration in itself brings to clear light the source of the present confusion. How can we hope to *discover* anything in affective psychology if the data from which we start are so largely *made* by our logic? I will return to this question later.

At this point let me pause to give an analogy. When visiting Yale University a friend once demonstrated a dog that had been trained to lie down as if dead whenever the name 'Harvard' was pronounced; if one should say to the dog 'What would you rather do than to go to *Harvard* ?'

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[34] The material of this paper was presented to the American Psychological Association at Ithaca, N. Y., December, 1925.

[35] P. T. Young, An experimental study of mixed feelings, this JOURNAL, 29, 1918, 237-271.

[36] M. Yokoyama, The nature of the affective judgment in the method of paired comparison, this JOURNAL, 32, 1921, 358, 368.

[37] J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544.

[38] This JOURNAL, 38, 1927, 180. "As to the meaning of the term (feeling), it is plain that further definition is requisite for a word that may mean (*a*) a touch, as feeling of roughness; (*b*) an organic sensation, as feeling of hunger; (*c*) an emotion, as feeling of anger; (*d*) feeling proper, as pleasure or pain." J. Ward in *Encyclopaedia Britannica*, article 'Psychology,' section 5.

[39] I have long questioned the emphasis ordinarily given to 'training.' It is customary to give the academic training of the *O*s, their degrees, and professional status, and frequently their psychological biases. See, for example, Yokoyama, *op. cit.*, 358.

immediately the dog would drop down as if dead. Now since one dog can learn a trick, it is probable that a group of dogs could be trained to react similarly and uniformly to the spoken word of 'Harvard.' Doubtless also another group of dogs at Princeton University could be trained to sit up uniformly and bark whenever the word 'Harvard' be pronounced. Now I ask: Which is the true, the genuine, the *scientific* type of behavior? Which type of behavior most accurately describes or expresses the ultimate essence of existence?

So in affective psychology one kind of training and one set of laboratory conditions may lead to a roughly uniform type of verbal report; but another type of training may yield an incompatible, contradictory result. Which type of report is true, and which is false? My own answer is that both results must be regarded as a product of conditions. The illustration from animal training is more than an analogy; for biologically regarded the human observer is also an animal and his training in speech is quite comparable to the training of the dog.

I am aware that some of my readers will object. The report of a trained *O* in a psychological experiment is not wholly the product of training. Surely there are existential mental processes, sensory or other, which at least to some extent determine the *O*'s report. Surely the introspective report is a direct expression of the *O*'s experience. It is 'symptomatic' of some phenomenal datum in the *O*'s field.

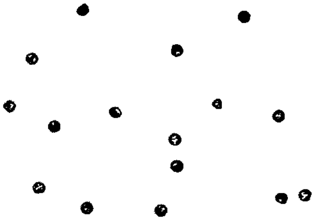

FIGURE I

I admit the justice of the objection and so proceed to a second analogy Let the eye move at random over the group of dots in Fig. I. Into what patterns and groupings do they fall? Some are grouped by threes; others by fours; others in more complex patterns. With proper instruction one may see large and small groupings, regular and irregular, and perhaps no groupings at all. Seemingly an indefinite number of shapes and patterns may be perceived.

So in affective psychology the so-called facts depend upon highly variable and little understood conditions. Trouble begins when we attempt to say which observation is true and which is false. The determination of the truth or falsity of an observation appears to be about as futile as to argue whether the dots are truly grouped in threes or fours, in this way or in that way.

Some of the conditions which determine the report in affective psychology are (a) the *O*'s education in psychology which includes the kind and amount of his information, (b) the *O*'s bias determined in part by his theoretical reflections, (c) the *O*'s understanding of words and his habits of speech, and (d) the suggestions which happen to reach him from various sources.

These factors, except possibly suggestion, may be grouped together under the term 'training.' Introspective investigators of feeling usually recognize the importance of training. Training is admittedly important; but perhaps it is too important. It is more important than is generally realized. It is so important that through training one can apparently demonstrate in affective psychology almost anything one likes. I will guarantee to train a group of unsophisticated *O*s to report uniformly that *P* and *U* are non-sensory affective processes, or meanings, or pressures, or bodily reactions, or something else. I will guarantee to train a group of naïve *O*s to localize *P* and *U* or to declare that these processes are non-localizable, to report mixed feelings or to declare that mixed feelings do not exist, to describe qualitative diversity within the qualities of *P* and *U* or to report lack of qualitative diversity, and so on indefinitely. The only requirement I make is that my *O*s be young and plastic. Occasionally I might have to discard the 'bad' reports of a recalcitrant *O*.

It is my belief that mere practice within a group of *O*s is not sufficient to guarantee a uniform result in affective psychology. Uniformity can be gained in a group through controlling the education of the group; but through the control of training one can demonstrate, with apparently equal validity, results which are logically incompatible.

The futility of the present situation is rather discomforting, but let us ask in a positive way: "What can be done about it?"

(1) I repeat that the attempt to determine what is true and what is false, along present lines, is futile unless we are ready to accept simultaneously a mass of logically opposed, incompatible, mutually contradictory propositions as being equally true. Unless, to take an analogy, we are ready to argue that the dots are truly grouped in this way and not in that way; unless we are willing to insist that this particular trained dog has the only virtuous, significant, and genuine type of performance. Almost any doctrine can find experimental support.

(2) The number of observational tricks which an *O* in affective psychology can be taught is seemingly indefinite. Certain tricks, however, are incompatible. A dog may be trained to bark or to lie down quietly at the spoken word; but he cannot be trained to bark and to keep quiet at the same time. So an *O* may be trained to report *P* and *U* as bare, non-clear process, or he can learn the pressure trick, or the meaning trick—but he cannot perform logically opposed tricks simultaneously. How many observational tricks are possible? The number is seemingly indefinite.

(3) The present situation can be cleared up only by the frank recognition of the complete relativity of so-called fact to the logical system which is the product of training. It is important to find common ground which is independent of training. In

the search for data which are independent of training I see hope; not in further training of trick *O*s.

A worth-while question is not whether this or that trick observation is true or false, but rather, "How did the creature get that way?" This is a genetic problem and it demands that we begin at the beginning.

Now there are data in affective psychology which are relatively independent of training and bias. Such types of performance as vomiting, the movements associated with bodily injury, the restless movements accompanying starvation and internal diseases, groaning, crying, etc., may be considered collectively as examples of disliking which are relatively free from the theoretical opinions of the afflicted person. On the other hand, the sight and smell of well-cooked food and the eating of the same when hungry, the specific sex responses, and probably other basic types of performance may be taken collectively as examples of liking which are relatively independent of psychological training. In the study of the basic affective reactions which are relatively independent of training I can see a way out of the present difficulties.

At present we are attempting to build a roof before we have a house.

V. The Framework of Psychology

Datum and Logic. The starting point for the natural sciences is a simple and direct acceptance of phenomena just as they are given to some observer. The individual phenomenon is observed by someone at a particular time and place. As examples of phenomenal data consider: a stone, a star, a ghost image, my living body, the color red. Reflection upon phenomena leads to the construction of logical systems, for example: the hypothesis of the electron, the theory of relativity, the idea of God.

The relation between datum and logic is exceedingly close. The logical construction of an observer in part determines the nature of the datum, for example: whether it be a ghost-to-be-feared or simply an interesting after-image. Many of our beliefs have never been logically formulated; they rest, perhaps, upon an irrational foundation; they are not the cold, clear, logical outcome of reflection upon data.

In the natural sciences the aim is to build up logical constructions, in words or in other symbols, which rest upon observed data. *Observation* is the key word to the sciences. Along with observation the man of science reflects and reflection leads to the construction of general propositions, the formulation of hypotheses and theories, the stating of problems.

The relation between datum and logic is circular, as shown in Fig. II. A logical system is built upon observed data. Reflection upon the data, or the discovery of new data, shows the weakness of a system and this leads to further observation. The logical structures of any field of learning are continually being modified to keep them adequate to the facts of observation. The facts in part make the logical construction what it is. The logical construction is a condition for the observation of new facts. The truth of a logical system is relative, pragmatic.

Objectivity. Normally most phenomenal data are taken to be objects which exist externally to the observer, which are spread round about, and which are relatively independent of the observer. In some cases—hallucinatory voices, visions, dream objects, after-images, entoptic phenomena, and the like—this objective character is commonly taken to be false or inadequate. Primitive man may take his dream experience objectively and later find that to do so leads only to confusion. Again, certain phases of the phenomenal order—feelings and emotions, ideas, desires, etc.—appear to be relatively lacking in objectivity; they are usually referred to the subject of experience, or regarded as mere experiences.

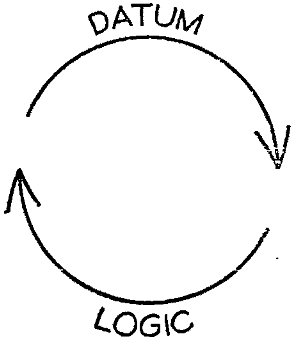

FIGURE II

These considerations may be generalized by saying that objectivity is a characteristic of some phenomena but not of all, and that the character of objectivity is sometimes taken to be valid and sometimes invalid.

Where Objectivity is Valid. In such studies as astronomy, physics, chemistry and most of zoölogy objectivity is tacitly accepted as a valid characteristic of phenomena. The objects observed are taken as if they existed externally to the observer and independently of him. Furthermore, these objects are frequently believed to exist in a real order which is independent of the given, or phenomenal, order of existence.[40] Practically we all believe that the inference of a real order is valid.

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[40] When once we realize that the real object is an inference and seek to understand it through logical analysis we are left with a bare object of perception and a point of view taken up towards the object. We are reduced to datum and logical system.

In contemporary American psychology there is an attempt to limit psychology to the study of behavior regarded physically. It should be realized that in accepting the logical framework of the physical sciences one has limited psychology to the sphere within which objectivity is commonly taken to be adequate. This limitation is methodologically sound. The study of animal movement is undoubtedly a genuine and important part of zoölogy. The physical behaviorist, however, has unduly limited the scope of psychology and has ignored the mass of data which historically have been regarded as constituting the subject-matter of psychology.

Phenomenal Relativity. All phenomena, taken as existences, are found to be relative to one object—my body and more particularly my neural structures. By 'my' body I mean in general the body of any observing, experiencing, human individual. The relativity is such that bodily changes in me modify or remove the phenomena. The age-old experiment of opening and closing the eyes is instructive;[41] the visual world with its objects appears and disappears at the wink of an eye. The eye is an all-important factor in the existence of visual phenomena. If I change the position of my eye, the entire structure of the apparent physical world shifts.

During dreamless sleep, anaesthesia, injury to the nervous system, all phenomena—so far as I am aware—cease. Here one finds phenomenal relativity of the all-or-none variety.

Conscious and Unconscious Conditions. I assume that sometimes I am unconscious. I never experience the unconscious condition for to experience it would be to be conscious. The unconscious condition is thus not datum, but inference.

Whenever there is present some phenomenon, some datum to be observed, studied, talked about, I assume that I am conscious. My criterion for the conscious condition is thus the presence of some phenomenal datum that may be observed. This is a psychological rather than a physiological criterion, by which I mean that the great bulk of phenomena are not given as physiological processes. The bodily processes themselves constitute a small part of the total phenomenal data.

It may be said that awareness itself is ultimate and that no criterion is needed. One may simply assert 'I am now conscious' and let matters rest there without further evidence for the statement.

Also, in the sphere where objectivity is valid, the conscious condition may be regarded as the total conditions found to be essential to the existence of a phenomenal field. For example,

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[41] Since the real world is generally believed to be substantial, it is very easy to regard the phenomenal world as itself a substance; but the concept of mass, for example, is not applicable to the appearance of objects. If an individual were weighed on a sensitive scale with eyes alternatively open and closed, no difference in weight—with the refinement of technique that is possible today—would be expected. Even a behaviorist, observing a white rat running a maze, would weigh the same when his eyes were closed and the phenomenal rat gone.

I may lose a limb or an appendix without permanently losing consciousness; but, so far as I know, the destruction of the nervous system is associated with the unconscious condition. Hence the immediate conditions for the existence of all phenomenal data reside in the nervous structures, although the phenomenal structures themselves are also dependent upon extra-neural conditions. The differences between dreamless sleep and normal waking life are matters for experimental investigation rather than for speculation.

Assumption of Phenomenal Fields. Where communication is possible, as among men, it is assumed that persons other than one's self are conscious. How about animals?

I walk down the street and watch a dog on the porch across the street. As I walk the dog's head follows my movements whether I walk fast or slowly, forward or backward. I assume that the dog is conscious in the sense that he too has a phenomenal field more or less like my own. He too sees objects before him and moves in relation to those objects. My assumption rests upon the observed movements of his head and neck, and also upon the study of his eyes and nervous system. The dog's visual field, which I assume to exist, is not to be confused with his movements (behavior). The dog's field is thought to be similar to my own; it includes objects seen and heard as well as the experiences of bodily movement. The dog's field, which I assume, is not to be confused with my field. I may believe that one and the same real object determines both fields, but there are always two perspectives—the dog's and mine—and they cannot easily be made to coincide.

Whether the assumption that the dog is conscious—in the same sense that I am conscious—is of value in interpreting the dog's behavior, and how the assumption may be utilized in the study of behavior, are problems for the psychologist. I am content to point out now that the assumption of a field is distinctly a psychological assumption, by which I mean that the consistent physical behaviorist does not make the assumption and cannot make it without departing from his physical postulates. The assumption of the field, however, is not made in violation of any physical postulate, so far as I can see; but it is made over and above the physical postulates, *i.e.* in addition to them. If the psychological facts demanded an assumption wholly contradictory to some physical postulate, I should not hesitate a minute in making that assumption.

The Principle of Full Adequacy. In common with other sciences psychology studies the inter-relations of phenomena. The student of behavior studies the movements of the white rat in relation to the structure of the maze and to other controlled conditions. The student of sensory datum studies qualitative and other characteristics of experience in relation to processes in the receptor organs and in the nervous system.

Along with the study of the inter-relations of certain phenomena there is frequently the need of making a guess, *i.e.*

supplying an hypothesis. In constructing an hypothesis the psychologist looks to the living body and asks: What must be assumed about the living organism to account for the phenomena now in question? What is fully adequate?

As a matter of method the psychologist may assume, perhaps, a real mind[42] as the basis of mental processes. Whether the assumption of a real mind is necessary or not remains to be seen. Ultimately the characteristics of the real mind may be found to be identical with the characteristics of the nervous system; so that two postulational systems—a mental and a physiological—may not be necessary.

As a practical faith the psychologist may assume that in the real world there are conditions *fully adequate* to account for every fact and phase of the phenomenal order. What must be assumed depends upon the facts, and the road to the facts leads to the laboratory.

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[42] Some of the characteristics of the real mind as it is popularly known are these: experiences leave impressions on the mind which are stored unconsciously and which later determine behavior and experience; the mind may be *set* as when one says 'I have made up my mind;' there are sometimes conflicts in the mind, especially between two possible courses of behavior; a person may be of two minds or determinations; a person may lose his mind, in which case his mind may disintegrate; the mind is involved in conscious thinking, remembering, desiring, liking and in other functions of the organism; the mind is free and capable of self-determination; the mind is dynamic; the mind travels from place to place with the body; the mind wanders a thousand miles from the body.

In so far as we leave out of account the facts of the field of consciousness the remaining characteristics of the popular 'mind' are known to be characteristics of the nervous system. The nervous system is modified through reaction; it retains; it may be set; it travels with the rest of the body, etc.

FIGURE AND GROUND IN THE VISUAL PERCEPTION OF FORM[*]

By Ernest Glen Wever, University of California

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New points of view in psychology have arisen most frequently in the field of perception. The development of *Aktpsychologie*, of the school of *Gestaltqualität*, and, most recently, of the psychology of *Gestalt*, has in each case been due to a dissatisfaction with the traditional methods of dealing with the problem of perception. In this problem the most insistent phase, and one which is crucial to a solution (as is shown by its prominence in the theory of the last two movements just mentioned) is that of the visual perception of form.

The greatest contribution to the theory of visual form-perception is the study by Edgar Rubin, in which the fundamental type of form-experience was found to consist of a figure standing upon a ground. The central feature of the perceptual experience is the figure; the marginal or recessive portion is the ground. One member of a whole cannot be considered alone; it can exist only in its reciprocal relation to the other member. This last point, only suggested by Rubin, has more recently been expanded as a primary principle of the *Gestalt* approach to perceptual theory. The figure-ground experience is the fundamental type of perception: every perception is a totality, a *Gestalt*, in which appear two components, the highly structured part, which is the figure, and the vague, undifferentiated, unformed portion, which is the ground or level from which the figure emerges.

In its rise in this theory of perception the *Gestalt* psychology has come into direct conflict with traditional systems. Perception in the older theory, to which Wundt gave authority, was set over against the (sensational, imaginal) elements as more complex, and as analyzable into these constitutive elements. As the critics of the sensationistic position point out, there are obvious difficulties to such a scheme, difficulties so great that at no time has a psychology of perception been attempted within the boundaries as described. Other affairs—attention, association, act, form-quality, creative synthesis, attitude—are called upon in attempts to complete the psychological picture. Some of these affairs constitute the residuum after analysis into sensational elements; others are agencies by which is explained the binding relationships between the elements revealed by the analysis.

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[*] From the Harvard Psychological Laboratory.

There are here, then, two views of mental structure—views widely different, and not to be confused because of the disparate use of the term 'structure.' In the one case mind is thought of as a structure whose properties are a function of the concurrence of certain primary elements; in the other case the situation is reversed, the primary affair is the totality, the *Gestalt*, and the properties of the parts, if indeed parts appear, are functions of the whole. The phenomenon of figure and ground may be used to illustrate this difference: in the traditional theory the experience might be explained as due to the coincidence of certain elementary experiences—a visual sensation (or group of sensations) of a particular quality, extent, duration, and clearness, and another visual sensation (or group of sensations) of somewhat different attributive properties; in the *Gestalt* theory, on the other hand, the experience is a whole, a *Gestalt*, in which one may find the components, figure and ground.

In the first case the method is one of cross-sectional analysis, by which the complex (perception) is resolved into its parts (sensations). In the second case such analysis is denied; the perception is not complex in the sense of being a sum of parts, but is a unitary configuration. The method of phenomenology is substituted for the analytical procedure, and the wholes (*Gestalten*) are taken as they come, in their own right, and described, not in terms of any arbitrary set of elements, but in any manner that seems adequate to the practical situation.

A great deal of this criticism of traditional methodology is well-founded. In fact, the principal objections made by the *Gestalt*-psychologists had been made previously, and were fairly well recognized. A question may be raised, however, about the matter of analysis. If the traditional method of analysis is invalid, is any analysis at all to be permitted? If so, what may its nature be? Apparently, a fundamental sort of analysis is allowed in the recognition of figural and groundal components in visual form-perception. Is this the end of analysis, or by appropriate procedure may one go farther? May there be discovered certain descriptive features which will identify the figure-ground experience? A solution of the problem for this fundamental, simplest type of *Gestalt* should have general pertinence to theory of structure and to analytical method.

That the figure-ground experience does, at some times at least, possess certain descriptive features was established by Rubin. One of these features is the thing-character of the figural component. The figure stands forth in the experience with object-character, the ground is of the nature of substance. The figure is said to be more highly "structured;" the greater the degree of "structuration," the "better" the experience. The nature of this matter of 'structuration' is obscure and will bear investigation. 'Structuration' might be regarded along the lines of traditional theorizing as a binding of the elements, as a process in which an act of the subject brings about the intimate grouping of the parts, or again, and more simply, as a process of accrual by which parts are added to a present core, giving rise to a new group (structure).

This raises in concrete form a very old problem, the problem of meaning. Is the 'structure' meaningful? Is the thing-character of the figure a meaning-designation? In a careful investigation of the figure-ground phenomenon one may hope for some light upon this general problem.

This whole problem is fundamental to the science. The problem of perception involves the basic problem of analytical method, and this in turn a theory of the nature of mind. A theory of mind as unitary structure raises the question of a general, fundamental analysis analogous to the figure-ground analysis already conceded. This is the general setting of the present problem. The specific setting is the question of the visual perception of form, and its keystone position in contemporary theory. The general suggestion of method issues from Rubin's recent work on figure and ground, and the hint of analytical relationships. The specific suggestion of attack is drawn from Rubin's procedure with ambiguous (reversible) stimuli, a procedure which is bolstered up by observations on figure-ground reversibility in previous work on visual form-perception.

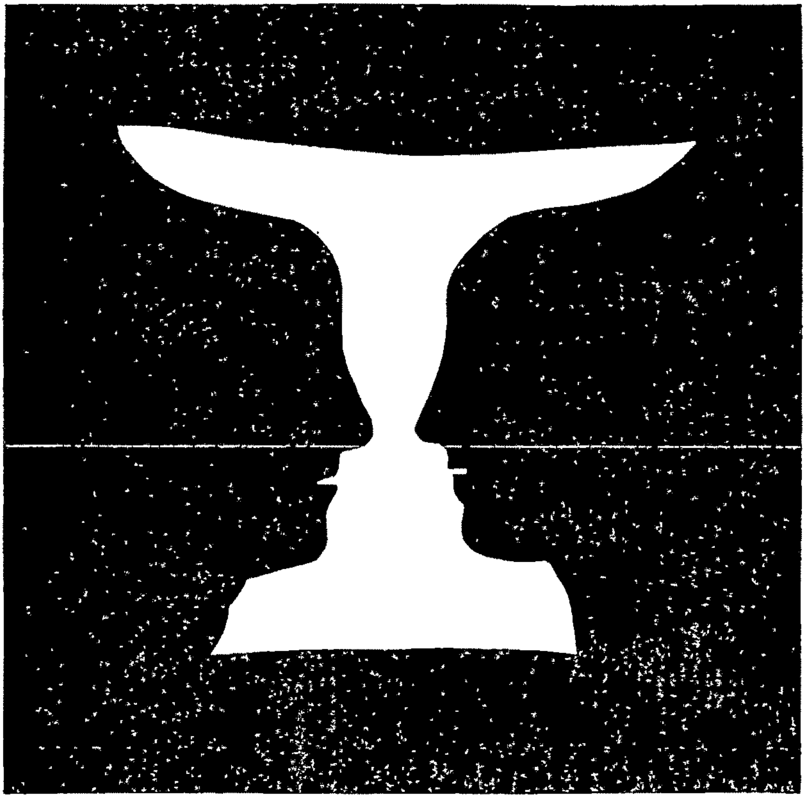

Fig. I. Goblet-figure of Rubin. Reprinted from E. Rubin, *Visuell wahrgenommene Figuren*, 1921.

History of Figure-Ground Phenomena

The distinction between figure and background in visual form-perceptions is one of long standing. It has been recognized, in portraiture for example, that the field or setting in which the figure occurs is an important determinant of the artistic value of the whole. The figure-ground relationship is most forcibly demonstrated in those instances in which the fields are reversible, where what formerly was figure may now be perceived as ground, and conversely.[1] This ambivalency of fields was not unknown in ancient art. It was a frequent motif in Egyptian, Arabian, and Indian design. The familiar goblet-figure of Rubin's (Fig. 1) was taken from an old vase. The picture-puzzle, in which a profile is concealed in the detail, is an ancient device.[2]

Psychological interest in the ambiguity of visual fields is derived in part from the study of optical illusions, particularly illusions of reversible perspective.[3] Helmholtz observed in 1867 that the Zöllner pattern, which usually appears as black stripes on a white ground, may also be perceived as white feathery branches lying on a black ground. This he explained as due to eye-fixation: the change can be effected by fixating firmly the white stripes.[4] Hering (1879) described an illusion in which light shining through a colored window is taken for colored spots.[5] In the course of a discussion of depth and perspective, Mach (1886) mentioned the reversal of visual fields in ornamental design.[6] Jastrow (1901) illustrated a number of reversible illusions, and gave a casual account of the conditions underlying reversal. The changes he ascribed to what "is in the mind's eye" and to familiarity, prepossession, and other subjective factors.[7]

Schumann experimentally attacked the problem of visual form.[8] In his work on grouping he found that when a number of parallel vertical lines are seen in pairs the white stripe between the lines forms a simple whole (the 'figure') and protrudes in consciousness, while the white surface between the groups ('ground') retreats and appears quite different. Not only does the lattice protrude, but the plane of the paper withdraws spatially.[9] The same holds for the checker-board designs. The conditions for the formation of a group ('figure') are spacing of the elements and volition. When the elements are similarly arranged volition operates, and the group changes readily. Spacing is sufficient for the formation of a group without regard to volition.

Schumann regarded visual form as a combination of elements into a unit or group. He argued against the theory of form developed by the school of *Gestaltqualität*, in which a new element, the form-quality, is added to explain the properties of the whole.[10] Schumann's experimental findings were verified by Zigler, who used checker-board patterns, incompleted line drawings, and other material similar to Schumann's.[11] He found that the marks differentiating a group may belong to the intervening spaces, the elements, and tied imagery. The spaces may become broader, or lighter, or darker. The elements may become blacker, clearer, more extended, and standing out in perspective.[12] Zigler agreed with Schumann in discarding the *Gestaltqualität*, and accounted for the perception of form as an attributive pattern established under a particular attitude, and correlated with a definite form-meaning.[13]

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[1] The terminology follows E. Rubin, *Visuell wahrgenommene Figuren*, 1921, vii, 5. If Fig. 1 is perceived as a vase or 'goblet' the white surface is "figure," while the black surface is the "ground" upon which the goblet is perceived; "field" is a neutral term applicable to both surfaces alike. The term 'stimulus' will be employed as a designation of two fields such as Fig. 1, irrespective of the experience which may arise.

[2] Lillien J. Martin's "King Louis XVI" device, taken from an old coin: Uber die Abhängigkeit visueller Vorstellungsbilder vom Denken, *Zsch. f. Psychol.*, 70, 1914-5, 213.

[3] See J. E. W. Wallin, *Optical Illusions of Reversible Perspective*, 1905.

[4] H. von Helmholtz, *Handbuch der physiologischen Optik*, 1867, 566 f.

[5] Hermann's *Handbuch der Physiologie*, 1879, III, 574.

[6] Ernst Mach, *Analyse der Empfindungen*, 1886, 89 f.

[7] J. Jastrow, *Fact and Fable in Psychology*, 1901, 294 f.

[8] F. Schumann, Beitrage zur Analyse der Gesichtswahrnehmungen, I, *Zsch. f. Psychol.*, 23, 1900, 1-32; II, 24, 1900, 1-33; III, 30, 1902, 241-291, 321-339.

[9] *Op. cit.*, I, 6-9.

[10] Cf. the discussion by M. Bentley, The psychology of mental arrangement, this JOURNAL, 13, 1902, 269-293.

[11] M. J. Zigler, An experimental study of visual form, this JOURNAL, 31, 1920, 273-300

[12] There are also changes in the contour lines. See below, p. 205.

The ambiguous character of visual fields forced itself to the notice of Rubin, since it affected grossly the results of an experiment upon the recognition of previously exposed stimuli. He used irregularly shaped green fields on a large black screen, and found that frequently the black instead of the green field was perceived as figure. Rubin's results are briefly summarized:

(1) If an ambiguous stimulus has previously been experienced with a given field as figure, the same field is more likely to be figure on a second presentation. This is the *figural after-effect*.[14]

(2) The recognition of an ambiguous stimulus is dependent upon the experiencing as figure of the same field as in previous presentation.[15]

(3) The figure and the ground are different experiential objects. (a) The figure has form; the ground in a sense has no form. This form seems to proceed from the contour.[16] The contour may so little limit the ground that it appears as extending uninterruptedly behind the figure. (b) The figure has thing-character; the ground is of the nature of substance.[17] (c) The figure has 'surface-color;' the ground is more likely 'film,' less dense, more primitive. Memory-color operates more upon a field when it is experienced as figure than when it is experienced as ground.[18] Irregularities of lighting are less striking when the field is figure; shadows are levelled off.[19] (d) The figure usually is localized in front of the ground.[20] (e) A field when figure is more *eindringlich*. It is named sooner. It is better remembered. Its color is more impressive. Differences in two stimuli are more readily reported when the compared fields are both figures than when both are grounds.[21] The figure is more likely to have connected with it various meanings, feelings, and esthetic values.[22]

Since Rubin's study there have been several investigations of phases of the figure-ground phenomenon. Von Hornbostel, 1922, in an investigation of reversible perspective, characterized the experience of inversion as a change of convex into concave, and concave into convex. The convex has figural properties, the concave the character of ground. It is in general more difficult to invert the convex than the concave.[23] Koffka (1922) found that in retinal rivalry a field plays a different rôle as figure than as ground.[24] Hartmann, in the investigation of fusion, found that flicker disappears for a white field sooner if the field is perceived as ground than if it is perceived as figure.[25] Gelb and Granit (1923) found that the color-limen has a larger value when the field is perceived as figure than when it is perceived as ground.[26]

Rubin's success in isolating the properties of figure and ground was due in no small measure to his use of completely reversible stimuli. An irregular green field within a dark area may be experienced at one time as green figure on black ground, at another as a dark surface with an irregular opening beyond which lies the green background; the change from one phase to the other is attended by a striking change in the object-character of the experience. It is interesting to add, however, that with certain other stimuli, such as the identical-field patterns often encountered in ornamental design, one may observe a complete reversal of fields without such change of object-character.[27] On the other hand, it is possible to observe a change of object-character without concomitant reversal of fields: in the familiar "Brain and Babies" drawing of Gudden's[28] one sees in the first case the convolutions of a human brain, in the second a group of sprawling infants, but in each instance the ground is the white surface of the paper.[29] Similar to this is the experience in illusions of reversible perspective; to take the well-known instance, a stairway viewed from above may suddenly reverse, and become a stairway seen from below. Here again the ground is altered little if at all, but there is a striking change in the perspective of the figure. A third phenomenon of this class is one in which the change is of grouping, illustrated by Schumann's dot-diagrams, which may be observed as horizontal rows or as vertical columns, while in each case the ground is the white surface. The ground here may change slightly, but it does not reverse. Intermediate between these affairs in which the figure may undergo alteration of object-character, perspective, or grouping, and the experiences in which reversal of figure-ground fields may be complete, stands a class of form-experience in which some limited portion of the ground may suddenly come out as figure, while the previous figure more or less recedes. This is best illustrated by the familiar 'picture-puzzles;' a case already referred to[30] is the "King Louis XVI" device. The primary figure is a large vase on a stone wall; then one may see the profiles of King Louis and his family: two outlined by the vase, and one on each side hidden in the branches. At no time does the vase disappear, though its prominence may somewhat diminish. The reversal here is but partial.

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[13] *Op. cit.*, 299 f. [14] Rubin, *op. cit.*, 6-21.

[15] *Ibid.*, 21-29. [16] *Ibid.*, 36 ff. [17] *Ibid.*, 46-51.

[18] David Katz, Die Erscheinungsweisen der Farben, *Zsch. f. Psychol.*, *Erg. Bd.*, 7, 1911, esp. 214 ff.

[19] Rubin, *op. cit.*, 52-59. [20] *Ibid.* [21] *Ibid.*, 67ff, 70 f, 41 f. [22] *Ibid.*, 73-79.

[23] E. M. von Hornbostel, Ueber optische Inversion, *Psychol. Forsch.*, 1, 1921, 154.

[24] K. Koffka, Die Prävalenz der Figur, *ibid.*, 2, 1922, 147 f.

[25] L. Hartmann, Neue Verschmelzungsprobleme, *ibid.*, 3, 1923, 363 f.

[26] Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle, *Zsch. f. Psychol.*, 93, 1923, 83-118; esp. 92-3.

As this discussion shows, not an inconsiderable portion of our visual experiences belongs to the class of equivocal forms, a class to which may conveniently be applied the term "polyfigurations," as used by Werner in a discussion of visual *Gestalten*.[31] The correlative class in Werner's terminology, the "monofigurations," comprizes the larger remainder of our visual world. Monofigurations may be stable, in which case they are little subject to alteration, or instable, in which instance they may undergo limited change. The face of a friend is a stable monofiguration; it is insistent upon being figure and not ground.[32] Familiarity (figural after-effect), centrality of position, small size, and movement are factors that make for stability: the last three are optimally shown in the 'falling star:' it is a small, luminous, rapidly-moving line, seen against the dim, unformed sky; it takes one unawares and lasts but a moment—it quite precludes reversal.

Instable monofigurations are complex visual affairs in which parts may at different times be more or less dominant in the experience, but the change is not of great magnitude. During the examination of a portrait, or a simple tapestry, now one feature, now another, may be prominent, but the general character of the experience remains constant.

It is apparent from this survey that changes of figural and groundal status are not uncommon in phenomena of visual form. The suggestion appears, moreover, that in the figure-ground relationship lies a unifying theory of results derived more or less independently in work on grouping, reversible perspective, attentional or conscious differentiation of the visual field, and the nature and behavior of the object. More than this, as we have seen, the figure-ground concept may be basic to a theory of perception, and therefore to a general psychological system. The work of Rubin has been absorbed into the system of the *Gestalt* school, and much has been made of the distinction of figure and ground. The figure-ground phenomenon is regarded by Koffka[33] as the simplest type of mental *Gestalt*, fundamental to human consciousness.[34] Perception in the theory consists in the emergence of a *Gestalt* from a general level (ground).[35] One may turn, therefore, in view of the pivotal position that the figure-ground phenomenon has assumed in psychological theory, to an inquiry into its precise nature and properties.

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[27] See the pattern illustrated by Mach, *Analyse der Empfindungen*, 1886, 89.

[28] Illustrated by E. B. Titchener, *Textbook of Psychology*, 1910, 278.

[29] Cf. also the J. Jastrow "Duck-Rabbit" figure, *Fact and Fable in Psychology*, 1901, 295.

[30] Note 2, *supra*.

[31] Heintz Werner, Rhythmik, eine mehrwertige Gestaltenverkettung, *Zsch. f. Psychol.*, 82, 1919, 198-218.

[32] Compare in this connection the peculiar resistance to change of size shown by the human face in investigation of the after-effects of seen movement, as with the Mach spiral. Indifferent objects looked at after long fixation of the rotating spiral may change either in size or in distance; the face of a person readily changes in distance, but strongly resists a change in size. This suggests the operation of a figural after-effect. It would be significant to investigate here the comparative rôles of figural and groundal fields.

EXPERIMENTAL ANALYSIS

Preliminary investigation showed that the nature of the figure-ground experience varies according to the time of exposure of a stimulus. This led to an analysis of the figure-ground phenomenon by variation of the time of presentation and, as early became necessary, control of the criterion of report.

Rubin had used exposures of about 4 sec., from the results of which, along with observations of his own, he derived the exposition of the properties of the figure-ground phenomenon, as summarized above. It appeared obvious that shorter times would suffice for the experience of figure and ground.

Materials. In Rubin's experiments, an irregular hole occupying an area of about 1.5 x 1.5 cm. was cut in a card, and the card was placed between a green light-source and a screen, so that the observer saw reflected from (or transmitted through) the screen a meaningless green field within a large black field. The stimuli used here were adapted from Rubin. On white cards were pasted irregularly-formed fields cut from a 1.5 x 1.5 cm. square of black paper; these 'nonsense-figures' were of the order of complexity of those illustrated by Rubin.[36] As stimuli 1060 different forms were used.[37] When in the tachistoscope the black field appeared centrally in a white area 5 cm. wide and 7 cm. high, all framed by the whitish object-holders of the instrument; beyond the object-holders the field was dark.

Apparatus. The exposure-apparatus consisted of a modification of the Dodge tachistoscope. One channel controlled the presentation, another the fixation field. The apparent position of every object was 32 cm. directly in front of the observer. The fixation-field consisted of black cross-lines on a white card. Before exposure the fixation-field was always illuminated; it disappeared 125σ before exposure, and reappeared $(96\text{-}D_t)\,\sigma$ after the exposure (D_t = exposure-time).

The control of exposure-time was by means of a heavy screen which fell under the force of gravity as freely as the slight friction of its guides would permit. Its action was calibrated by means of a chronoscope; its variation was shown to be negligible. A slit in the screen, adjustable in width by means of a vernier, fell from a constant height past a 1 cm. window so that during the period of apposition light from the light-box was admitted to the appropriate channel, illuminating the stimulus. When the width of the slit was less than that of the window, as it was for values of D_t less than

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[33] Koffka, Perception, an introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 553-570; *The Growth of the Mind*, Eng. trans., 1924, 131 ff.

[34] W. Köhler, Gestaltprobleme und Anfänge einer Gestalttheorie, *Jahresbericht über d. ges. Physiol.*, 1922, 523 ff; *Die physischen Gestalten*, 1920, 183 ff.

[35] Koffka, *locc. citt.*

[36] Rubin, *op. cit.*, Ab. 1. [37] Stimuli were used over in succeeding series.

12σ, the illumination was proportionately reduced. The sources of illumination were 75-watt, 110-volt, frosted incandescent lamps, in separate compartments of a light-proof housing.

All the experimentation was conducted in a double dark-room. One compartment contained the apparatus. The hood of the instrument was in the second compartment, where the *O* sat. An overhead light was kept on in *O*'s compartment; this was not visible during observation of a stimulus, but served between presentations to keep the subject in a mild state of photopia. Since the general illumination of the room was approximately that which obtained during presentation, adaptive variations were obviated.

Observers. The observers were Mr. K. E. Zener (*Z*), Assistant in Psychology; Mr. J. G. Beebe-Center (*B*), Tutor in Psychology; and Dr. F. A. Pattie (*P*), National Research Fellow in Psychology. All were experienced observers.

Since the procedure proved unusually productive from the standpoint of descriptive material, the quantitative study will be accompanied by a consideration of certain of the figure-ground phenomena on the basis of the protocols. Among the quantitative results are included (1) Simplest Figure-Ground experience, (2) Minimal Contour, (3) Continuous Contour, (4) Minimal Localization, (5) Definite Localization, (6) Halo-experience, (7) Good Figure-Ground experience, and finally (8) 'Perfect' Figure-Ground experience.[38]

For each of the first seven topics the general procedure was the same. After preliminary exploration,[39] which consisted in taking descriptive reports of experiences obtained through various times of exposure, the *O* was given a particular criterion of report, and by the method of constant stimuli a set of percentages was obtained for a chosen range of exposure-times. These percentages represented the frequencies with which the designated experience occurred under the given conditions, and from these data a temporal RL was computed by Urban's method.

In each of the tables (I-VII) the first column (D_s) gives the time-values in slit-widths as they were originally recorded: this value is translated into sigma (0.001 sec.) in column 2 (D_t). In the third, fourth, and fifth columns are given the percentages of positive reports for each *O* for every exposure-time. At the foot of the table are, first, the computed limen (RL_s) in slit-widths with the precision-value (h_s) in the same units, and below that the same limen translated into sigma (RL_t).

Series 1. Simplest Figure-Ground Experience. It was very early evident that a reduction of the time of presentation occasioned the dropping-out of some of the features of the figure-ground phenomenon known to be present in long exposures. The question arose as to how far this limitation might proceed without elimination of the essential features of the figure-ground experience. In order to answer this question the *O*s were given rather free instructions:

Instructions. "Your criterion is the simplest experience that you would designate as 'figure-ground experience.' Following the presentation report 'Yes' or 'No' accordingly as the experience was present or absent."

The procedure, as has been explained, was in the manner of a determination of a stimulus threshold, with frequent demands for descriptive reports. In their descriptions the *O*s show striking agreement as to the nature of the simplest figure-ground experience.

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[38] The actual order of experimentation was not the same for any two *O*s. It was for *Z*, Series 3, 2, 1, 6, 7, 4, 5, 9, 8; for *B*, Series 3, 2, 1, 4, 5, 6, 7, 9, 8; for *P*, Series 3, 2, 1, 4, 7, 5, 9, 8, 6.

[39] The exploration- and practice-series occupied usually the first 15-20 min. of the period; during this time descriptive reports were liberally taken.

(1) The first essential characteristic of the simplest figure-ground experience appears to be a certain degree of heterogeneity in the visual field: the field is so differentiated that two separate, intrinsically homogeneous regions are perceived.

B: "I call the figure-ground experience one in which a part of the field is more homogeneous than the entire field. The relative homogeneity of a part is greater within itself than the homogeneity of the entire field taken in general."

Z: "The essential characteristic of this criterion is the existence of something within the field of vision which can be pointed to as some kind of unit differing from the rest of the field. A mere difference in tint is not sufficient. There must be the appearance of some sort of shape within the figure."

P: "When it is homogeneous I say that there is no figure-ground there. A sort of smudge I don't call figure-ground."

(2) With this character of heterogeneity is always correlated a difference in brightness between the two fields. This was reported by all *O*s.

(3) Shape, in a technical sense as a roughly restricted, designable expanse, was expressly reported by two of the *O*s (*Z* and *P*), and the third *O* found the figure always to have 'spatial position' of a characteristic sort. *Z* required more or less 'form' for the shape. *P* appeared to demand somewhat more 'form;' his shapes apparently were all slightly 'formed.'[40]

(4) All *O*s required a bounding mechanism of a very primitive sort. *P* called this 'contour,' but the descriptions showed his requirement to be very limited: this contour was no sharp gradation, as a rule, and might appear only in a limited portion of the experience. *B* and *Z* reserved the term 'contour' for a sharp gradation;[41] they did not here require contour as they defined it. The evidence indicated that *P* demanded for simplest figure-ground experience a more nearly abrupt transition than did the other *O*s.

The nature of a typical experience may be shown by one of the more complete descriptions:

Z: "Figure vaguely in the shape of a rectangle, though quite irregular. Unequal tint. The greatest blackness was toward the bottom of the figure. Contour extends to a certain extent along the bottom. The top had shape without contour."

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[40] There is here some difficulty in terminology. It appeared that there is needed one term to designate the primitive spatial distribution of a (figural) field, and another to apply to the precise object-like character of a field. Following the tendencies of the *O*s, in the first capacity will be used the term 'shape,' in the second, 'form.' 'Form' corresponds to the German '*Gestalt.*' 'Shape' corresponds more to what others have termed 'expanse' or 'extent,' but is a somewhat more definite term than these. Z remarked: "A *shape* is a patchy affair, a place in the visual field different (here, in intensity) from the rest of the field, with a minimum of spatial characters. To me 'form' implies a rather definite contour."

[41] In the following the term 'contour,' unmodified, will be used as designating the sharp, definite affair. See below, p. 205 f.

Figure-ground was reported absent when the essential features as mentioned above were not present:

Z: "A simpler sort of experience is a sort of black that fades out;" "a borderline case; suggestion of a certain focus of greatest black; just diffused out without constituting any figure at all. The field was not uniform, but there was no definite shape to the locus."

B: "Field was darker and was not homogeneous, but there was no part of the field that was more homogeneous than the rest."

P: "No definite contour; sort of a smudge;" "homogeneous all over."

TABLE I*
Simplest Figure-Ground

D_s	D_t	Percentages *P*	*B*	*Z*
0.4	8.7		.04	
0.5	9.3	.00	.24	.04
0.6	9.9	.08	.56	.24
0.7	10.5	.56	.96	.60
0.8	11.1	.85		.88
RL_s		0.698	0.561	0.675
h_s		8.437	17.062	6.827
RL_t		10.52	9.68	10.37

TABLE II
Minimal Contour

D_s	D_t	Percentages *P*	*B*	*Z*
0.6	9.9	.04	.08	.00
0.7	10.5	.12	.12	.04
0.8	11.1	.68	.36	.16
0.9	11.7	.88	.80	.64
1.0	12.3	.88	.96	.96
RL_s		0.771	0.815	0.869
h_s		4.910	5.883	8.508
RL_t		10.95	11.22	11.55

TABLE III
Continuous Contour

D_s	D_t	Percentages *P*	*B*	*Z*
1.0	12.3	.24		
1.1	12.9	.24	.08	
1.2	13.5	.68	.52	.08
1.3	14.1	.76	.68	.48
1.4	14.7	.88	.92	.52
1.5	15.3			.76
1.6	15.9			.80
RL_s		1.157	1.229	1.381
h_s		3.118	5.937	3.350
RL_t		13.29	13.71	14.61

TABLE IV
Minimal Localization

D_s	D_t	Percentages *P*	*B*	*Z*
0.5	9.3		.08	
0.6	9.9	.18	.30	
0.7	10.5	.54	.64	
0.8	11.1	.72	.88	
0.9	11.7	.94		
1.2	13.5			.20
1.4	14.7			.36
1.6	15.9			.54
1.8	17.1			.50
2.0	18.3			.68
RL_s		0.707	0.653	1.683
h_s		5.457	6.100	1.030
RL_t		10.57	10.24	16.41

The quantitative results are brought together in Table I. The values of the limens reflect the agreement of the *O*s in regard to the nature of the figure-ground experience at its most elementary level. The conclusion from these results is that under the given conditions of illumination, distance, and stimulus, a figure-ground experience of the most primitive nature arises from an exposure of the order of $10\ \sigma$. The h-values indicate a relatively rapid rate of change of the phenomenon in this region. The descriptive features of the typical experience are two heterogeneous fields, differing in brightness, one of which fields (the figure) has 'shape,' and a 'contour,' or a bounding mechanism, of a primitive sort.

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*Each RL is calculated from 25 series. The same is true of Tables II and III. The remaining tables (IV-VII), where the values of h are not so large, involve 50 series.

Series 2. Minimal Contour. Since, as the first series showed, a definite contour is not essential to the simplest figure-ground experience, the question naturally follows as to what order of exposure-time will give such contour in at least a portion of the experience. The *O*s were instructed:

Instructions. "Your criterion is minimal contour: any contour at all is to elicit the judgment 'Yes,' entire absence of contour the judgment 'No.' "

Minimal contour consists in a sharp gradation, a definitely marked boundary, between figural and groundal fields; the contour is not necessarily continuous. This is shown in the reports of the *O*s:

B: "An experience in the upper part of the field of a sharp gradation that could be called a contour."

Z: "The only contour existed on the left-hand side. The boundary here was quite definite. The rest of the figure blurred out, in a sort of smudgy way, in the field."

P: "A sort of irregular square on a background that was not much lighter than the square itself. I think the place that I caught the boundary was mainly on the upper side. It was not as definite a contour as I had experienced where there was more difference between figure and ground."

The figure-ground experience at this stage has other characteristic features. It consists typically, under these conditions of exposure, of a dark grey or very dark grey field of rather indefinite shape, upon a grey or less dark grey background; the intensity is relatively low.

The results of the psychophysical procedure are shown in Table II. There is close agreement among the *O*s, showing that this criterion of minimal contour is a definite one. For each *O* the limen for figure-ground experience with minimal contour is greater than the limen for simplest figure-ground experience, showing, in accordance with the descriptive data of Series 1, that in the statistical average the simplest figure-ground experience occurs before definite contour has time to develop.

Series 3. Continuous Contour. In the longer exposures of the last series the contour was frequently described as completely enclosing the figure; in this series the criterion of report in the determination of a limen was that of figure-ground experience with such continuous contour. The *O*s were instructed to report 'Yes' or 'No' accordingly as they experienced figure-ground with complete, continuous contour. The contour here, except for the added requirement of extending all around the figure, is of the same character as that of the last series. The experience as a whole, however, is somewhat altered. In general, in the figure-ground experience with complete contour, the perception is that of a dark grey or black field, of definite shape and greater or less homogeneity, which rests upon a light grey background, and is separated from that background by a contour that is definite and continuous.

As is shown in Table III, the RL's for all *O*s are very close together, and in every case are somewhat longer than those for minimal contour. The variations between *O*s are in the same order in the two series, a fact which correlates well with the general agreement of the *O*s in their descriptions of the experiences. The figure-ground experience with complete, continuous contour has been found to arise, under the conditions, with an exposure of 13-14 σ.

One may now turn to a descriptive analysis of the contour experience. Schumann, in his work on grouping,[42] found that sharper boundaries assist in differentiating a group from its background. This peculiar character of the bounding lines he attributed to better accommodation. Zigler[43] confirmed Schumann in finding group-formations to be characterized by alterations in the boundary lines, and found further that the contour-lines are blacker, clearer, more extended or broader, and more sharply defined at the edges. Such was the extent of knowledge of contours before Rubin published his study.

Rubin[44] found: (1) The figure seems to be due to the operation of the contour. The contour may be regarded as giving form to the figure-surface. (2) This contour may sometimes have so little to do with the ground that the ground extends uninterruptedly behind. (3) The contour has no breadth. (4) The contour has no color. (5) The contour differs from the surface in that the latter is a unity, but the contour may have several components.

Rubin seems to have made a distinction between contour (*Kontur*) and edge (*Rand*). In scotopia (and sometimes also in photopia) the observer may experience blurred surfaces (*Flächenfiguren*), in which case Rubin speaks of an edge, which, however, preserves close relationships to definite contour.[45] While such a distinction may be of practical convenience, one is forced to point out that no line of demarcation exists between the sharp gradation that Rubin calls "contour" and the more gradual transition that he calls "edge." However, in this study the practical distinction is more or less maintained, for the *O*s in speaking of the less definite affair usually employed such expressions as 'hazy contour,' 'fuzzy contour,' etc.

(1) The term 'contour,' unmodified, is designated as "abrupt gradation" (*P*), "abruptness of transition" (*B*), "definite outline," "continuous break between black and white

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[42] *Zsch. f. Psychol.*, 23, 1900, 23.
[43] See above, p. 197.
[44] Rubin, *op. cit.*, 105-7.
[45] *Idem.*

with sharp distinction" (*Z*). This agrees with Rubin's use of the term as designating an experience which is without breadth and without color.

(2) A contour may have breadth; that is to say, the gradation from black figure to white ground is gradual, extending over an interval.

Z: "The figure was uniform grey, not of surface-character, but like dark woolen cloth, a little bit fuzzy. The edge seemed to have a definite existence, to be a little bit in addition to being the edge of the figure."

B: "The brilliance of the blob was greater than that of the rest. The blob stood out, was three-dimensional, though indefinite. The degree of heterogeneity between blob and background was very little. The gradient was gentle rather than steep. The outline was fuzzy, not homogeneous in brilliance; that is, the brightness between background and blob was not homogeneous either."

(3) The contour may have tridimensional properties.

Z: "A line around the edge of the figure, a thing which was a little bit darker than the rest of the figure, a little bit more solid, firm, with more substantiality. It definitely belongs to the figure, and is not part of the ground in any sense."

B: "Dark blob, continuously contoured. Blob seemed to have object-character, three dimensions. The blob was homogeneous. The contour was very much as if it was not an abrupt stop of an object, but like that of an object that was three-dimensional. Hard to say anything, except that it was a *three-dimensional contour*."

(4) The separation-mechanism may be a very gradual shading of one field into the other. Here the grey quality of the contour is more marked.

P: "Hazy contour. Noticeable brightness-difference between figure and ground. The contour was not sharp: there was not a distinct contrast between white and black. The contour was not definite, like a line, but represented more or less by an area. Grey, intermediate between the dark grey of the figure and the light grey of the outside."

B: "To the left of the vertical line was a dark patch which merged abruptly into the background on the left. Down towards the right lower corner it merged gradually to a darkish grey, a fog."

This affair may or may not be called a contour.

Z: "The figure was a dark grey of rather misty appearance. It seemed to merge to a certain extent into the field, especially on one side, where it sort of streamed off, like a whiff of cloud. The field itself was unevenly dark grey, nearly as dark as the figure on the edges of the field. It was somewhat lighter immediately adjacent to the figure. I object to calling that a contour—it is a fuzzy edge."

(5) The merging may be so gradual that no 'step' is perceived. (If this condition prevails over a relatively large portion of the experience, differentiation of figure and ground is usually absent.)

Z: "A small part of the figure had a contour which was definite. In one spot near the top the thing was very vague and misty—no contour; the figure sort of flowed off into the ground." "It very definitely faded out. A rather square-looking figure. Dark in the middle, grading out to the edge with decreasing character. The whole thing was not solid, or surfaced, or smooth, especially near the edges. . . . I should hesitate to call that a figure; it was more like a 'shape.' "

B: "A little white spot, surrounded by a dark ring, ring merging into a white. At some points there was a definite step, a change in brilliance, at others there wasn't any step. I couldn't say whether the step is white-to-black or black-to-white."

P: "No contour. Card with field, brightest around the edges. Toward the center it got gradually darker. I wouldn't say it's a figure-ground experience. Just a black nucleus which very gradually shaded out. No contour."

This continuum of experiences, from definite contour to a gradual merging of the fields, is roughly correlated with decreasing time of exposure.

Besides differentiation by the grade of transition, there is that by the extent of occurrence of contour in the experience; all stages are shown between part-contour and complete contour. With relatively long times the contour is continuous; this ranges from the most definite sort, in which tint-contrast is uniform and continuously abrupt, through intermediate stages to the 'hazy' or 'fuzzy' type, in which the tint-contrast is low and less uniform.

Incompleteness of contour may appear in several forms. In the course of a sharp or of a 'fuzzy' contour there may occur complete gaps—one, two, or more. In the extreme case there is but a 'corner' of contour in the experience. Frequently both sharp and 'fuzzy' contour are present in an experience, though the contour is not complete. With shorter times the 'fuzzy' or 'foggy' type of contour is present in but a limited portion of the field. In those portions of the field reported as without contour the one field shades off into the other so gradually that there appears no 'step' of such height as to become contour; it is in this region that one finds the lower threshold of figure-ground experience. Just below this threshold the gradation may be sufficient to give a certain degree of heterogeneity in the field of vision, yet the experience is not one of figure and ground. Typically the experience here is one of a dark nucleus standing in the center of the field and grading imperceptibly into the periphery. As one *O* described it, it is "like a smudge of ink—it reaches the maximum in the center, and fades out so that there isn't any place where there is a contour" (*P*). This experience merges into one of utter homogeneity.

That contour is a function of the brightness-difference between the two fields is suggested in the reports of all the *O*s. The lower forms of contour are correlated with a low degree of brightness-difference. In these experiences the entire field is usually of low intensity, and of grey, foggy character. At the lowest stage of the phenomenon one field passes imperceptibly into the other, and no 'step' is experienced between the dark grey of the one area and the lighter grey of the other. When the difference in brightness has reached a certain value, however, a 'step' appears, and the figure is marked off from the ground by a line of separation. Later, when the brightness-difference is greater, the separation is more marked, and the figure takes on unit-character, or thing-character, in higher degree.

Series 4. Minimal Localization. The fact is significant, in view of the work of Rubin on the figure-ground phenomenon, that in the three series just described but casual mention has been made of depth-localization of figural or groundal fields. Contrary to what one might be led to expect from previous work on this subject, a difference of depth-localization of the two fields, though it may at times occur, is not necessary in the primitive stages of figure-ground experience. The appearance of such a difference in localization was investigated in this series. The instructions were:

Instructions. "Your criterion is a difference in localization, as regards depth, of the field which is figure and the field which is ground. When such a differentiation occurs you are to report 'Yes;' in the absence of such differentiation, report 'No.' "

Minimal localization consists typically of a 'standing forth' of the figure from its ground; numerous other conditions, however, satisfy the criterion of a difference in localization, as the reports of the subjects showed:

B: " 'No'-judgments mean either (1) that the figure and ground are seen as one plane or (2) that the figure has no place. 'Yes'-judgments mean that the figure and ground are separate in one of two senses: (1) one back or in front (usually the ground is behind, but not always) or (2) the ground has no localization."

P: For this *O* the figure always, or nearly always, stood out from the ground; "a rather irregular figure, with a good many nicks on the right side. The right side had a good contour, and here it seemed to stand out from the ground." He reported localization as absent when the experience consisted of "a sort of blur, shape of a round object; no definite contour; didn't stand out from the field."

Z: The difference of localization might consist "in not being on the same plane, or . . that the figure is on a definite surface and the ground on a definite surface but not on the same surface. The figure may be on a plane and the ground may not have surface-quality, is . . . indistinct."

The 'standing out' of the figure was by *P* correlated with brightness-difference, by *Z* with *Eindringlichkeit.* The background of the experience in most cases shows peculiar properties of texture. *P* often referred to the background as 'blurred,' *B* and Z called it 'filmy.' *B* emphasized the texture-difference of figure and ground in these experiences:

"Dark patch on a lightish grey background . . ., looked like lighted space, or opaque glass." "With one attitude or another I can get surface or film."

The results (see Table IV) show some differences among the *O*s. For *B* and *P* the limen for minimal localization is slightly greater than 10 σ; for the third *O* (*Z*) it is over 16. This difference may be due in a measure to the fact that *Z*'s experiences were somewhat more definite affairs than those of the other *O*s: for the most part both figure and ground were surfaced, and on different planes. For him, experiences in which the figure was localized in a plane but the ground was not at all localized were infrequent. Since surfaced character of the fields, and particularly of the groundal field, is correlated with longer times of exposure, the larger limen of this *O* is partially accounted for.

The principal conclusion from these data is that a localization-difference of figural and groundal fields arises at a comparatively early stage in the development of the figure-ground phenomenon. The fundamental and striking feature of this difference of localization is the protrusion of the figure in the field of vision. The ground, on the other hand, is recessive; its negativity may be such that it is not at all localized, and in this event it manifests usually an indeterminate, filmy character of texture.

Series 5. Definite Localization. It was suspected that something perhaps of fundamental nature might be found in the difference between that localization-phenomenon in which the figure protrudes from an indeterminate background, and that experience in which the two fields are definitely perceived as standing upon different planes in space. The former is what has just been subsumed under minimal localization; the latter is the more familiar phenomenon of figure-ground localization, as it is described by Rubin, and as it familiarly occurs with objects of our visual world. To test this suspicion, a series was given with the following instructions:

Instructions. "Your criterion is localization as regards depth, interpreted in distinct terms. To elicit the judgment 'Yes' the experience must be of a surfaced figure definitely situated *in front of* its ground. The ground may be either surface or film, but it must be definitely *behind* the figure. 'No'-judgments are to be made when figure and ground are on the same spatial level (as regards depth) or when figure-ground differentiation fails. In case the figure appears *behind* the ground, this is to be especially reported as such."

Definiteness of localization is accompanied by marked changes in the nature of the whole experience. The absolute brightness is greater; the brightness-difference is more marked. The contour is sharper, more distinct, and continuous. When a texture-difference is a part of the experience, it is the figure-surface which has plane-quality, while the ground usually is filmy, insubstantial. Yet here the localization of the ground is not indeterminate; the ground tends to be tridimensional, and may be termed 'hazy,' 'foggy,' 'filmy,' or 'translucent,' but this does not mean that it is unlocalized. In fact, it may be well-localized in a general region of space; there is a difference between a rough localization and no localization at all.

The instructions have ruled out those cases in which localization of the figure without localization of the ground, and localization of the figure back of the ground, are taken as positive reports. This alone would tend to make the limens larger. The requirement of definite character to the localization is the prime feature, however.

The results are to be found in Table V. The disparity among the *O*s is apparent. *P*'s limen is but slightly larger than that for minimal localization. The reports showed his criterion here to differ but slightly from the previous one:

> "Cases of best localization have big differences of brightness between figure and ground. The contour is sharp and distinct all the way around. The figure is homogeneous. The field is continuous behind. The thing seems to be unsteady with respect to motion. In other cases I don't get the idea that the field is continuous. When the contour becomes just a little bit more vague it seems merged."

B's limen was greater; his criterion differed in the point that here the ground, though film, was in a sense localized in space.

> "The dark blob was surrounded by a light grey film which didn't have any surface-qualities. The blob seemed to be at the front edge, or in front, of the film. The film had no particular place except that it extended behind the figure."

The localization-experience varied a good deal for *Z*, but the typical case appeared to be one in which both figure and ground had surface-character, with the figure localized slightly in advance of the ground:

> "Figure slightly in front of the ground. Homogeneous, contoured figure. Ground somewhat surfaced."

The data are by no means conclusive on the question whether this definite sort of localization is fundamentally different from that considered in the previous series. The simplest supposition would be that minimal localization is a primitive and definite localization a more advanced stage in the development of localization. There is no evidence which would controvert such a view. On the other hand, the nature of minimal localization as a protrusion of the figural field, and the suggestion of intimate connection with insistence and *Eindringlichkeit*, point toward a possible relation to attention, while subjective localization, along with more common phenomena of localization, has not ordinarily been regarded as having anything to do with attention. The difficulty here can be removed only by further investigation.

A descriptive analysis on the basis of the protocols will serve to emphasize the variety of localization-experiences and the complexity of the whole problem of localization. Rubin in his discussion of subjective depth-localization of figure and ground mentioned four types of experience: (1) figure and ground are on the same plane, (2) the figure is in front of the ground, (3) occasionally, the figure is behind the ground, and (4) the ground is in general less definitely localized than the figure. A number of additional types of depth-localization present themselves.

As has been suggested above, a difference in localization of the two fields may occur in two principal forms. (a) In the plainest instance both figure and ground are perceived as definitely situated in space, and on different levels of depth. Here the figure is nearly always in front of the ground; sometimes the ground extends uninterruptedly behind the figure, more often it does not do so, but stops at the contour. Occasionally the figure is back of the ground. (b) Mention has already been made of the case in which the figure is perceived as definitely situated in space, but the ground is not localized.

When there is no difference in depth-localization, the experience may be (1) that of a figure and ground definitely situated in space, on the same plane, or (2) one in which there is no definite localization of either figure or ground.

Certain other phenomena are striking enough to warrant mention here. There is frequently a difference in depth-localization between separate portions of one of the two fields. Though usually the figure is on a plane perpendicular to the optical axis, this is not invariably the case. At times the figure is on a tilted plane.

P: "A submerged figure . . ., as though the figure had been placed in liquid, with one corner sticking out."

Rather frequently the figure is convex, especially near the boundary:

Z: "The figure seemed like a solid object rounding off at the edges into the field." "The figure was like some sort of 'pillow'—the edges were on the same plane as the ground, but the center bulged out."

Occasionally the figure is concave:

B: "Curved so that the two edges were nearer to me than the center."

Particularly with longer times of exposure the figure is definitely tridimensional:

B: "Head of a monkey; good—has thick thickness, especially over the head of the monkey."
P: "Like a glob of metal with a handle."

Often the ground is not a plane surface. It may be curved, usually so as to be concave towards the *O*.

Z: "The ground seemed to fall away from the figure slightly around the contour."

Sometimes the ground has volume, and extends all around the figure:

B: "The figure was situated in a grey mist . . both behind and in front, mostly behind;" "in a sort of atmosphere."

Z: "Figure not homogeneous, seemed rectangular in shape; no definite contour. Appearance of a black object seen through a mist. The ground was behind and in front of the whole thing. The whole thing was blurred and smudged; no accurate localization."

Depth-localization of figure and ground is an unusually labile affair. With any given conditions of presentation is manifested a great variety of experiences. Only when taken statistically is the depth-phenomenon shown to be correlated with time of exposure. One cannot establish compulsory conditions for the demonstration of a given type of depth, though one may maintain conditions that favor some desired experience.

TABLE V

Definite Localization

D_s	D_t	Percentages *P*	*B*	*Z*
0.8	11.1	.22		
0.9	11.7	.50		
1.0	12.3	.84		
1.1	12.9	.90		
1.2	13.5	.98		
2.0	18.3		.16	
2.5	21.2		.26	
3.0	24.1		.66	
3.5	26.9		.82	
8.0	51.4			.16
9.0	56.5			.32
10.0	61.6			.42
11.0	66.6			.52
12.0	71.5			.60
RL_s		0.898	2.807	10.901
h_s		5.062	0.973	0.208
RL_t		11.72	22.96	66.06

TABLE VI

Halo-experience

D_s	D_t	Percentages *P*	*B*	*Z*
0.5	9.3			.04
0.6	9.9	.16		.24
0.7	10.5	.36	.12	.48
0.8	11.1	.34	.32	.52
0.9	11.7	.52	.64	
1.0	12.3	.60	.92	
1.1	12.9	.80		
RL_s		0.883	0.849	0.783
h_s		2.263	5.990	3.658
RL_t		11.62	11.42	11.02

TABLE VII

Good Figure-Ground

D_s	D_t	Percentages *P*	*B*	*Z*
0.7	10.5	.08		
0.8	11.1	.20		
0.9	11.7	.54		
1.0	12.3	.68		
1.1	12.9	.96		
1.4	14.7		.38	.20
1.6	15.9		.70	.34
1.8	17.1		.72	.54
2.0	18.3		.84	.64
2.2	19.5			.74
RL_s		0.885	1.478	1.813
h_s		5.106	1.426	1.317
RL_t		11.64	15.19	17.18

The relationship between depth and texture has been touched upon, but the matter needs extensive investigation. The evidence shown here is in support of the generalization that indeterminate localization is correlated with filmy texture, and the more definite localization with greater surface-quality.[46]

Depth has also an important connection with object-character. The thing-character of the figure is very probably related to that field's more precise localization. Later it will be shown that changes of object-character are accompanied by changes in the spatial properties, of the figure.[47]

Series 6. Halo-experience. P had reported spontaneously, in previous series, what he called a 'corona' or 'halo:'

"A black spot, then a white halo around it, and then a dark field or ground." "A sort of corona;" "corona seems attached to the black object in the center. Corona and black object seemed to stand out from the black background. Corona is thrown out from the black object."

B reported a sort of 'contrast,' which he later called an 'aureola' or 'halo:'

"Field is much lighter around the figure—contrast." "A slight aureola;" "a dark, very dark grey patch, roughly square in shape, surrounded by a light grey, quasi-circular field, it itself surrounded by a darker grey field. The transition between black and halo was sharp. The transition between halo and dark grey was gradual."

When Z was asked he immediately reported that he had experienced "something in the center, lighter about that, and darker on the outside."

Instructions. The *O*s were instructed: "Report 'Yes' when you perceive an aureola or corona-effect in the field; 'No' when you do not."

There are perhaps two extreme cases of this experience. (1) What may be called the corona-experience proper begins as a lightening around the black figure, and extends outward in all directions, decreasing in brightness toward the periphery, and shading off into the marginal darker ground. In this form the light grey effect occupies a rather large proportion of the area outside the black figure.

Z: "Outside of the figure the field was light, the lightness fading out not so very gradually—like a logarithmic curve—toward the edges."

P: "Dim corona . . ., this white streak sort of shaded off into the background. Figure and ground had just about the same blackness."

B: "A lighter patch surrounding the dark figure, and between the dark figure and a medium grey background. Extended a good half-centimeter from the figure, and from the figure-side was sharply differentiated, but merged distinctly more gradually into the background."

(2) The halo or aureola may consist of a narrow bright streak just outside the black figure.

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[46] See the experiments of Hering, Hermann's *Handbuch der Physiologie*, V, i, 574 f,; Katz, *Zsch. f. Psychol., Erg. Bd.*, 7, 1911; Rubin, *op. cit.*, 51 ff. and note, 57; M. F. Martin; Film, surface, and bulky colors and their intermediates, this JOURNAL, 33, 1922, 451-480. Rubin did not find the above-suggested correlation to be absolute.

[47] See below, p. 220f.

Z: "A ring or a lighter band around the darkest inner part of the field."

P: "Small streak of white, more or less all the way around."

B: "A slight halo of light grey;" "very small aureola, very concentrated around the figure; practically a contrast-line."

The first form of halo-experience obtains when the time of presentation is brief, and the ground is of low intensity. The second form appears with longer times. Between these two classes all intermediates appear. The instructions in the experimental series included all varieties of the experience. A complete halo was not required: the typical experience was a slight halo in one portion of the field, adjacent to a relatively pronounced contour-line.

The results of the psychophysical procedure are shown in Table VI. The limens for the three *O*s are in fair agreement. *P* showed a good deal of variability, due to uncontrolled changes of attitude; this was sufficient to produce an inversion in the percentages. The other *O*s showed fair regularity. The halo-experience in its liminal form is shown as occurring with a presentation of about 11 σ.

The factors determining halo-experience are in part obscure. Attitude plays a part. The degree of light-adaptation is an important feature. The halo-experience is probably a form of the phenomenon of simultaneous contrast, and the results show liminal conditions for the appearance of the phenomenon. The course of the phenomenon with longer times of exposure is yet undetermined.

Series 7. Good Figure-Ground Experience. Under "Simplest Figure-Ground experience" the most primitive stage of figure and ground has been discussed. It is important to consider more advanced stages of the phenomenon. 'Good figure-ground experience' was rather arbitrarily defined for the purposes of this experiment as an experience simultaneously containing all the features of simplest figure-ground experience in highly developed state.

Instructions. The *O*s were instructed to report the concurrence of marked difference in brightness, distinct definition of figure and ground, well-marked contour all the way around the figure, and localization of the figure in front of the ground. The last-named factor of localization was found not to be essential, and was not insisted upon.

It must be noted that the purpose of the instruction was not to define, but primarily to 'grasp' the phenomenon, and the results are in accordance with this presumption. What here is regarded as 'good figure-ground' is not merely the concurrence of a few factors that seem logically necessary, but a unitary affair in which can be demonstrated various descriptive features.

(1) The primary condition of 'good figure-ground experience' seems to be a relatively large difference in the brightness of the two fields. This was emphasized by all *O*s. *Z* reported at one time:

> "My criterion is reduced primarily to judging on the basis of a certain absolute difference in tint between figure and ground. The figures almost all possess continuous contour and difference between figure and ground. . . Difference in tint is a function largely of the uniform blackness of the figure and the whitishness of the ground, probably predominantly of the latter. There seems to be more difference, more variation, in the tint of the ground than in the tint of the figure. There seems to be a difference between neutral greyishness and a white."

(2) Another essential condition is continuity and well-marked character of contour. For *P* this was not emphasized as much as brightness-difference. The contour required was not as sharp a contour as for the others.

(3) Heterogeneity, or distinctness of the two fields, follows directly upon the brightness-difference between the surfaces.

(4) The intrinsic homogeneity of figural and groundal fields was emphasized. *B* remarked:

> "I should call a 'good figure-ground experience' one in which a certain portion of the field was homogeneous and abruptly heterogeneous. I would say that the 'goodness' of a figure-ground experience is a function of the homogeneity of the figure and the heterogeneity between it and the ground."

(5) A rather general factor is definite ('formed') shape; the figure has a distinctly perceived shape that can be named:

> *B:* "Shaped like a pear laid on its side."

(6) The figure protrudes, stands out from the background with a certain insistent character. This is the property already referred to under the section on minimal localization. Perhaps to be distinguished from this is (7) a definite difference of depth-localization of figure and ground, which in 'good figure-ground experience' may be present, but is not essential.

> *Z:* "No definite difference in localization, but didn't seem exactly in the same plane."
> *B:* "This space thing doesn't come in."
> *P* did not mention localization aside from the 'standing out' property discussed above.

The limens (see Table VII) show fair consistency. *P* shows a difference, in that his limen is smaller than that for continuous contour; this is due to the fact that here the continuous contour required was not of the same definite character as before—the contour was continuous, but might be of a 'fuzzy,' 'hazy' kind, not a sharp gradation. All the limens are below the values for 'definite localization,' reflecting the fact that definite localization—contrary to the anticipation of the original instructions—is not a necessary property of 'good figure-ground.'

The properties of 'good figure-ground experience' are marked brightness-difference of the fields, definite, or fairly definite, continuous contour, heterogeneity of figural and groundal surfaces intrinsic homogeneity of the separate fields, 'formed' shape, protrusion of the figural field, and, at times, separation of the two fields in depth.

'Perfect' Figure-Ground Experience. Out of the study of 'good figure-ground' arose the question whether there could be isolated, by the procedure previously employed, conditions for the production of an experience that could be regarded as *'perfect'* figure-ground experience—the phenomenon at its highest stage. 'Good figure-ground,' as has been demonstrated, may or may not show a definite difference of localization between the two fields; it represents, moreover, a rather arbitrary point in the curve of development of the phenomenon. The problem here was the determination of those conditions of presentation-time through which an experience might be obtained containing in their state of highest development the various features which have been found to enter into the figure-ground phenomenon, among which are primary heterogeneity, brightness-difference, contour, shape, localization, and intrinsic homogeneity of the fields.

Series 8.

Apparatus. The apparatus was the same as before, except that time was controlled with a pendulum instead of the screen. The pendulum tripped metal contacts which were adjustable along the graduated arc of its excursion; the contacts turned on and off lamps so as to illuminate the presentation-fields at the desired times. The lamps used were special high-speed, hydrogen-filled, 150-watt, 110-volt, tungsten lamps. The fixation-field was illuminated except for an interval from just before until just after the presentation-period. The intensities of presentation-field and fixation-field were equated by means of a diaphragm. Thirty-two exposure-times were used, from .091 to .445 sec., by steps of about .01 sec.

Instructions. The *O*s were given the following instructions: "Give absolute judgments of your experiences on the following basis: 'Perfect'—what you would regard as *perfect* figure-ground experience. 'Very good'—not quite perfect. 'Good'—about as much less good than 'very good' as 'very good' is less than 'perfect.' " Report of failure was permitted.

In order to obtain a crude indication of 'perfect' figure-ground, and of the rate of change of the phenomenon with increase of exposure-time, 25 judgments were taken, under the above instructions, for each of the 32 times.

The figure-ground experiences under these conditions of exposure showed no essential differences from the ones previously obtained. The fundamental properties, as found in simplest and in good figure-ground experience, played the same rôles as before. It is true, their character showed some change, for the most part in the direction of greater definition and intensity, but there appeared no new significant features which would serve to distinguish a figure-ground experience that could be regarded as 'perfect.'

The *O*s agreed that the instructions could not be carried out in an absolute sense in the report of experiences of 'perfect' figure-ground, because any definition of such 'perfect' experience is not absolute, but relative. That is to say, the designation of 'perfect,' in every case, is but an indication of the comparatively high position which a given experience is said to occupy in a particular series.

In this series, with the range of exposure-times as stated, reports of 'perfect' figure-ground experience (in the relative sense) occur with increasing frequency as the times of exposure increase. Reports of 'good' figure-ground experience occur only for the shorter times. Reports of 'very good' occupy the intermediate range. The conclusion is that as time of presentation is lengthened, the 'goodness' of the experience is increased. This increase, however, proceeds at a constantly diminishing rate, being rapid at first, falling off gradually, and approaching zero near the end of the temporal series. The increase of 'goodness' is shown essentially in progressive alterations in the fundamental features of the experience.

The figure-ground experience here called 'perfect' is one of a homogeneous, very black figure, with definite, sharp, and continuous contour, standing forward from a ground which is a homogeneous white. There is a decided brightness-difference between the two fields. In this much there was entire agreement among the *O*s. Here, however, divergence began, a fact which establishes further the arbitrary nature of any designation of 'perfect' character to a figure-ground experience. *B* and *Z* emphasized a characteristic texture. For *Z* the black figure had "surface-character," while the ground was "thick film." *B* described the figure as "alive," "lively," "like shiny paper," the ground as "milky," "pearly," "translucent." *P* emphasized especially the definite character of the contour, basing his judgment of 'perfect' figure-ground chiefly upon this and brightness-contrast. *B* regarded the 'perfect' experience as a compromise of brilliance-difference with depth and texture. Depth and translucency of ground (the essential feature of the texture-difference) vary concomitantly (he reported), and these vary reciprocally with brilliance. *Z* appeared to judge mainly upon depth and texture-difference between figure and ground.

A poorer experience (judged 'very good') is one in which the figure is homogeneous, black, and with continuous contour. The brightness-difference between figure and ground is good or medium. Again there was failure of agreement among the *O*s in regard to the nature of secondary features. *P* reported that the figure stood out from the ground, *B* that the figure stood out very little from the ground or that the two fields were on the same plane, and *Z* that the fields were on the same plane or that the ground was not distinctly localized either in the same plane as the figure or in another plane. *B* reported the absence of marked difference in texture: both figure and ground were matte, the black having a 'dead' character. *Z* reported the figure as "rather luminous," the ground as milky with duller finish. *P* emphasized the fact that the contour, though continuous, was not quite perfect, having a bit of indefiniteness about it. This was his chief characterization of the experience; *B* relied mainly upon the poor depth and matte texture as criteria of judgment; *Z* depended upon texture-difference and lack of depth.

The next type of experience (judged 'good'—but not to be confused with 'good' figure-ground experience as heretofore discussed) was that of a usually inhomogeneous, smudgy figure of not very intense blackness, and with continuous contour. Brightness-contrast between figure and ground was not great. The ground was not white, but a medium grey. *B* reported sometimes no depth, sometimes best depth, for this experience. *Z* reported the two fields as on the same plane. *P* found the figure to stand out as in all cases. *B* reported that the background was not very distinct from the figure; rather, the two fields formed a unitary whole. *Z* reported further that figure and ground had a dull finish. *P* based his judgment chiefly upon the fuzzy, indefinite character of the contour. *B* reported usually that the contour was not strictly sharp. He distinguished this experience by low intensity of the black and undifferentiated yellowish background. *Z* relied for his judgment essentially upon the lack of difference in depth and in texture.

'Perfect' figure-ground experience is characterized by homogeneity of the fields, intensity of the figural field, definiteness of the (continuous) contour, decided brightness-difference of the fields, protrusion of the figure in front of the ground, and, perhaps, texture-difference, in which the figure is of surface-character, while the ground is filmy or translucent. As these features are reduced in value, the experience moves downward in the 'goodness' scale. A given experience can be judged only by reference to this scale, and the scale itself is a function of the particular range of conditions included in the series. Only by an extension of series can one determine the full course of development of the figure-ground phenomenon.

Series 9. In this series the stimuli were presented for much longer times, varying from a few seconds to more than two minutes, to test whether such extension of exposure would reveal any fundamental change in the course of the figure-ground phenomenon.

Apparatus. The apparatus consisted of the tachistoscope as before, with a new method of control of exposure. A double-throw switch was operated by *E* to remove the illumination of the fixation-field and immediately to illuminate the exposure-field; simultaneously with this was completed a magnetic circuit which traced an exposure curve on a chronograph. On the same chronographic record a seconds pendulum traced a time line. Two other simultaneous tracings, actuated by finger keys, gave points of orientation in the exposure.

Instructions. The instructions were: "Observe the presented stimulus and during the observation report your experience. The stimulus will be maintained for a time of several seconds. Report any change in the experience during the period of presentation."

This procedure revealed nothing new; there is little to add, aside from confirmation, to the results of the previous series. The fundamental features of the figure-ground phenomenon were present in essentially their previous form, with expected minor differences of intensity and degree.

It is interesting to note that the 'goodness' of the experience does not continue to increase with extension of the exposure-time. This, indeed, was suggested by the 'law of diminishing returns' found in operation in the previous series. With prolonged staring the intensity of the field decreases (due to retinal fatigue), and brightness-difference, contour, and shape are reduced. Depth, on the other hand, may be greatly augmented, so that evidence is added to that previously obtained in support of the statement that the various features in the figure-ground phenomenon, though at times concomitantly varying, are not absolutely bound.

There were obtained certain data which, though incidental to the principal question of this section, are of interest in the general problem. All *O*s spontaneously reported, with these protracted exposures, a reversal of the experience in which white became figure and black the ground. In fact, it is the general rule, in an extended exposure of this sort, for the experience to alternate between black as figure and white as figure. At times this reversal is extremely rapid, too rapid for report, like a flutter; but usually the fluctuation is very irregular, with changes occurring on an average of once in 2 or 3 sec. Frequently only a portion of the white takes on figural properties, while in another region of the stimulus-field a black figure is still present.

The experience usually takes on object-meaning:[48] It looks like, or 'is,' some familiar object, and can so be named.

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[48] The expression 'object-meaning' is here used to indicate the reference of an experience to some particular object, by being named as that object, or being reported as like that object. The intention is to avoid the implications which the use of the unmodified term 'meaning' might involve, for this term has frequently been employed by writers who main-

B: "Black figure. Getting meaning—caricature of a face."

P: "Looks like a bud growing off a tree."

Z: "There was a bewilderment at first, and almost immediately it jumped into a Mutt-and-Jeff motif. Then the figure became more definite and clear and more uniform in color."

Object-meaning may change rapidly; report of such change is ordinarily to be correlated with a change of dimensionality.

Z: "This figure first appeared to be a duck. Then some kind of inner closed object. Then the metal handle of some motor. In all these it tends to become an object in three dimensions, and can be shoved back into a flat picture. Here it turned into a silhouette picture of a woman."

When object-meaning is present the fields may be less liable to reversal, due either to the persistence of a single object-meaning, or to the persistence of a given field as the bearer of object-character.

P: "The only way that I can hold the figure is to make the figure mean something. This can make a face with a prominent nose. So long as I have that meaning in mind the thing stays."

Z: "By fluctuating between various meanings the black kept stable character."

When object-meaning is present, alternation is accompanied by change of object-meaning.

Changes of dimensionality have been mentioned in connection with alternations of object-character. Such changes may be most striking:

Z: "First looked like a helmet. Striking change when it turned into a bird. The plane of the picture seemed to shift so that the wings became more horizontal. The ground is thin, vaporous, of the character of the sky. The figure stands out. The wings and the posterior part of the body are at right angles in space." "When a particular part determines the perception it seems to be clearer, with greater difference of depth at that point. Alternations between the figure coming out and going back to the level of the ground: seems to come out most clearly when there is a definite meaning."

After protracted observation (30 sec. or more) the *O*s reported unusual experiences of depth. For *B* these were particularly striking:

"Hole now. When it's a hole I get the snow-effect. Now the figure's becoming grey. Now black again. Now black. Now hole. Now grey film, localized way behind. I can see at least two feet back. I can see way, way back in that hole."

These unusual experiences of depth are related to a 'grey haze' that comes with long observation of a stimulus. The clouding-over that appears after steady fixation of a color-spot is a familiar affair. The experience here seems to be similar. *Z* reported:

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tain that in the temporal course of experience a 'meaning' is added on to a previously unmeaningful sensory core. It was very obvious in these experiments that the most primitive experience has a real meaning, though it is not necessarily referred to some familiar object outside its setting. The simplest figure-ground has truly a significance to the subject. Upon more extended observation the perception undergoes change. When this change is such as to produce a report, a naming, as of some familiar object, an object-meaning may be said to be present. The distinction represents merely a particular sort of change in the figure-character of the experience.

> "Very marked changes of intensity: grey waves coming over and disappearing. These waves stop before coming to the contour. The contours seem to be blacker than the rest of the figure."

The features of interest in the present context are three in number. The correlation with unusual depth may be understood by reference to the common phenomenon of aerial perspective: due to atmospherical effects, distant objects appear blurred and hazy as compared with near objects. A peculiar fact is that the grey film always stops short of the contours. It would seem that central factors are such as to maintain the contour in its definite form aside from any changes due to retinal adaptation or fatigue. The haze-effect itself periodically comes and goes, without discoverable correlation with alternation of fields.

The experiencing of one or another field as figure is in some degree correlated with the constitution of the stimulus. Rubin is confirmed in the proposition that fields with numerous prongs and lobes are more easily perceived as negative (peripheral-field) figures than are simple fields. Some stimuli are more easily experienced with a particular field as figure. Fixation was reported by all *O*s as one of the determinants in the perception of a given field as figure, though one of the *O*s declared that fixation is not a compulsory factor. This declaration is supported by results of further studies.

> *P:* "Black figure. Now when I fixate the lower white bump the black part may be figure or ground. It is necessary to have something in addition to fixation for the change. What that is I don't know." And again, "It's attention . . . I'm fixating now the white point, and I still see the thing as a black figure, because I attend to the contours at another place than where that white point is. Now when I fixate and attend to the white point the contours become less distinct and less clear, and the white thing looks like the inside of a cave, the black is opening."

One must here leave open the question whether change of fixation occasions a reversal of fields, or whether reversal is prior to a change in fixation.

As suggested above, attention may be one of the important determinants of a given manner of perception of an equivocal stimulus. The reports of the *O*s suggested that the part attended to tends to come out as figure.

One may conclude from the results of the last two series that increase of presentation-time, even as high as 30 sec., occasions no striking change in the figure-ground phenomenon. No new essential features are revealed. Further, one is not able to establish any conditions under which all of the various features of figure-ground experience will in highest development be insured, even 50% of the time. There is evidence that long times, up to a certain point, favor brightness-difference, but militate against perception of the best depth; beyond that point, however, increased exposure, through adaptation or fatigue, reduces brightness-difference, and gives an experience of most exaggerated depth. The features of heterogeneity, brightness-difference, contour, and shape have been found in all stages to maintain their fundamental position as distinctive of the figure-ground phenomenon.

Summary of Results

The figure-ground phenomenon is a complex affair which on analysis reveals a number of properties. These properties are presented in a series of stages ranging from simplest upwards in the direction of complexity. Certain fundamental properties—heterogeneity, brightness-difference, contour, and shape—are present, in primitive or advanced degree, at all stages of development of the phenomenon. The presence of these fundamental features is essential to the perception of figure and ground.

(1) *Heterogeneity.* For figure-ground perception there must first be present a differentiation of regions within the field of vision. One of these regions is the figural field, the other is the ground. To be a (figural or groundal) field a given visual region must not only be distinct from the remainder of the visual area, but must be within itself homogeneous; at least, the homogeneity of the visual area taken entire must not be greater than the homogeneity of each of the two fields taken separately. Or, what is the same thing or a consequence of this, the figural and groundal fields must each constitute a unit; if the visual area as a whole constitutes a unit, figure-ground differentiation does not appear.

(2) *Brightness-difference.* The intrinsic homogeneity of the separate fields is a function of the distribution of absolute brightness; the heterogeneity of the experience is a function of the difference in brightness of the two fields. A minimum of brightness-difference is necessary for the experience of figure and ground. As brightness-difference increases, the 'goodness' of the experience increases, though at a constantly diminishing rate.

(3) *Contour.* After brightness-difference reaches a certain value there is a perceived 'region of separation' of the two fields. This 'region of separation' is actually a region; it extends over an area. It is grey in quality (for a black and white stimulus). It marks off one field from the other. In earlier stages this separation is not abrupt; it passes gradually from one region to the other. The transition must attain a certain grade before figure-ground experience appears. Below that grade the experience is merely that of a nucleus which is dark in the center and grows gradually lighter toward the periphery; here there is no differentiation of fields. As exposure-time increases above the mini-

mum the separation-mechanism undergoes progressive change, narrowing in its sectional course until there appears in the perception a definite 'step.' This 'step' may have areal properties still (called a 'fuzzy contour'), but contraction continues until it is reduced to a simple line, which in its higher stages loses its dimensional properties and finally is nothing more than the place where the figure ends. This is 'definite contour,' the breadth-less, çolorless contour of Rubin. As exposure-time increases from this point, contour increases in its definiteness or sharpness; that is to say, the 'step' becomes a higher 'step.' In the lower stages there are regional differences in the contour; after a certain stage the contour is continuous.

(4) *Shape.* Shape is a property of the figural field. It is a function of the areal distribution of the field, and of the separation-mechanism. Shape occurs before definite contour: a gradual transition is sufficient to outline the shape of the figure. As the contour becomes definite, the 'formed-ness' of the shape becomes greater.

When these fundamental features are minimally present the experience is of the simplest sort. When conditions are such that they occur in more developed state, and additional properties are present, the phenomenon is at a 'higher' stage. Additional properties are minimal localization (or protrusion of the figural field), definite localization, texture, and halo.

(5) *Protrusion of the figural field.* Apparently dependent upon the degree of heterogeneity between the two fields is a protrusion of the figural field. The figure stands forward from the background. This minimal difference in depth-localization of the two fields does not seem to be differentiated from the more impressive, more insistent character of the figural field.

(6) *Definite localization.* In the higher stages of the phenomenon many experiences present a definitely perceived difference in depth-localization of the two fields. Between this and minimal localization occur various intermediate forms. The precise relation between these more definite forms of localization and the minimal type of localization just mentioned is yet obscure. The complete course of the depth-experience with progressive increase of time of exposure has not been worked out, but preliminary results show an initial increase of definiteness of the depth-experience, and then a decline up to the point at which protracted staring brings on retinal changes which result in a decrease of brightness and a coincident increase in depth.

(7) *Texture.* Closely related to a typical form of depth-localization is the texture of figural and groundal fields. The figure, in certain advanced stages of the phenomenon, has a surfaced texture, while the ground is filmy or translucent. The

evidence that appeared is in agreement with previous experiments, which showed relationship between texture and localization. Ordinarily the field that is surfaced has a definite localization, while the localization of the filmy field is indeterminate.

(8) *Halo.* Under certain conditions a halo appears just outside the dark figure as a region which is brighter than the peripheral portion of the ground. Typically, the halo is a part of the ground, though at times it has different localization. It is presumably due to the little-understood phenomenon of simultaneous contrast.

As has been shown, the figure-ground phenomenon is a function of the particular conditions of observation, and changes as the conditions change. With stimulus and setting kept constant, a progressive change in time of exposure and illumination has correlated with it a continuous change in the phenomenon. Certain stages of the phenomenon were selected for intensive study, and may be regarded as reference points in the whole course of development of the phenomenon. These stages are 'simplest' figure-ground experience, 'good' figure-ground experience, and a highly-developed stage that appeared in the quest of an experience that could be called 'perfect.'

(*1*) *Simplest figure-ground.* If within the field of vision there appear two intrinsically homogeneous regions with a degree of heterogeneity sufficient to give a differentiation into what can be called two fields, if there is, for a black and white stimulus, a minimal difference of brightness between the two fields, if there is a certain grade of separation between the two fields, and if one of these fields has an areal distribution of a certain perceptual value, then figure-ground experience is present in its simplest form. Liminal conditions for this stage are about 10 σ.

(*2*) *Good figure-ground.* When the fundamental properties of 'simplest' figure-ground experience are present in more highly developed state, the brightness-difference has a greater value, the separation-mechanism is a 'step' continuously bounding the figure, and the shape is fairly well 'formed;' and when in addition the figural field protrudes in the field, the experience can be termed a 'good' figure-ground experience. Definite separation of the two fields in depth may be present, but is not essential. Liminal conditions for this experience are 11-17 σ.

(*3*) *'Perfect' figure-ground.* As exposure-time increases, the fundamental properties of figure-ground experience attain for the most part greater definiteness, but there were found no liminal conditions under which an experience arises that can be called 'perfect.' With longer times of exposure texture-differences between figure and ground are more likely to be demonstrated. With increase of exposure-time the various properties do not develop at the same rate; one, depth, may even vary inversely along part of the temporal course. This would seem to preclude the appearance of an experience containing all the properties of figure-ground experience in most highly developed state.

CONCLUSION

The share of consideration that has of late been accorded the phenomenon of figure and ground is due in large measure to the assimilation by *Gestaltpsychologie* of the experimental work of Rubin, and the status of this figure-ground concept in the theory as a fundamental type of mental structure. As such a type the figure-ground experience has been a focal point of many systematic issues, for it involves the question of a fundamental analysis of simple visually perceived form into figural and groundal components. Moreover, since figure-perception is said to involve thing-perception and the appreciation of wholes, it is plain that the experience may be crucial in the understanding of the problem of objectification and meaning. This theoretical significance of figure and ground in the visual perception of form was apparent even at the time of Rubin's study, but recently the situation has grown much more critical because of the conflict between *Gestalt* theory and traditional psychological method. If the figure-ground experience is, as held by the *Gestalt* group, the simplest perceptual type, its analysis and conditions should give some cue to the general problem of perception.

This analysis, as conducted in the present setting, was by way of controlled variation of the time of exposure of simple, equivocal stimuli, with the maintenance of relatively constant attitude and conditions of observation, so that any change in the phenomenon might be regarded as a function of a simple variable—time (and a quantity dependent upon time, intensity of illumination). Changes do in fact appear: as the time of presentation increases from very brief exposures, the experience changes in the direction of definiteness and complexity.

At the lowest level of the figure-ground experience few features appear, under the conditions just outlined, and these in but poorly developed state. The figure-ground experience of the very simplest sort shows as distinguishing features (1) a given degree of heterogeneity of the visual field, in which two regions are discriminated, (2) a certain stage of brightness-difference between these regions, (3) a liminal value to the demarcation, or contour, separating the fields, and (4) such an areal distribution of the visual qualities that one field, the figure, has a simple, perceived shape. The separation-mechanism at this stage is no definite, well-marked affair, but a region of transition which may vary in breadth from one experience to another, and in different portions of the same experience. At a somewhat more advanced stage of the phenomenon definite contour begins to occur, showing itself in a limited portion of the experience as an abrupt transition from one field to the other.

This abrupt gradation is later extended so as to encircle the figural field as a continuous or complete, definite contour. Its development is accompanied by changes in other fundamental features: the brightness-difference is much greater, the degree of heterogeneity is increased, and the shape has taken on more and more 'form.' Moreover, the figural field presents a primitive type of localization, a protrusion in the visual field. This stage may be called 'good' figure-ground experience. Definite localization of the figural field may be present at this time, but shows itself more prominently somewhat later.

With increase of exposure-time the various features are presented for the most part in more highly developed state, though there seems to be operating a law of diminishing returns: at the remote end of the developmental course a further increase of presentation-time has little effect. Depth-localization is the least uniform feature. It appears in a variety of forms and degrees, and of the factors mentioned above it is least subject to control. A halo-experience comes in at a more primitive stage of the phenomenon, but is not an essential feature of figure-ground experience. Nor is texture, which, as it tends to appear at comparatively advanced stages, may be classed among the 'higher' properties.

No compulsory conditions were found for a 'perfect' experience of figure and ground. With increase of exposure-time the various isolated factors do not develop at the same rate. In fact, time may be increased to a point at which adaptive changes cause a reversal in the direction of development of brightness-difference by reduction of visual intensity. No new features were found to appear with increase of exposure beyond the brief times involved in 'simple' and in 'good' figure-ground experience, so that from these data one is led to conclude that the essential features of the figure-ground phenomenon have been revealed.

The nature of the experience of figure and ground depends upon many things. An analysis may be effected by variation of one phase of the situation while other conditions are constant, yielding a series of experiences correlative to the variable, time. It is the whole correlated series that is to be regarded as *the figure-ground phenomenon.* A given figure-ground experience, then, is but one of many possible stages of the complex perceptual phenomenon, and its precise nature is a function of the various components of the situation in their entirety.

THE INFLUENCE OF MUSCULAR TENSION ON THE EFFICIENCY OF MENTAL WORK[1]

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PROBLEM

It is commonly assumed, with some experimental justification, that intense mental activity is accompanied by increased muscular tension. The present study was undertaken to determine whether such increased muscular tension exerts any influence upon the efficiency of mental work, or whether it must be considered as merely an overflow phenomenon without functional significance. It was hoped that the investigation might throw some light upon the nature of *effort*, how it is exerted, and how it reacts upon mental efficiency.

HISTORICAL SUMMARY

The direct evidence thus far is limited to one side of the problem only, that of demonstrating that increased muscular tension accompanies mental work. The other side of the problem, the question as to what functional relationship this increased tension bears to mental efficiency, has not yet been attacked directly. The evidence which exists is indirect.

With regard to the first question, the preponderance of evidence is positive. On the negative side, Loeb (1888) found that pressure on a dynamometer tended to slacken when accompanied by mental activity, and concluded that mental effort lessened muscular effort. His results were confirmed by Lehmann (1900) with the ergograph. R. MacDougall (1896) measuring the contraction of the index finger during concentrated attention, found a reduction in tonicity.

On the positive side, Lombard (1887) found that the knee-jerk was enhanced by accompanying mental activity, and his findings were very recently confirmed by Tuttle (1924). Dresslar (1891) got similar results with voluntary tapping. Féré (1889) found that subjects would increase their maximum squeeze on the dynamometer by one-fourth, when mental activity was induced. Among others, Mosso (1894) obtained a heightened ergographic curve from a subject engaged in strenuous mental activity. The most extensive work has been done by Golla (1921) who measured the tonicity of the various body muscles during mental effort by the use of extremely delicate apparatus, and invariably obtained heightened curves.

As has been said, the evidence bearing on the second half of the problem, the influence of this increased tension on mental efficiency, is mainly indirect. Several studies tend to show that increased *attention* tends to increase the intensity of sensation. In the field of vision, W. McDougall (1904) found that when successive flashes were presented 1 sec. apart, either could be made brighter by voluntarily setting the attention. One of the most recent studies is that of Newhall (1923), who found that attention to cutaneous and visual stimuli increased their intensity. The difficulty is that such studies as these employ a subjective definition and measure of attention, and do not necessarily imply increased muscular tension. That muscular tension was probably involved, however, is evident from studies such as those of Jacobson, Breese, and Miller.

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[1] From the Psychological Laboratory of the University of Chicago.

Jacobson (1912), in a study upon the influence of distracting stimuli in inhibiting sound sensations, found that the effectiveness of the distractor depended on the amount of attentive effort which subjects reported to be given to the distractor. Moreover the influence of the inhibiting agency could be neutralized by voluntary attention, and when the distractor caused the subject to increase this effort of attention to the sound, the sound seemed even stronger than when it occurred with normal attention, without distraction. He then tested the influence exerted on the intensity of the sensation by the abandonment of all attentive effort. This required teaching the subjects how to abandon attentive effort. From their introspections and objective behavior, he discovered that attentive effort involved tense muscles. "In attentive effort," says he, "the observer's brows might be wrinkled, his mouth compressed, the muscles of his trunk and limbs alert and contracted." By requesting the subjects to eliminate these tense attitudes, he succeeded in securing a high degree of effortlessness, with resulting decrease in sensation intensity. Upon reversing the procedure and allowing the subject to exert strong muscular effort to attend to the sounds as distractors, he succeeded in getting complete inhibition of odors.

Breese (1899), in working with the phenomenon of retinal rivalry, discovered that contraction of the arms and legs of either side of the body, and the maintenance of this rigid tension during the experiment, tended to lengthen the periods of visibility of the respective fields.

Miller (1925) found in administering electric shocks to the arms of her subjects, that, if they relaxed the body musculature, the intensity of the subjective experience of the shock was greatly reduced or even entirely removed.

Many studies demonstrate the influence of attention on the speed of reaction. Cassel and Dallenbach (1918) found that shortness of reaction time is correlated with attention. Others find that the interval most favorable for preparing and holding the attentional attitude by a warning signal is most conducive to short reaction times. The shortening of reaction time with the 'motor' as opposed to the 'sensory' attitude, originally noticed by L. Lange (1888) working under Wundt, may be due to the fact that the motor set involved more muscular tension. This seems to be borne out by the work of Judd (1905) and of Williams (1914) in analyzing the difference in preparatory muscular adjustment involved in the two types of set.

The discovery by many investigators that stimuli used as distractors often act as facilitators may be explained by the fact that greater muscular tension is called forth by distraction. Morgan (1916) reports an improvement in the speed and efficiency of mental work as a result of introducing sensory distractors. He explains it by the fact that his subjects put forth greater effort under distraction. This effort involved muscular tension, shown by the greater pressure on the reacting key, and by muscular activity of the speech organs, etc. Conversely, speed of reaction is reduced by relaxation of the body musculature, as is shown by the work of Miller (1925).

The broader question of the relation of muscular tension to 'purpose,' and the 'problem idea' has been given admirable theoretical treatment by Washburn (1916). "The motor innervations underlying the consciousness of effort," she says, "are not mere accompaniments of directed thought, but an essential part of the cause of directed thought."

It is entirely probable that the results which were secured by W. McDougall (1919) in his investigation of the influence of the 'conative' attitude upon the speed of learning and efficiency of retention of memorized material, were due to the fact that his 'conative' attitude involved greater muscular tonicity than the control condition. At least such studies could be made quantitatively more clear-cut if the variable condition could be defined in terms of amount of muscular tension rather than by such a vague factor as 'conative tendency,' or the absence of it.

The foregoing historical survey demonstrates the need of a quantitative and qualitative study of the influence of muscular tension on the efficiency of mental activity.

Analysis of Problem

Will an increase in the total amount of muscular tension in the body increase mental efficiency? It is impossible to measure the total amount of tension in the body. But it is possible voluntarily to increase this amount by a definite increment and to note the effect of this increase on mental output, as contrasted with the output under the normal condition. The problem is complicated by the following factors:—

First, in evaluating the efficiency of mental work by the nature of the output, certain criteria of excellence must be chosen. The speed with which the work is done constitutes an important measure of efficiency. Yet speed is meaningless unless taken in conjunction with quality. This, in the case of learning problems, refers to the thoroughness with which the material is assimilated, and may be measured by the amount recalled and the saving in relearning over the original learning time. In other mental tasks it usually refers to the accuracy of the results. Hence our problem is to determine just how tension influences each one of these factors individually.

Secondly, since the largest amount of tension and random muscular activity has been observed to accompany the early stages of learning, the question arises whether after practice in any type of mental work, the initial benefit from muscular tension will tend to decrease, until tension becomes no longer effective, *i.e.* in other words, what effect will practice have?

Finally, how will muscular tension influence the general decrement from continuous mental work? Since it involves fatigue of muscle tissue, will it add to or decrease the mental fatigue?

The problem may therefore be restated as follows: (1) Will an increase in muscular tension beyond the normal result in an increase in the efficiency of mental work? (2) Will this effect increase or decrease with practice? (3) Will it increase or decrease with the onset of fatigue? (4) Will the results differ for different criteria of mental efficiency?

Apparatus and Methods

Four distinct experiments were performed. They differed from one another in the type of mental work used, the work instructions, and the criteria of efficiency measured. But throughout all of them, the method of attack was similar. It consisted in securing a large number of measures of mental out-normal amount of muscular tension was used; the other where it was increased by a definite and constant amount. Hereafter these two conditions will be referred to respectively as *normal* and *tension*. They were always given alternately, so that practice and fatigue effects were equalized for both in the averages. Additional statistical validity was secured by the order of arrangement of individuals.

The *S*s sat in customary posture before a table on which was exposed the material to be used. If memory material, it was presented through a Chicago memory-drum. Otherwise it was exposed on a tilted standard at a convenient distance for clear vision. The only difference between the normal and tension conditions was that, under the latter, the *S*s grasped, and squeezed continuously at a constant pressure, spring dynamometers, one for each hand. The strength of the dynamometers had been reduced two-thirds by inserting weaker springs. An optimal degree of pressure was determined for each *S* in a preliminary series, and then adhered to throughout. Experiment I differed from the others in being exploratory. Hence both constant and intermittent tension were used, and the amount was allowed to vary. Kymographic records were kept by connecting the dynamometer needle with a recording device, and the resulting records were compared with the performance. It was found that constant tension was as satisfactory as intermittent tension, and was less disagreeable. Moreover, practice soon enabled the *S*s to maintain a constant amount without prompting. Hence the recording device was discarded in later experiments. The weakening of the springs, and the use of two dynamometers instead of one, were changes made after the first experiment.

EXPERIMENT I

In Experiment I the mental work consisted of *learning nonsense syllables*. Nine *S*s, 4 women and 5 men, all advanced college students, took part. They were kept in ignorance as to the purpose of the experiment. Every individual reported twice daily, once to learn the list of nonsense syllables, and again 3 hrs. later to recall and relearn it. The two work conditions were employed on alternate days so that, of the 20 lists which every *S* learned during the 20 days of the experiment, 10 were learned under the tension and 10 under the normal condition. Half the *S*s began on tension, half on normal. The regular series was preceded by 2 days of practice under both conditions to familiarize the *S*s with the procedure.

Carefully selected lists of comparable difficulty were used. Every list contained 9 syllables of 3 letters each, exposed one at a time, and learned by the anticipation method. That is, after the list had been presented once, during subsequent presentations *S* recited every syllable he could recall, just before it appeared. When he could anticipate the entire list correctly, it was considered learned. Record was kept of the number of complete presentations of the list necessary to learn it, and of the syllables correctly anticipated during every presentation. Since each syllable was exposed just 2 sec., and the lists contained 9 syllables, the actual learning time can be computed, for a given list, by multiplying the number of pre-

Record was kept of the number of syllables recalled on the first presentation, and of the number of presentations necessary to relearn the list. By comparing this with the original learning time, the saving in relearning could be computed thus:

$$\text{Saving score} = 100 - \left(\frac{\text{presentations necessary to relearn}}{\text{presentations necessary to learn}}\right) \times 100.$$

This gives us the following measures of learning efficiency: (1) learning time; (2) syllables recalled; (3) percent saved in relearning.

A comparison of the learning records for successive days displays a well marked practice curve for both conditions, from which we can judge whether the relative effectiveness of the two contrasted work conditions is differently influenced by practice.

Results of Experiment I: (A) Absolute ratios between 'tension' and 'normal' scores. By all three criteria of learning efficiency, 'tension' is shown to be more efficient as a work condition than 'normal.' The average learning time under the normal condition, as measured in repetitions, is 9.8 repetitions per list, whereas under tension it is only 8.6, a decrease of 1.2 repetitions per list, or approximately 13%. The probable error of this difference is 0.27, which insures its statistical significance for all practical purposes. On the whole, then, we may say that our *S*s learned more rapidly under increased muscular tension.

This result would have little significance, however, if it were coupled with a compensating loss in the efficiency with which the material was assimilated, as shown by the recall and saving criteria. Yet, as a matter of fact, the tendency was to recall a greater number of syllables in the case of the lists learned under tension. The average recall for lists learned under 'normal' is 3.9 syllables; for lists learned under 'tension' it is 4.4 syllables, an increase of 0.5, or nearly 13%. In this case the probable error of the difference is not so low, being 0.198, or about ⅖ of the difference, which makes the chances more than 5 to 1 that a new pair of measures would show a difference in favor of 'tension.' Again, when we compute the saving scores, we find that the average saving for lists learned under 'tension' is 64.4%, while for those learned under 'normal,' it is only 60.6%, a difference of 3.8, or about 6% in favor of 'tension.' The raw relearning scores show a more marked difference. The average number of repetitions required to relearn lists learned under 'normal' is 4.9, whereas for those learned under 'tension' it is only 3.7, a decrease of 1.2 repetitions, or about 32%.

A comparison of the learning records, under the two contrasted conditions, as to the proportion of correct to false responses during the actual learning, throws light on the cause for the better retention of material learned under 'tension.' The ratio of correct responses to total presentations gives us what might be called an *overlearning score*. The more overlearning a syllable receives, the better it will be retained, we assume. The

TABLE I

Absolute Ratios Between Normal and Tension Scores

S	LEARNING TIME				RECALL				RELEARNING TIME				SAVING				OVERLEARNING			
	No. Repetitions				No. Syllables Recalled				No. Repetitions				% Saved in Relearning				Ratio Between Presentations and Correct Responses			
	Tension	Normal	More with Norm.	% More with Norm.	Tension	Normal	More with Tens.	% More with Tens.	Tension	Normal	More with Norm.	% More with Norm.	Tension	Normal	More with Tens.	% More with Tens.	Tension	Normal	More with Tens.	% More with Tens.
Al	7.1	6.7	−0.4	−6.0	5.2	5.1	0.1	2.0	3.2	2.6	−0.6	23.0	65.0	72.5	−7.5	10.3	71.3	71.2	+0.1	0.14
Fi	10.1	10.0	−0.1	−1.0	5.7	5.2	0.5	9.6	3.7	4.0	0.3	7.5	74.6	70.1	4.5	6.4	62.4	60.5	1.9	3.1
Fr	14.1	11.7	−2.4	−21.0	4.1	2.9	1.2	41.4	4.7	6.5	1.8	28.0	71.6	51.5	20.0	38.8	63.0	58.8	4.2	7.1
He	7.8	7.9	0.1	1.3	4.0	1.9	2.1	111.	3.8	6.7	2.9	43.3	60.9	37.1	23.8	64.2	70.4	65.6	4.8	7.3
Li	5.5	7.5	2.0	26.6	2.5	3.6	−1.1	30.5	5.1	3.7	−1.4	37.8	39.0	59.0	20.0	34.0	72.0	73.4	−1.4	−2.0
Mo	9.4	12.4	3.0	24.0	0.5	2.8	−2.3	82.0	5.3	9.3	4.0	43.0	47.4	47.8	−0.4	−0.8	69.7	63.6	6.1	9.6
Pi	7.0	12.0	5.0	42.0	6.5	5.5	1.0	18.2	2.3	3.5	1.2	34.0	77.3	72.2	5.1	7.0	76.7	66.4	10.3	15.5
Sh	6.9	7.0	0.1	1.4	5.6	4.7	0.9	19.2	2.3	3.1	0.8	26.0	70.0	65.0	5.0	7.7	65.6	62.5	3.0	4.8
Te	9.2	12.9	3.7	28.0	5.3	3.9	1.4	36.0	3.4	4.2	0.8	19.0	74.1	70.3	3.8	5.4	56.9	51.4	5.5	10.7
Av.	8.6	9.8	1.2	14.0	4.4	3.9	0.5	13.0	3.7	4.9	1.2	32.0	64.4	60.6	3.8	6.0	67.5	63.7	3.8	6.0
N = 90	P.E. = .27				P.E. = .20															

'tension' condition shows an average overlearning score of 67.5, or 6% more than that under 'normal.' Is there not, then, justification for concluding that a larger proportion of the syllables are learned at once, under tension, and hence receive more overlearning? Under the normal condition, on the other hand, they are learned later in the game, and hence suffer from lack of repetition, and are sooner forgotten.

Table I shows the relative efficiency of every *S* under the two contrasted conditions, as measured by each of the criteria tested. It will be noticed that 6 *S*s out of the 9, or ⅔ of the group, learned more quickly under 'tension,' and that of the 3 who did not, every one compensated for this fact by recalling more under 'tension.' On the other hand, 2 of those who learned more quickly under 'tension' compensated for this gain by a loss in recall efficiency. Since no one criterion is more valid than another, the best way to get at the true measure of efficiency is to determine how many of the *S*'s excelled in every criterion under tension, and how many of the rest excelled in the majority of the criteria. We find that 4 *S*s, He, Pi, Sh, and Te, excelled in all criteria under tension; and that, of the remaining, 3 excelled in the majority of criteria. Hence, on the whole, tension was a more efficient condition for more than ⅔ of the *S*s in this experiment.

(*B*) *Influence of practice on the relative efficiency of 'tension' and 'normal.'* A comparison of the practice curves for the two conditions, when *learning time* is the criterion, shows that the decrease in time due to practice is somewhat greater for tension than it is for the normal condition. With *recall* and *saving* as criteria, however, the opposite is true; the normal condition shows an increase in amount recalled as a result of practice, whereas the tension condition shows an actual decrease, even though this decrease is not enough to incline the balance for the whole experiment. As previously mentioned, the absolute averages show 'tension' to be the more efficient. Evidently the different criteria are differently affected by practice, and consequently require separate analysis.

(1) *Practice and learning time.* Table II shows how practice influences the difference between the 'normal' and 'tension' conditions in speed of learning.

TABLE II

Influence of Practice on Learning Time: Average Number of Repetitions

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Gain	First Half	Second Half	Gain	Greater Gain With Tension	% Greater
10.6	9.0	1.6	9.6	7.5	2.1	0.5	31

It gives, for both conditions, the average learning time for the first 5 lists and for the last 5 lists and the difference, showing the decrease in time due to practice. The last two columns of the table show how much greater this decrease, or practice effect, is under tension. The averages for all *S*s show a difference of 31% in favor of 'tension.'

Graph I gives a clearer picture of the difference in trend, showing as it does the day-to-day changes. It also enables one to compare the initial and final day's records. The 'tension' curve begins at a higher level, but soon drops well below the 'normal' curve. We may conclude, then, that the increase in speed due to practice is somewhat greater with tension than under normal conditions.

(2) *Practice and recall and saving.* Table III gives data similar to that of Table II, the criteria this time being the average recall and saving for the first 5 and for the last 5 lists learned. Here the opposite tendency is noted in both cases: under the normal condition, the recall and saving scores show a very slight tendency to improve with practice, whereas with tension there is a falling off in initial efficiency, although this never reaches the low point at which the 'normal' scores started.

TABLE III

Influence of Practice on Recall and Saving

AVERAGE	NORMAL			TENSION			DIFFERENCE	
	First Half	Second Half	Gain	First Half	Second Half	Gain	Smaller Gain with Tension	% Smaller
No. Syllables recalled	3.8	4.2	0.4	5.0	3.8	−1.2	1.6	400
% Saved in relearning	58.6	62.6	4.0	70.3	58.5	−11.8	15.8	400

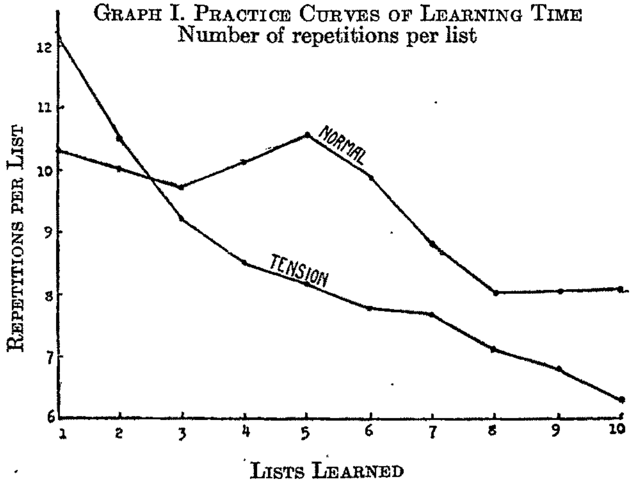
Evidently, for these criteria, tension is no help to efficiency after the habit has once been well fixed. Thereafter it merely increases speed without affecting the amount recalled, or the saving in relearning.

Graphs II and III show the same tendency. The 'normal' curves take a more or less horizontal course, to which the 'tension' curves drop eventually. Thereafter they tend to travel along together at the same level. There is some evidence, however, that, if the curves were prolonged further, the 'tension' curve would continue at a slightly higher level.

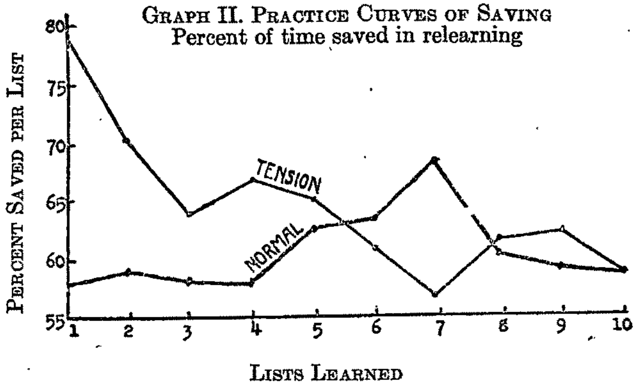
Summary. The purpose of Experiment I was to determine the influence of muscular tension on the efficiency of learning nonsense syllables. Record was kept of the learning time in number of repetitions per list, of the number of syllables recalled, and of the saving in relearning. *By every criterion, tension proved to be the more efficient condition for the majority of subjects.*

The influence of practice on the relative efficiency of the two conditions was then determined by plotting the day-to-day scores, and by comparing the scores for the first 5 lists with the scores for the last 5. It was found that the tension condition gained in relative efficiency with practice, where learning time was the criterion, but that for the other criteria, the initial difference in favor of tension tended to disappear, so that the two conditions became more nearly equal in efficiency.

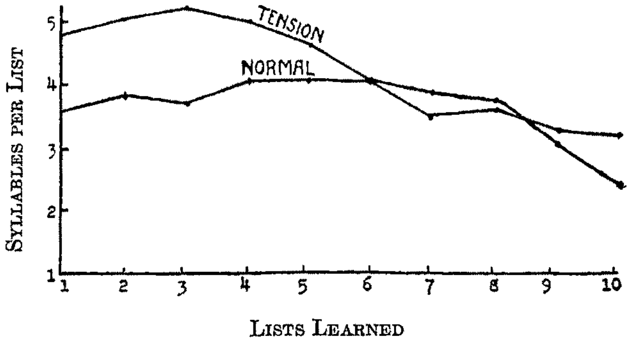
GRAPH I. PRACTICE CURVES OF LEARNING TIME
Number of repetitions per list



GRAPH II. PRACTICE CURVES OF SAVING
Percent of time saved in relearning



GRAPH III. PRACTICE CURVES OF RECALL
Number of syllables recalled



EXPERIMENT II

In Experiment II, the mental work consisted of *learning paired associates.* The 10 *S*s used were all graduate students of considerable training, 3 of them women, the rest men. The experiment differed from the preceding one in the following points: (1) Real words, selected at random, were substituted for nonsense syllables. (2) The words were arranged in lists of 10 pairs, and learned as paired associates instead of by the anticipation method. That is, the second word of each pair was recalled on presentation of the first. Tension was not used during recall. (3) The lists were presented only once, every pair being exposed just 3 sec. by means of a Chicago memory drum, and recall followed immediately.

The purpose of using sense words was to determine whether the results of the previous study would be altered bya change from pure rote to semi-logical learning. The paired associate method sets up simple horizontal bonds, and hence lends itself better to quantitative analysis where learning is incomplete. Most important of all was the third change. In the previous experiment we were dealing with two dependent variables, *time* and *recall.* This made it impossible to judge how tension and relaxation would affect *recall* alone, when not complicated by variations in learning time. Hence, in Experiment II, time was held constant, at 30 sec. per list, under both conditions for all *S*s.

Practice effect was equalized for the two conditions, in the following way: Every *S* learned 4 lists a day, the first and third under one condition, the second and fourth under the other. If, on Day 1, he learned lists 1 and 3 under tension, then on Day 2, he learned lists 2 and 4 under tension, etc. Moreover, half the *S*s began with tension and the other half began with relaxation. Thus, by this counter balanced order, both conditions were equally favored as to position. In all, 50 lists were learned under tension, and 50 under the normal condition. The regular series was preceded by two practice sittings, to familiarize the *S*s with the two conditions, to ascertain their optimal degree of tension, and to train them in maintaining a constant tension throughout the learning of a list.

As had been explained previously, the set-up for the tension condition was simplified in this and later experiments. Two dynamometers of ⅓ strength were employed, one for each hand. An optimal tension was determined for every *S*, by determining the amount of pressure he could maintain without annoyance. This depended, of course, on his strength. He was then instructed to use this amount during each 'tension' exercise, and the experimenter made sure that the degree was constant by watching the dial needle. Although the *S*s were invariably prompted before beginning, it was extremely rare for an *S* to need any reminder during a sitting. A habit or set was established during the practice sittings and maintained unconsciously.

Record was kept of the number of words correctly recalled. As in the previous experiment, a comparison of the performances of successive days displays a marked practice curve for both conditions, which enables us to judge whether the two conditions respond differently to practice.

Results of Experiment II: (A) Absolute ratios between 'tension' and 'normal' scores. A glance at the average results for all *S*s in Table IV shows that without exception every individual recalled more words under tension than under relaxation. On the average, 13% more words were recalled under tension; and the extreme case is that of Pa, who recalled 50% more material under tension than under the normal condition. The statistical significance of these differences in favor of tension is shown by the last column, which gives the number of times the difference exceeds its probable error. In the case of the general averages, this is over 6 times.

TABLE IV

Number of Words Recalled Per List

S	Tension	Normal	M_1-M_2	P.E.M_1-M_2	Diff. exceeds P.E.	% More recalled under Tension
Bn	5.0	4.4	.6	.31	.2	14
Be	7.3	6.4	.9	.24	3.8	14
Co	8.7	8.3	.4	.17	2.3	5
Dr	4.1	3.2	.9	.14	6.4	28
Ga	3.7	3.2	.5	.18	2.8	16
Hi	8.9	8.4	.5	.14	3.6	6
Ho	5.6	4.8	.8	.21	3.8	16
Pa	6.0	4.0	2.0	.28	7.1	50
Ws	6.4	6.2	.2	.31	0.7	3
Wh	4.5	3.9	.6	.22	2.8	15
Av.	6.0	5.3	.7	.107	6.5	13

TABLE V

Distribution of Recall Scores

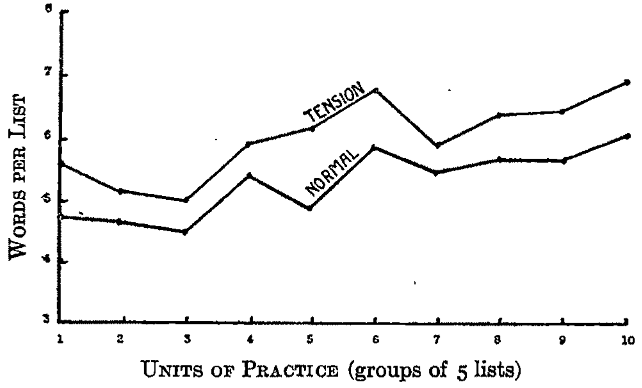
S	Number of Times More Words Recalled Under		
	Tension	Normal	Equal
Bn	23	14	13
Be	32	12	6
Co	26	15	9
Dr	31	9	10
Ga	23	17	10
Hi	21	10	19
Ho	27	13	10
Pa	38	4	8
Ws	18	20	12
Wh	28	12	10
Av.	26.7	12.6	10.7

Table V shows, for every *S*, the day to day consistency with which tension gave superior results. If we place the 50 lists learned under 'tension' and the 50 learned under 'normal' side by side, we find that every *S* except one got superior results much more often under tension than he did under normal conditions. In fact, the average for all *S*s shows that tension gave superior results more than twice as often as the normal condition. The extreme case is that of subject Pa, who displayed superior efficiency more than 9 times as often under tension.

Learning under tension is thus shown to be more efficient for meaningful material, when time is held constant and amount retained is the criterion of measurement. We may conclude, then, that the changed conditions of this experiment did not alter the fact established in the previous experiment, namely, that tension gives a higher absolute level of efficiency than the normal condition.

(*B*) *Influence of Practice.* It is in the practice curves obtained by plotting the day-to-day recall scores under each condition that we find a decided contrast between this and the preceding experiment. In the former study, whereas for speed of learning the initial advantage in favor of tension increased with practice, the recall ratio took the opposite course, so that finally the tension and normal levels were about equal. In the present experiment, on the contrary, practice does not change the relative levels of efficiency displayed under the two conditions. Tension gives about 20% more recall than normal, from the first day, and continues to hold its advantage throughout the series.

GRAPH IV. PRACTICE CURVES OF RECALL
Number of words recalled per list



In Graph IV, each point on the curves represents an average of 5 sittings for 10 subjects, or 50 measures. This gives us 10 reliable practice units for each condition, from which to judge the trend. It is evident that the higher level gained with tension is consistently maintained. Does this not suggest that probably the tendency in Experiment I for the tension curves of recall to lose their initial advantage was due to the fact that learning time was not held constant?

Summary. The purpose of Experiment II was to determine whether a change from nonsense to sense material would alter the relative levels of tension and normal efficiency, and whether, if the learning time were held constant, we should still get what we did in Experiment I, namely, a shift in favor of the normal condition as a result of practice in recall. The results prove that tension is more efficient as a learning condition for sense as well as nonsense material, and that, with a constant learning time, practice does not alter this situation.

Experiment III

Learning is a very special kind of mental work. This research would be incomplete if our tests were not also applied to other forms of mental activity involving more routine intellectual operations. The last two studies will therefore be devoted to non-learning functions, where the operations involved are assumed to be fairly habitual ones. This not only gives us a more comprehensive treatment of the problem, but at the same time enables us to introduce a new criterion of efficiency, that of accuracy. What is the influence of tension on accuracy of performance?

In Experiment III the mental work consisted of adding columns of 20 digits. Since the average time necessary for the adding of one of these columns was 20 sec., it was possible for the *S*s to add 12 separate columns in one session. Every *S* reported for 10 sessions, 2 a week for 5 weeks, so 120 distinct measures were obtained from each. Eleven *S*s, graduate students in psychology, participated in these experiments.

The columns of digits were constructed as to be comparable in difficulty, yet each different in order from all the others. Zeros and ones were discarded. All the 8 remaining digits appeared in every column. The columns to be added appeared on typewritten cards placed on a raised standard at convenient distance from *S* for clear vision. When the sum was obtained, it was reported orally and checked right or wrong by *E*. The time in seconds was also taken by means of a stop-watch. In this manner a record was obtained regarding two criteria of efficiency, *speed and accuracy of performance.*

The two conditions, tension and normal, were alternated from list to list, and the other additional checks that we described in Experiment II were also used. The directions were given orally. After placing the card containing the figures in front of *S*, *E* said, "Tension; column one; ready; go;" or "Normal; column one; ready; go;" depending upon the condition to be used. At the word "tension," *S* grasped the dynamometers and squeezed them with the appropriate pressure. At the next command he fixated the top of the column, and at the signal "go," added as rapidly and as accurately as possible. If the word were "normal," however, *S* folded his hands in his lap, relaxed, and then the procedure was the same.

Instructions were given at a fixed rhythm and an attempt was made to give them uniformly in the same tone of voice and bearing. Between every two additions, *S* turned his eyes to the window and rested them for 10 sec. This respite enabled *E* to record the time and accuracy.

Since every *S*'s daily record contains 6 successive measures under each condition, it is possible to plot regular work curves for both 'tension' and 'normal,' showing what influence fatigue has upon the relative levels of efficiency of these contrasted conditions. Inasmuch as there were 10 successive work-sessions in the total experiment for both conditions, practice curves can be plotted which will indicate whether tension loses its effectiveness as the task performed becomes automatic.

Results of Experiment III: (A) Absolute ratios between 'tension' and 'normal' scores. The average time required for all the *S*s to add a column of 20 digits under tension was 23.3 sec., under the normal condition, 25.7 sec.; a difference of 2.4 sec. in favor of tension. In other words it took on the average 10% less time to add a column when tension was used. That this is a reliable difference is shown by the fact that it exceeds its probable error by 7.5 times. A glance at the individual records in Table VI shows that there were no exceptions to this rule. But in some cases the difference was so slight as not to be statistically significant. Hence we have indicated in each case the number of times the difference exceeds its probable error. Two cases are on the borderline, and three fall below the statistical requirements.

TABLE VI

Average Number of Seconds per Example

S	Tension	Normal	M_1-M_2	P.E. M_1-M_2	Diff. times P.E.	% Less Time under Tension
Pa	21.4	28.1	6.7	.69	10	24
Wa	14.2	16.9	2.7	.334	8	16
Sh	10.1	12.1	2.0	.25	8	17
Bi	16.7	19.9	3.2	.25	12	17
Br	26.4	28.0	1.6	.613	3	6
Th	19.4	19.7	.3	.48	1	2
Wi	29.1	31.1	2.0	.7	3	7
Gn	28.0	29.0	1.0	.68	1.6	4
Lo	27.7	28.7	1.0	.47	2	4
Yu	31.5	34.0	2.5	.68	4	7
Ho	32.3	34.9	2.6	.92	3	8
Av. N = 660	23.3	25.7	2.4	.321	7.5	10

TABLE VII

Distribution of Time Scores

S	Number of Times Greater Speed is Gained With		
	Tension	Normal	Equal
Pa	50	8	2
Wa	50	7	3
Sh	44	8	8
Bi	52	2	6
Br	34	24	2
Th	31	25	4
Wi	36	21	3
Gn	34	24	2
Lo	34	23	3
Yu	37	21	2
Ho	35	24	1
Av.	39.7	17	3.3

Regarding the consistency with which tension excelled, we have evidence in Table VII, which shows the proportion of times that tension gave superior results, in the case of every *S*.

On the average, out of 100 mean scores, 67% show better results with tension, 28% with normal, and 5% are tied. In the extreme case, that of Bi, tension gave superior results 87% of the time, while normal excelled only 3%. The least successful case, that of Th, gives superior records for tension 10% more times than for normal. For the speed criterion, then, the results point unmistakably to the superiority of the tension condition.

An analysis of the *accuracy* records is given in Table VIII.

TABLE VIII

ACCURACY

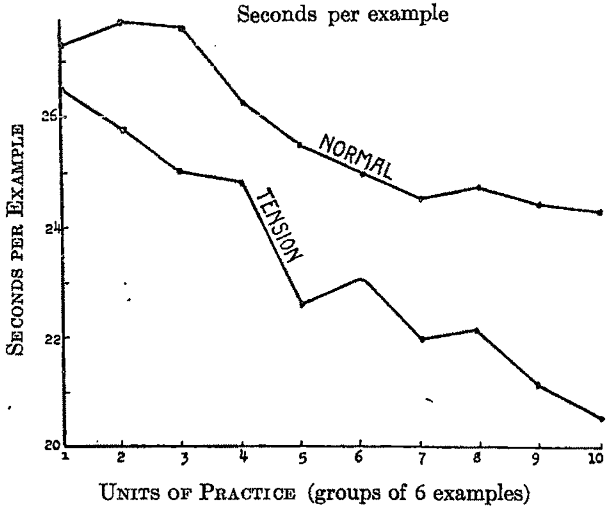
S	*Absolute Ratio Between Normal and Tension Scores* Normal	Tension	Gain	% Gain
Pa	5.4	5.6	.2	4
Wa	5.4	5.8	.4	7
Sh	4.5	5.3	.8	17
Bi	5.8	5.9	.1	2
Br	5.2	5.4	.2	4
Th	4.3	5.2	.9	21
Wi	5.5	5.9	.4	7
Gn	4.7	5.1	.4	9
Lo	5.9	5.8	−.1	−2
Yu	4.3	4.3	0	0
Ho	5.3	5.0	−.3	−6
Average N = 660	5.1	5.4	.3 P.E. of Difference = .026	6

The average daily number of columns correctly added under tension was, for all *S*s, 5.4, as against 5.1 under the normal condition. This means that tension gave 6% greater accuracy; not a large difference but a significant one, since it is over 12 times its probable error. Th, who showed the least difference in speed, showed the most in accuracy, in favor of tension. In general, though, speed and accuracy were neither positively nor negatively correlated, as is shown by applying the Spearman Rank Method to the last columns of Tables VI and VIII. The ρ formula gives a correlation coefficient of $-.16 \pm .19$.

(*B*) *Influence of practice.* In spite of the fact that such a mental function as adding figures is fairly automatic to start with, it is capable of being considerably improved with intensive practice such as that afforded by the present study. The practice curves for the two conditions show a decided drop in the amount of *time* per column between the first and last sessions of the experiment.

In Graph V, the average time per column for all *S*s has been plotted against the successive units of practice, or days, and the resulting trend shows that speed of performance increased in both conditions, but 100% more in the 'tension' than in the 'normal' curve. The points on the curves represent an average of 100 measures, and hence give quite a reliable estimate. Moreover, there is a tendency for the 'normal' curve to round off toward the end, as though approaching an asymptote, whereas the 'tension' curve is steepest toward the end. Evidently, the advantageous effect of tension, instead of decreasing with practice in the function tested, grows more and more pronounced.

Graph V. Practice Curves of Time
Seconds per example



Accuracy presents a different picture. It gives results which are comparable with those for recall efficiency in the last experiment. Practice neither increased nor decreased the initial advantage in favor of tension. Evidently in addition, as in learning, it is the speed rather than the quality of the performance which is increasingly benefited by tension as practice renders it habitual. Any theory explaining the beneficial influence of muscular tension upon thought must take this fact into consideration.

Table IX

Influence of Practice on Accuracy: Average Number of Examples Correct per Day

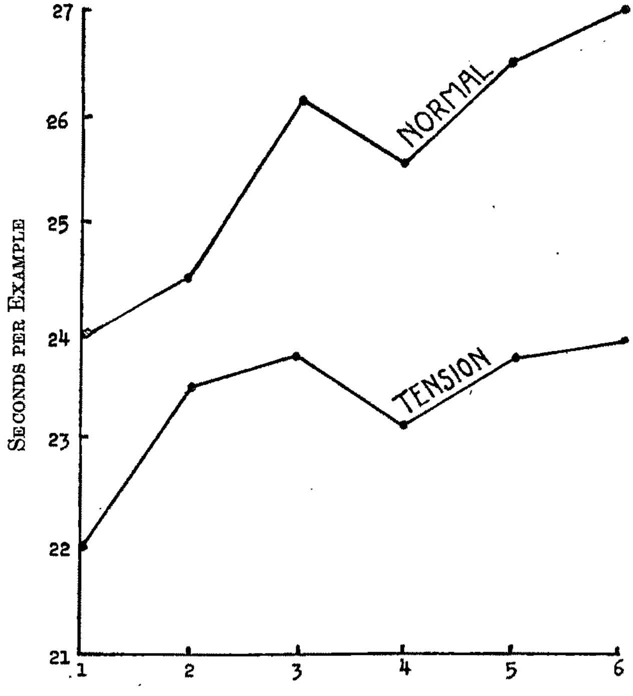
NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Gain in Accuracy	First Half	Second Half	Gain in Accuracy	Greater Gain With Tension	% Greater
5.0	5.2	.2	5.3	5.5	.2	0	0

On the average, there is a slight increase in accuracy for both conditions with practice, but neither condition is more benefited than the other. The accuracy curves for the two conditions run parallel to each other, as did the recall curves in Experiment II.

We may conclude, then, that practice increases the initial superiority of the tension condition, where speed is the criterion, but does not alter it where the criterion is accuracy.

(C) *Influence of fatigue.* The present experiment differs from the preceding in the fact that several measures were obtained at each sitting. This makes possible the plotting of daily work curves for each condition, showing the influence of tension on fatigue. In Graph VI, *time* per column has been plotted against successive units of work, or columns added, during an average work session. Two curves are thus secured, one for tension, and one for normal.

GRAPH VI. WORK CURVES OF TIME
Average number of seconds per example



UNITS OF WORK (examples)

The points on the curves represent an average of 110 measures, since the curves are made by averaging 10 sessions for each of 11 *S*s. The very marked widening which occurs can therefore be relied upon as a true indication of the trend. Whereas the decrement under tension is only 2 sec., that under normal is 3 sec.; moreover, the tension curve shows a tendency to level off, while the other continues steep. The jog in the middle of both curves is accounted for by the fact that at this point the display cards were shifted, and the *S*s got a fresh start.

As in the case of practice, when *accuracy* is the criterion, the results are different. To begin with, in the majority of cases, there was no accuracy decrement under either condition, but rather an increment perhaps due to warming up. Hence we can compare the two conditions only as to amount of increment, to see which shows the greater. This is done in Table X, where the two halves of the work session are compared.

TABLE X

Influence of Fatigue on Accuracy: Average Number of Examples Correctly Done

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Gain in Accuracy	First Half	Second Half	Gain in Accuracy	Greater Gain With Tension	% Greater With
2.4	2.5	.1	2.6	2.7	.1	0	0

It is plain that in general there is no marked difference between the results for the two conditions. This agrees perfectly with the previous results for the accuracy criterion. Neither practice nor fatigue changes the ratio of efficiency between the two conditions, when quality of work is the characteristic measured.

We may conclude, therefore, that the effect of fatigue is to enhance the initial superiority of the tension condition, where speed is the criterion, but that with accuracy as a criterion the ratio is not altered.

Summary. The purpose of Experiment III was to determine the influence of tension on the speed and accuracy of adding columns of digits. The following facts were discovered. (1) Tension improves the absolute speed and accuracy of performance in adding columns of digits. (2) As a result of practice the initial difference of *speed* in favor of the tension condition is enhanced; but the *accuracy* ratio tends to remain constant throughout the practice period. (3) With the onset of fatigue the speed of performance suffers less decrement under tension, but the accuracy ratio tends to remain the same.

EXPERIMENT IV

The purpose of this experiment was to test further the findings of Experiment III with regard to the influence of tension on the decrement from continuous work, or *fatigue*. We wished to determine what influence tension would have on the speed decrement under straight speed instructions, uncomplicated by accuracy, where practice effect had been eliminated. A type of mental work involving rapid perception was chosen, a modification of the color-naming test. The 10 *S*s used had already practiced the tests till improvement from practice was negligible.

Gummed letters, ¼ in. high, were mounted in rows, 10 rows of 10 letters each, on cardboard sheets 12 × 12 in., in haphazard order. Five types of cards were made, depending on whether they contained all possible arrangements of 2, 4, 8, 16, or 24 distinct letters. During a work session, *S* would read one card through at top speed 20 consecutive times. Time was recorded for every repetition of the card, thus giving 20 consecutive measures of speed from which to plot a work curve. The accuracy factor was eliminated from the results by having the *S*s correct mistakes as they read. Mistakes were very rare. Every one of the 10 *S*s was put through all 5 types of card, under both tension and normal conditions, or 10 work sessions. This gave 100 separate work curves in all.

TABLE XI

Absolute Ratio Between Tension and Normal Scores

Average Number of Seconds per Repetition of Card: Time

S	Normal	Tension	M_1-M_2	P.E. M_1-M_2	Difference times P.E.	% less Time under Tens.
Bl	24.6	25.5	−0.9	.28	3 ¼	−3.6
Ey	28.8	28.6	0.2	.213	1	0.7
Ko	30.4	28.9	1.5	.409	3 ¾	5.0
La	26.0	22.6	3.4	.252	13½	13.0
Mc	32.9	32.7	0.2	.267	¾	.06
Od	26.3	24.8	1.5	.33	4½	6.0
Os	27.9	26.7	1.2	.34	3½	4.3
Ri	21.7	19.5	2.2	.23	10	10.0
Ru	27.1	26.0	1.1	.334	3¼	4.0
Th	25.3	25.7	−0.4	.365	1	− 1.6
Av.	27.1	26.1	1.0	.134	7½	4.0

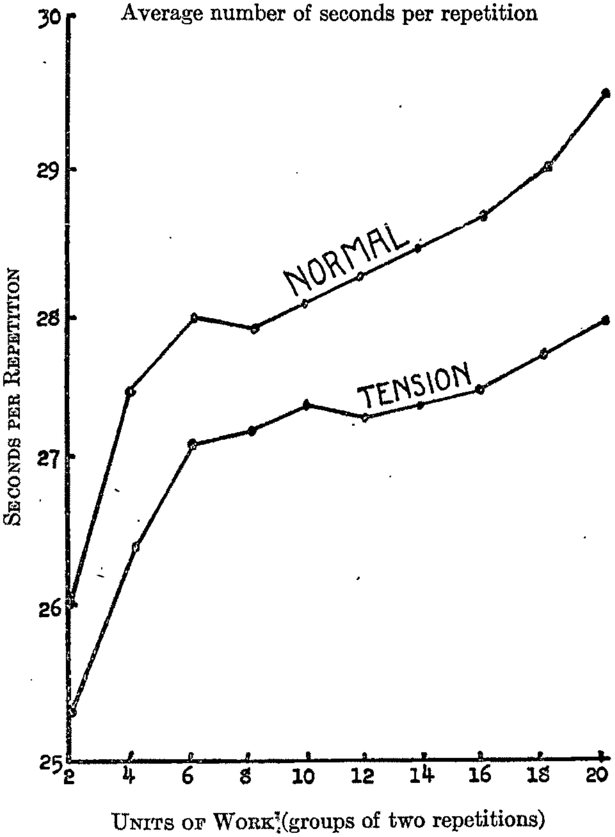
N = 1,000 100 measures for each subject.

Results of Experiment IV: (A) Absolute ratio between tension and normal scores. By finding the average time per repetition for the entire 100 measures secured from every *S* under each condition, we can secure quite a reliable estimate of the difference in efficiency of the two conditions. Table XI gives these, as well as the general averages for all *S*s combined. We find that for all *S*s the mean time per repetition under normal is 27.1 sec.; that under tension 26.1 sec. The difference is 1 sec., or about 4%. Though small, it is quite significant statistically, being 7.5 times its probable error. Perception, then, of the type involved in this task, is speeded up by the use of muscular tension.

(B) *Influence upon fatigue.* The important question is how tension affects the work decrement. Only one type of card gives true decrement curves. With the others, fatigue is concealed by warming up and there is a slight increment. Hence we have divided the results into two parts, the first dealing with the results from the two-letter type of card, where a decrement appeared; the second combining the results of the other 4 types, where an increment appeared.

Graph VII. Work Curves of Time: Two-Letter Combination

Average number of seconds per repetition



(1) The following table shows the average results for the two-letter combinations. There is 36% less decrement under tension.

Table XII

Influence of Fatigue on Speed: Seconds per Repetition of Card for Two-letter Combinations

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Speed Loss	First Half	Second Half	Speed Loss	Smaller Loss With Tension	% Smaller
27.6	28.7	1.1	26.8	27.5	0.7	0.4	36

In Graph VII, on the preceding page, the average work curves for the two conditions are compared, resulting from the two-letter cards only. Considerable decrement appears in both, but that for the normal condition is much greater. Both show a very pronounced initial spurt, which continues only through the first 6 repetitions.

(2) Table XIII shows the average results for the 4, 8, 16, and 24-letter cards. The difference is more clear-cut than for the 2-letter cards. There is 100% greater increment with tension than with the normal condition. It is a safe assumption that tension is exerting a beneficial influence here in counteracting fatigue and allowing warming-up to appear.

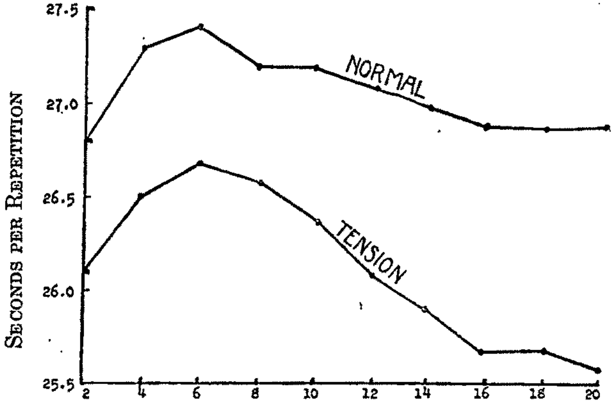
TABLE XIII

Influence of Fatigue on Speed: Seconds per Repetition of Card for Higher-Letter Combination

NORMAL			TENSION			DIFFERENCE	
First Half	Second Half	Speed Gain	First Half	Second Half	Speed Gain	Greater Gain With Tension	% Greater Gain
27.2	26.9	0.3	26.4	25.8	0.6	0.3	100

Graph VIII compares the normal and tension work curves. If the beginning and end are compared, the normal curve shows a slight decrement. The tension curve, on the other hand, shows a slight increment. If the initial spurt is disregarded, however, both show increment, but tension the greater amount by far. Each point on these curves represents a mean of 40 points.

GRAPH VIII. WORK CURVES OF TIME: AVERAGE OF ALL LETTER COMBINATIONS



UNITS OF WORK (groups of two repetitions)

Summary. The results of Experiment IV may be summarized as follows: In perceptive operations of the type here used, involving the continuous recitation of letters in scrambled order, not only does the tension condition give a higher absolute level of efficiency, but it is less susceptible to the effects of fatigue than the normal condition.

General Results and Conclusions

Table XIV is designed to bring together the results of all four experiments with regard to the absolute difference in level between performance under tension and under relaxation. The results are grouped under the three main criteria used, time, recall, and accuracy. The letter 'N' refers to the number of measures from which the average was computed. *Under all criteria of measurement, then, tension proves the superior condition.* The individual scores are in the main consistent. In Experiments II and III every *S*, without exception, gave better

Table XIV

Absolute Ratios Between Normal and Tension Scores

TIME

(Seconds per Unit)

	Normal	Tension	Less with Tension	P.E. of Diff.	% Less with Tension
EXPERIMENT I. (N = 90) Learning Nonsense Syllables	157	137	20	4.32	13
EXPERIMENT III. (N = 660) Adding Columns of Digits.	25.7	23.3	2.4	.32	10
EXPERIMENT IV. (N = 1000) Perception of Letters	27.1	26.1	1.0	.134	4

RECALL

(Items per List)

	Normal	Tension	More with Tension	P.E. of Diff.	%. More with Tension
EXPERIMENT I. (N = 90) Learning Nonsense Syllables	3.9	4.4	.5	.198	13
EXPERIMENT II. (N = 500) Learning Paired Associates	5.3	6.0	.7	.107	13

ACCURACY

(Examples correct per day)

	Normal	Tension	More with Tension	P.E. of Diff.	% More with Tension
EXPERIMENT III. (N = 110) Adding Columns of Digits	5.1	5.4	.3	.026	6

results under tension. In Experiment I, two-thirds did better, and in Experiment IV, 80% excelled under tension. Where individuals failed to conform to the general rule it was probably due to the conditions of the particular experiments, although it is possible that individuals differ in their susceptibility to improvement from increased tension.

Taking into consideration all the facts brought out by this research, the following general conclusions can be drawn.

(1) That muscular tension, of the form and amount used by us, does increase the efficiency of mental work of the kinds tried. The amount differs with individuals.

(2) That the increase in efficiency gained by tension is enhanced with *practice*, where speed is the criterion, but remains constant where other criteria or sets are used.

(3) That the added efficiency gained by using tension tends to increase as the subject grows more *fatigued*, when speed is the criterion, but remains constant where other criteria are used.

THEORETICAL DISCUSSION

The question remains as to the manner in which increased muscular tension exerts its influence upon mental efficiency. We are justified only in outlining the possible explanations and suggesting what bearing our results have upon them. It is probable, but not certain, that the muscular tension normally accompanying intense thinking functions similarly to the tension used in this experiment.

The possible theories are grouped roughly under two heads, depending on whether the influence is thought to be exerted mainly through nutritive, or mainly through neural channels.

(*A*) *Through Nutritive Channels.* Increased activity stimulates various vital processes. Blood supply is altered, breathing changed, glands activated, etc., by muscular exertion. The effect, however, is not necessarily beneficial to thought. The exertion in this experiment accompanied the mental work; hence its effects must have been direct, no time being given for roundabout paths.

(*B*) *Through Neural Channels.* When the sensory-neuromuscular system is already active, it is thought that the threshold is thereby lowered to subsequent neural excitation and discharge. The tension may supply the previous activity. This again assumes antecedence in time. Theories supporting a simultaneous influence of tension on thought favor either an inhibiting or a facilitating effect.

The inhibitory theory seems to have been favored by Wm. James (1890) in his 'draughting off' hypothesis regarding motor concomitants; extra neural excitement is thereby withdrawn from thought channels. Others suggest that it is irrelevant *motor* tendencies which are checked or inhibited by the static innervations accompanying strong attention. Irrelevant sensory stimuli might be excluded similarly. That the inhibitory theory is inadequate in the present study is shown by the fact that our subjects were not subjected to distracting stimuli from without, and that their irrelevant movements during the normal sittings were no more numerous than under tension.

Theories assuming facilitation are more numerous. The fact of a dynamogenic influence of simultaneous processes on one another has long been recognized. It probably accounts for the frequent cases where so-called distractors act as facilitators, and for all cases of summation of stimuli. The tensions used in this experiment may have functioned by setting up a flow of afferent impulses from proprioceptive organs in the muscles, better calculated to act as facilitators than the sudden, spasmodic distractors coming from without the individual.

Again, these tensions might exert a dynamogenic effect in the more indirect way suggested by Morgan, who found that external distractors acted as obstacles which, in the effort to overcome them, increased the speed and accuracy of mental operations, but did so at the expenditure of greater effort. Yet this should give more pronounced fatigue effects than the normal, and in the present study the decrement from fatigue was reduced.

If tension acted merely as a distractor, there is reason to suppose that its influence, like that of other continuous distractors, would gradually disappear by reason of adaptation.

The fact that reaction time in thinking, or speed, was most consistently affected by tension, seems to suggest that it was the muscular preparation involved which directly accelerated the responses. This effect would gradually increase as the tension became automatic through practice, which actually happened. This might explain why speed was influenced in a different manner from accuracy and recall efficiency.

It is probable that tension produced its effects in different ways at different stages in the process, or in several ways at once. Further experiment should be directed toward a qualitative analysis of the effects of muscular tension on thinking.

Because of the light which the study throws upon the rôle of muscular innervations in energizing and controlling thought, it paves the way for an objective treatment of the mechanism of 'will,' or of 'purposeful,' directed thinking. It offers an objective test of Washburn's hypothesis (1916) that "the motor innervations underlying the 'consciousness of effort' are not mere accompaniments of directed thought, but an essential part of the cause of directed thought."

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THE ACCURACY OF THE METHOD OF CONSTANT STIMULI

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Culler has recently published a series of articles which are very suggestive and give great promise for the future work of their author.[1] Some of his discussions, however, are marred by errors and mistakes which must not remain uncorrected. One of them is his determination of the probable error in the method of constant stimuli.

Culler proposes to find the probable error of both h and the limen directly from h itself. He conceives h as the measure of precision of the differential curve whose integral is fitted to the observed relative frequencies, and puts the probable error of h proportional to this quantity itself. This idea is clearly erroneous; h is derived from observations and its accuracy must be determined from the same data. This is done by comparing the observed results with those obtained by calculations. No formula which does not contain the sum of the squares of the deviations can be correct.

Culler, furthermore, misses the main point of my argument. He calculates the probable errors of h and c, but does not determine the probable error of the interval of uncertainty, as I do. My paper in the *Archiv* gives very detailed directions for finding the probable errors of both h and c.[2] My formulae are just an adaptation of the method of least squares to the problem of the method of constant stimuli. If you calculate h and c by least squares, you have no choice but to follow the same rules for the determination of the accuracy of the values found. No other procedure is logically consistent.

The constants of the psychometric functions are found with a fair degree of accuracy. Culler stops at the probable errors of these quantities and does not go any further. He forgets that h and c are not of immediate interest, but serve for the determination of the limen only. Here it is where the trouble lies. The limen is found by dividing c by h, and this process gives a very inexact result. The interval of uncertainty is the difference of the upper and the lower limen, and this subtraction further reduces the accuracy, roughly speaking, by one-third.

Calculating the interval of uncertainty by the $\Phi(\gamma)$-hypothesis is a roundabout process and the accuracy of the final result is very small indeed. This fact does not appear, if one considers the probable errors of h and c only without determining the resulting probable error of the interval of uncertainty. The errors of h and c combine in such a way as to produce a surprisingly large probable error in the interval of uncertainty. Table 68 of my paper in the *Archiv* gives the probable errors of the interval of uncertainty for the seven subjects of my lifted weights experiments.[3] The sums of the squares of the deviations are also given in this table, but the probable errors of h and c, being auxiliary quantities only, are not recorded. They are easily found in the way indicated in my article.[4] The probable errors of h and c in the example discussed by Thomson and Culler are 0.0021 and 0.64 respectively.

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[1] Elmer Culler, Studies in psychometric theory, *Psychol. Mon.*, 35, 1926, 56-137. The article in question is: VI. The probable error of the limen and its derivation.

[2] F. M. Urban, Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, *Arch. f. d. ges. Psychol.*, 15, 1909, 261-355; 16, 1910, 168-227.

The decisive criterion for the reliability of any method is the probable error of the final result, in this case of the interval of uncertainty. The data of the table fully bear out my statement that calculation by the $\Phi(\gamma)$-hypothesis, *i.e.* the method of constant stimuli, is the least satisfactory of the four methods investigated. Observation or calculation by the method of just perceptible differences or calculation by Lagrange's formula give much more accurate results. Culler does not do justice to my argument, when he asks how the $\Phi(\gamma)$-hypothesis can be less exact than the arctan-hypothesis. I did not calculate the probable error of the interval of uncertainty by this latter hypothesis and, therefore, could not pronounce on the accuracy of this process. I merely showed that this hypothesis does not fit my observations as well as the $\Phi(\gamma)$-hypothesis and left it out of my further considerations.[5]

Culler then compares his results with those obtained by Thomson. This is a mistake if it is meant that these data are different from mine. The point of Thomson's criticism was settled five or six years ago. Thomson did not work out his suggestion; for, if it is consistently carried out, it leads back to the method of least squares and, therefore, to my own formulae. There is no real difference between his results and mine, provided that one does not stop halfway, as Thomson did.

My experience with this intricate problem of the accuracy of the method of constant stimuli is very curious. I started out with the idea that this method was the best of all. Its superiority in regard to experimental procedure is so marked that no experimenter can fail to be impressed by it. Is the same true in regard to the algebraic process involved, or is it at least as reliable as other processes? There exists just one way to decide this question: produce an experimental material which may be worked out by the different methods and find the probable errors of the results. If you want to judge the accuracy of algebraic processes, you must apply them to the same material, for otherwise you are never sure that the better result may not be due to the superiority of the experimental material. This was done in my paper on psychophysical methods and my disappointment was great, when the results were so unfavorable to the method of constant stimuli.

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[3] *Op. cit.*, 226. [4] *Ibid.*, 186 ff.

[5] I seize this opportunity to indicate a misprint in the last formula of my article (op. cit., 226). The second term in the bracket under the rootsign should read b_2S^2.

At first I suspected a flaw in my argument or an error of computation, and did the work over and over again. Before proposing this result, which was so startling to me, I wanted to be sure that my argument contained no avoidable error. This is one of the reasons why my monograph on statistical methods in psychophysics,[6] which was brought out under the auspices of the University of Pennsylvania, does not contain the theory of the $\Phi(\gamma)$-hypothesis. Not being able to find the fault, I decided to publish my results such as they were, hoping that one of my reviewers would readily point out to me where the error is. In order to arouse contradiction I formulated the result rather more strongly than warranted by proclaiming the method of constant stimuli the least exact of all.

My hopes did not materialize for, with one exception, none of my reviewers thought it worth while to mention this point. The one who proved the exception, was not particularly competent to judge such delicate questions and contented himself by printing the conclusion and putting an exclamation sign in brackets at the end of the sentence. I felt inclined to let the reviewer know that I shared his sentiments and would welcome an answer. At that time I would have been very glad to be shown that my deduction contained an error, for my results confront us with the puzzling question, how it comes that the method of constant stimuli is so disappointingly inexact in its final result.

Things did not move for several years and I decided to look for help, since nobody offered voluntary assistance. At this time Thomson began the long line of his brilliant contributions to psychophysics and he looked to me like the providential man who might help me out of my quandary. So I wrote to him, explaining my difficulty and asking him to look into this matter. I mentioned my suspicion that the determination of the probable error in the method of constant stimuli contained an error. This letter, if I remember correctly, was written in the winter or spring of 1914.

This was a very good idea, although Thomson's criticism was, perhaps, undeservedly hard on my efforts, even if it had been entirely justified, which it was not. Thomson set himself to work and finished in a comparatively short time a very remarkable paper on this problem. Any reader of his article must be struck by the correctness and originality of his starting point, and by his obvious failure to arrive at a satisfactory result. His formulae are forbidding, and he evidently was frightened by them, for he devised all kinds of shortcuts and approximations to make them manageable at all. The outcome, of course, is that his final results are not much better than mere guesses.

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[6] The application of statistical methods to the problems of psychophysics, 1908.

That paper placed me in a difficult position. I clearly saw the merit of Thomson's idea. On the other hand, he had not shown me an error in my argument, but merely proposed another starting-point. It did not necessarily follow that I was wrong if Thomson was right. It also was plain to me that the numerical work would have to be arranged in an entirely different way, if his suggestion was to be taken seriously.

There were two alternatives before me. The first was to do the numerical work seriously, without shirking any computation no matter how laborious it might be. This work would be tedious and not very conclusive. Supposing that I obtained the same results by Thomson's formula and by my own, this coincidence would appear accidental.

The other alternative was to work Thomson's suggestion out to its logical end. My formulae are derived by least squares and are right, provided that no error crept into my deduction. The method of least squares contains no errors or contradictions, for the combined efforts of so many investigators would have brought them to light during the century which has elapsed since its invention. Supposing that Thomson's suggestion is right, it ought to be possible to deduce from it the well-known formulae for the probable error in the method of least squares. I saw that this work would be unusually hard; but I did not realise how difficult it would be, for otherwise I might not have ventured upon it.

Several lucky strikes helped me along and by 1920 I was in the possession of the complete solution, which is presented in my book *Die Grundlagen der Wahrscheinlichkeitsrechnung und der Theorie der Beobachtungsfehler.*[7] All the formulae for probable errors in least squares are worked out by Thomson's suggestion. His idea is right, but he did not work it out. His suggestion leads to my own formulae and his criticism is unjustified. Knowing the difficulties which are encountered on this way, I am the last to blame Thomson for not having gone to the end, and I am glad that my letter elicited such a splendid idea.

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[7] Pp. 223-238.

I communicated my results to Thomson, but not being fond of public controversy I did not answer him in print. This was a mistake. I had no right to assume that all workers in psychophysics would read my book on the calculus of probabilities. The book was primarily intended for mathematicians, so no reference was specifically made to psychophysical questions. It requires great insight and a very detailed acquaintance with the actual state of the problem to recognise that the deductions in my book are intended as a refutation of Thomson's criticism. One must not blame Culler for not knowing that my formulae and those of Thomson are essentially identical. I am sorry that my failure to answer publicly induced this gifted author to look for an error, where probably there is none and to use methods which are inadequate to this problem. My only excuse is that my answer would have come several years late, and after the war it looked futile to dig up old controversies.

The present situation, as I view it, is this. My presentation of the method of constant stimuli is in agreement with the ~~method of least squares. This regards not only my~~ determination of the probable error but also, as I may remark incidentally, the formula for the weight of the observation equations, with which Culler also finds fault. There exists just one way to refute my formulae: it must be shown that my deduction is in contradiction with one of the rules of least squares. None of my reviewers has done this. It is not sufficient to propose some new starting point and derive other fromulae; for if the new idea is in agreement with the method of least squares, it leads back to my own formulae, and if it is not, it must be thrown out of court at once.

At present I no longer hope that an error will be found in my determination of the probable error, which is rather a pity. It would be an easy matter to set the calculations right; but it is not easy to find the reason of the small accuracy of the method of constant stimuli. Neither is it easy to make up one's mind about the real standing of this method, which is from the point of view of the experimentalist, the best of all.

IS THE QUESTION OF ANALYSIS OF SOUND BY RESONANCE IN THE COCHLEA OR BY 'CENTRAL ANALYSIS' IN THE BRAIN STILL AN OPEN ONE?[1]

By G. WILKINSON, Sheffield, England

In a recent article in this JOURNAL Edwin G. Boring reviews the experimental evidence bearing on the theory of hearing.[2] He states his view that the question of the locus of tone-analysis is still open, but gives his vote for analysis by the brain according to frequency, rather than analysis in the cochlea according to pitch level through the mechanism of resonance. The present writer is a convinced adherent of the resonance theory, and to him it seems that the position taken by Boring is retrograde, abandoning, as it does, to the sea of doubt ground that has been laboriously reclaimed from the waves for physiology, and which is in process of being consolidated, with the prospect of its being rendered fertile. He is more concerned, therefore, to vindicate the resonance theory from doubts that have been cast upon it, than to criticise in detail the theory that has been put forward by Boring. To him it seems that Boring has omitted from his purview many weighty considerations in favor of the resonance theory, and that some of the arguments he advances as against that theory are open to question.

The novelty of Boring's thesis consists in his making perception of intensity the starting point. He sets out to formulate a comprehensive theory of intensity, leaving the other factors of sound perception to fit in as best they may. As he states this is a reversal of the usual procedure of starting from pitch perception, and then filling in the other factors—perception of intensity, direction, volume. There is this to be said for the latter method that the specific function of the auditory mechanism is to perceive sounds, *i.e.* tones, either simple or combined, whilst all the sense organs are concerned with the perception of intensity, volume, and (with the possible exception of smell) of localisation. It is natural in investigating a sense organ to pay special attention to the function which it alone fulfils. In the case of the ear, it may be supposed that the general plan of the organ, and the special mechanical features in which it differs from other sense organs will be correlated with the particular function of perceiving tones. Still, it is obvious, if we can discover the true method by which the organ performs one of its functions, it will in no way prevent, and may help, the explanation of its other functions.

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[1] Manuscript received for publication, August 25, 1926.

[2] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 157-188.

Let us start then, as Boring has done, with perception of intensity. Boring's conception of the mechanism of the cochlea is that the disturbance due to the impact of waves of sound starts at the proximal or basal end of the basilar membrane, and that the extent to which the movement is transmitted along the membrane depends on the intensity of the impulse, the more intense disturbance travelling further towards the apical end than the less intense. His theory has a resemblance to the 'travelling bulge' theories of M. Meyer, Hurst, ter Kuile, and Watt, the difference being that he correlates the linear dimensions of the 'bulge' with intensity, and not with the duration of a single wave impulse, as do the latter theories. The older 'travelling bulge' theories gave a plausible solution of tone perception, but entirely failed to explain tone analysis. Similarly, Boring's theory might explain intensity perception, but could not explain intensity analysis. Boring states that "fusion of intensities is the most intimate kind of fusion known" but this is only true of intensities of the same pitch. Intensities of sounds of different pitch do not fuse to any considerable extent. Masking of sounds of one pitch by more intense sounds of another pitch occurs, but their fusion is not a recognised phenomenon. However many musical sounds we hear together we recognise that they have their relative intensities, as well as their relative pitches. The conductor of an orchestra immediately detects whether one or other of the instruments is playing too loudly, or not loud enough. The different sounds are perceived as separate entities each with its proper character (*i.e.* pitch distribution) and magnitude (*i.e.* intensity). This is difficult to explain except on the supposition of a discrete stimulation of the sense organ at different levels and with different amplitudes of movement at these levels.

The recognition of any difficulty in the interpretation of perception of intensity is only of recent origin, and is due to the formulation of the 'all or nothing' doctrine of nerve impulses. Before the work of Adrian and Keith Lucas appeared it was assumed that intensity was related in the simplest manner to degree of stimulation, or in other words depended on the amplitude of the displacement of the receptor organs. The 'all or nothing law' states that, if a stimulus to a receptor organ rises above a certain minimum (or limen), it will cause a discharge of energy to be propagated along the nerve fibre in connection with the receptor, and that the strength of the discharge is independent of any further intensity the stimulus may have beyond the minimum. The original experimental work on which the theory was based was concerned with motor nerves only. As there is no obvious difference in the structure, the chemical, and electrical reactions of motor and sensory nerves, there seemed no reason why the same law should not hold good for sensory as well as for motor nerves. If this were so, how could the perception of intensity be accounted for? There appeared to be two possible explanations: either that for each increment of intensity an additional nerve fibre must be stimulated, or that more stimuli must be transmitted along the nerve fibres implicated in unit time. Boring adopts the first of these alternative views, and makes it the basis of the theory he has tentatively put forward. The second explanation was elaborated on theoretical grounds by Forbes and Gregg in 1916.[3] Recently Adrian has published experimental work on the current of action in sensory nerve fibres, which demonstrates conclusively that increase of intensity of the stimulus causes increase in the rate of rhythmic discharge of afferent nerve impulses.[4] "There is no evidence that an increase in the stimulus causes an increase in the size of the action current in single fibres, but the *frequency of the impulses* in the nerve trunk increases."[5] Thus we owe the extension of the 'all or nothing law' to sensory nerves to the original exponent of that law. This is a most notable advance, which will clear the ground of much unfounded speculation.

Adrian demonstrates that a constant stimulus applied *through the receptor organs* to a sensory nerve gives rise to a rhythmic discharge of afferent impulses, and that the frequency of the rhythm varies with the intensity of the stimulus. No doubt we may expect further communications working out these conclusions in greater detail, but the experimental results appear to be quite convincing. They even give us a hint as to the meaning of Fechner's logarithmic law of intensity of sensation. A study of the records reproduced in Adrian's paper shows an approximation of the number of nerve impulses to the log of the intensity of the stimulus. In the case of the current of action in the proprioceptor nerve fibres from the frog's gastrocnemius muscle, excited by stretching of the muscle, a weight of 5 grams gave a frequency of 120 impulses per sec., and a weight of 50 grams gave 310.[6] In another observation a weight of 10 grams gave approximately 180 per sec. and one of 100 grams gave approximately 360.[7]

An increase in the stimulus causes an increased frequency up to limiting value (corresponding no doubt to the absolute refractory period). The limit in the case of a frog's gastrocnemius-sciatic preparation was found to be 400 per sec. In the case of the cutaneous nerves of a decerebrate cat at a temperature of 25°C the maximum number was 420, and in the afferent fibres of the vagus from the expanded lung, 450.

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[3] A. Forbes and A. Gregg, Electrical studies in mammalian reflexes, *Amer. J. Physiol.*, 39, 1916, 234.

[4] E. D. Adrian, Impulses produced by sensory nerve endings, *J. Physiol.*, 61, 1926, 46-72. [Since Mr. Wilkinson wrote this article, there have been two other papers which further support the 'intensity-frequency' theory and the author's view: Adrian and Y Zotterman, *idem*, Pt. 2, Response of a single end organ, *ibid.*, 151-171; *idem*, Pt. 3, Impulses set up by touch and pressure, *ibid.*, 465-483.—Ed.]

[5] *Ibid.*, 72. [6] *Op. cit.*, 65. [7] *Op. cit.*, fig. 3, C and E.

It seems to the writer that this 'intensity-frequency' law of Adrian's must dispose once and for all of the theory of 'central analysis' of pitch, or 'pitch-frequency' theory, as Boring calls it. Indeed, this theory had already worn sufficiently thin by reason of the recognition of the latent period of nerve stimuli before the recent work of Adrian shivered it to pieces. It could only be maintained on the basis of a number of arbitrary assumptions for which there was no particle of positive evidence, such as, for instance, that the refractory period in the auditory nerve might be much less (by some 60 times) than that of any of the nerves for which the period had been experimentally determined, and that frequencies up to 20,000 per sec. may not only pass the nerve unchanged, but also through the various cells and synapses that lie in the tract between the receptors and the cortex.[8] The maximum number of nerve impulses found in Adrian's experiments is 450. However, the question of the maximum number of impulses which can be transmitted by the auditory nerve has lost its significance so far as pitch perception is concerned, for if number of impulses means intensity it cannot at the same time mean pitch. On the 'frequency-intensity' basis, the number of impulses travelling to the cortex can bear no constant relation to the *number* of vibrations acting on the receptors. We are left, therefore, with relative position of the particular receptors on the basilar scale as the only possible criterion of pitch.

Incidentally, it may be remarked that the 'intensity-frequency' hypothesis offers a satisfactory explanation in the much more difficult case of visual perception. Such a problem as the appreciation of the intensity of light in an evenly illuminated field could not possibly be explained on the assumption that an additional nerve fibre is excited for each added degree of intensity of stimulus, but is readily explained by an increase in the number of nerve impulses transmitted.

What is the number of 'quanta' of intensity for a tone of single pitch? Intensity perception follows the Weber-Fechner law, but estimates of the magnitude of the steps vary somewhat widely. Knudsen[9] gives a 5% ratio; *i.e.* the least perceptable increase is 5% of the previous intensity. Fletcher[10] states that for intensities greater than 10^4 times that of the threshold value the ratio has a constant value of 10%. Fletcher and Wegel[11] give the maximum range of loudness of audible sounds (for tones from 512 to 2048 d. v.) as varying in different individuals from 1 to 10^6 to 1 to 10^7, *i.e.* the maximum intensity of sound over these regions of pitch is from one to 10 million times greater than minimum audibility. (Intensity in this case is measured by pressure change in the sound wave impinging on the tympanic membrane.) Maximum intensity is defined as the point at which sensation of sound merges into sensation of feeling. At intensities of stimulation slightly greater than the 'maximum' the sensation begins to be painful. Knudsen's basis of 5% steps, extending over a range of 1 to 10 millions gives us 663 quanta of intensity. The 10% ratio of Fletcher would give us 338 quanta for each distinguishable pitch within the range 512 to 2048 d.v. In any case the number is large. It may well be that future work on the rate of rhythm of impulse in sensory nerves will show that the number required is in excess of the 'quanta' which can be transmitted by a single nerve fibre. This, however, would create no real difficulty for the 'pitch-localisation' theory, as it cannot possibly be maintained that the movement of the basilar membrane giving rise to a sensation of a pure tone is confined to a single receptor. No exponent of the resonance theory (so far as is known to the writer) has assumed this exact localisation of resonant vibration; certainly not Helmholtz, whose celebrated experiments on 'shakes' had for their object the determination of the 'spread of resonance' along the basilar membrane in response to a pure tone. The loudness of the sound heard would presumably be determined by the sum of the 'quanta' transmitted by the nerves connected with the receptors involved, but a greater number of 'quanta' would be transmitted by the fibres connected with the receptors at the maximum point than by those connected with receptors further removed from the center of the disturbance, and our judgment of pitch would be determined by the position on the basilar scale of this maximum point. This is in accordance with Gray's 'principle of maximum stimulation'[12] which he derives from analogy with the sense of touch. As Boring has invoked this principle in support of his views on localisation,[13] he cannot deny its validity as applied to the determination of pitch locus.

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[8] Boring, *op. cit.*, 181.

[9] V. O. Knudsen, *Phys. Rev.*, 19, 1922, 261. Cf. Fowler and Wegel, Audiometric methods, *Western Electric Co.'s Reprints*, 1922.

[10] Harvey Fletcher, Physical measurements of audition, *J. Franklin Instit.*, 196, 1923, 295.

[11] H. Fletcher and R. L. Wegel, Frequency sensitivity of normal ears, *Proc. Nat. Acad. Sci.*, 8, 1922, 5 f.

What is the number of receptors in the cochlea? There seems to be considerable difference of opinion as to what constitutes a unit receptor in the cochlea. Some writers have reckoned the number of head plates of the arches of Corti; others have counted the number of nerve fibres going to the organ of Corti, as indicating the number of separate receptors. To the writer it seems that the unit receptor must be a hair cell. Retzius estimated the number of outer head plates of the pillars of Corti at 3848. There are, on an average, rather fewer than three hair cells to each outer head plate, say 10,000 in all. These are so arranged within the organ of Corti that those lying furthest outwards receive the greatest amplitude of vibration communicated by the fibres of the basilar membrane underlying them. The outer hair cells will be the ones in which the stimulus will first rise above the threshold value. Consequently, in an area vibrating in response to a pure tone, the 'quanta' will be clustered around the maximum point of disturbance, both by reason of the greater number of receptors stimulated at that point, and the greater number of 'quanta' transmitted by the individual nerve fibres connected with it. In the periphery of the vibrating area only the outer receptors will be stimulated, and the number of 'quanta' transmitted by them will diminish according to their distance from the center of the disturbance. This will serve to accentuate the 'maximum' point. If this line of reasoning is correct, it follows that the application of the 'all or nothing' doctrine to sense perception in the cochlea creates no embarrassment for the resonance theory, but rather gives it added support. Its position remains unshaken. The theory gives a reasonable explanation of all the facts of hearing, and is not in demonstrable disharmony with any of the known facts. The same cannot be said of any of the other theories which have been put forward.

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[12] G. Wilkinson and A. A. Gray, *The mechanism of the cochlea*, 1924, 192.
[13] Boring, *op. cit.*, 168.

But the positive evidence in its support must carry more weight than merely negative evidence, and to this positive evidence Boring has not, in the opinion of the writer, done full justice. It would exceed the due limits of this paper to analyse this evidence in detail, but the main points may be briefly summarised.

(1) The point of evidence which appeals most strongly to the writer is the fact of the regular graduation of the basilar fibres as to length, tension, and mass. The fibres increase in *length* progressively from the basal to the apical end of the cochlear scale. The spiral ligament, which attaches the fibres to the outer wall of the cochlear galleries, is graduated in the reverse direction, being very slender and delicate at the apical end, but dense and bulky at the basal end. The fibrils of which it is composed radiate fanwise from their attachment to the basilar fibres, and are disposed so as to give the impression that they are exercising tension on those fibres. Moreover, when cut along its length, the spiral ligament is seen to be composed of triangular leaves of tissue, corresponding in distribution to the individual basilar fibres to which they are attached. There can be little doubt that the spiral ligament is the organ of tension of the cochlea, and that the *tensions* on the basilar fibres are greatest at the basal and least at the apical end, and that they

are regularly and progressively graduated throughout the scale. Thus, the basilar fibres are differentiated by length and tension in the same sense so far as their tuning to respond to vibrations of different periods is concerned. Those that are shortest are tightest, and those that are longest are least stretched. The fluid which fills the cochlea acts as a 'load' on the basilar membrane, since, at whatever level the membrane is vibrating, the fluid between the round and oval windows and the vibrating sector must also be oscillating synchronously. Thus the fluid load is graduated proportionately to the distance of the vibrating sector from the round and oval windows. It is greatest on the apical sectors, and least on the basal, and diminishes progressively from the distal to the proximal end of the scale. The graduation of the sectors for mass is thus in the same sense as that for tension and length, so far as the differentiation of their periodicities of vibration is concerned. In fact, we have exactly the kind of differentiation we should look for in a set of strings tuned to vibrate to tones extending over a wide range of pitch.

(2) Yoshii's[14] classical experiment on the effect of prolonged overstimulation by tones of a single pitch show that the resulting damage to Corti's organ, and to the ganglion cells connected with it, is localised at levels of the cochlea which presumably correspond to the receptors of the pitches employed. This is a very strong argument for the 'pitch-level' as opposed to the 'pitch-frequency' theory. The criticism that has often been put forward that the resulting damage was more widespread than would be expected on the assumption of the validity of the resonance theory was met by Yoshii by the contention that the stimulus applied to cause the damage was essentially a forcing one, and also that the tones employed could not be regarded at pure tones of a single pitch.[15]

(3) The compliment of Yoshii's experimental investigations is to be found in the clinical investigations of Ritchie

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[14] U. Yoshii, Experimentelle Untersuchungen über die Schädigung des Gehörorgans durch Schalleinwirkung, *Zsch. f. Ohrenhk.*, 58, 1909, 201-250.

[15] Boring states, "Yoshii himself concludes that the region affected is too broad for Helmholtz's theory and falls back on Ewald's" (*loc. cit.*, p. 184). I am unable to trace the reference on which this statement is based. Yoshii says, "The result of experimental investigation by continuous tones gives a further new strong support for the well known but hitherto not fully received resonance theory of Helmholtz. It should especially be noted that in each experiment the cochlea is damaged in one and indeed quite definite streak, corresponding to the height of the pipe used. By the experiment with pipe c^5 constantly the upper half of the basal spiral was damaged. With h^2 the effect was shifted a half to a whole winding higher, and with g 1½ turns higher. Consequently the higher the source of sound is the deeper lies the damage in the cochlear scale" (244). Yoshii criticises Ewald's acoustic camera adversely on the grounds, that the membrane in the camera does not conform to pattern, as its tension is not wholly transverse (245). He might have added that Ewald's membrane misrepresents the basilar membrane in every particular. It is not differentiated for width at different levels. Its tension is not regularly differentiated. The membrane is formed by painting a solution of rubber over the slit in the plate, and allowing it to dry. Such a membrane would certainly possess some tension given to it in the process of drying, but such tension would be fortuitous and irregular in distribution. It would not be regularly graduated as is the tension of the basilar membrane. Lastly, the 'oval' and 'round windows' of the camera are not disposed in the model with the same relation to the membrane as are those of the cochlea, and the loading by fluid columns would be entirely different.

Rodger[16] on occupation deafness. He found that the functional damage to the organ of hearing was confined to, or at all events affected earliest and most markedly, the range of tones predominating in the noises to which the patient was subjected in his daily work. Yoshii[17] quotes Putelli as having made similar observations.

(4) Recently a still further confirmation of the 'pitch-level' theory along the same lines has been brought forward by Mollie Weinberg and F. Allen.[18] They investigated the effects of fatigue due to stimulation of the ear by pure tones. They found that fatigue effect was maximum for tones whose pitch was in exact agreement with the fatiguing tone. When the difference between the fatiguing tone and the test tone is only 8 d. v. per sec. the fatigue effect is much less marked, and evanescent. When the ear is fatigued by two or more tones simultaneously, the results are suitably expressed by superposing two or more fatigue curves on the same graph.

It would appear as though the question of the existence of discrete pitch levels in the cochlea had been subjected to every possible test. Excessive monotonal stimulation in animals produces localised disorganisation of the cochlea at levels corresponding to the pitches employed (Yoshii). Less violent but more prolonged stimulation in man from occupational noises produces deafness for the tones corresponding in pitch to the noises to which the individual has been subjected (Rodger and Putelli). Overstimulation of shorter duration produces fatigue effects narrowly restricted to the tones employed (Weinberg and Allen). How could these results be explained otherwise than by the supposition that tones of one pitch affect certain receptors, whilst tones of another pitch affect other receptors? The experiments of Yoshii demonstrate further that these receptors are arranged in a progressively graduated series along the cochlear scale.

Of physical tests demonstrating the similarity in the behaviour of the ear to other forms of resonators we have a large number, all pointing to the same conclusion. Of these one may mention the experiments of H. Hartridge[19] as being specially ingenious and apposite. He employed a siren so arranged that an instantaneous change of phase of half a wave length in the successive pulses emitted by it could be produced. The effect of the change of phase was a short period of silence interrupting the flow of the tone. The deduction drawn is that the change of phase necessitated the damping out of the resonant vibrations which had previously been set up within the cochlea, and the reëstablishment of a new series of vibrations.

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[16] Ritchie Rodger, Boiler-makers' deafness, *J. Laryng.*, 1923, 66.

[17] Yoshii, *loc. cit.*

[18] M. Weinberg and F. Allen, *Phil. Mag.*, Jan., 1924, 50-62, 126-141.

[19] H. Hartridge, A vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 12, 1921, 142-146.

The resonance theory alone offers a reasonable explanation of the main facts of audition. No other theory which has been put forward has any explanation to offer of the analysis of compound into simple tones, or the fact that alteration of the phase relationship of simultaneous tones, though modifying profoundly the form of the compound wave, produces no alteration in the quality of the sound heard. The occurrence of isolated pitch ranges of hearing in the congenitally deaf, and of isolated pitch ranges of loss of hearing (relative or absolute) in other cases of deafness, also point to the discontinuity of the receptors and of the associated nerve tracts concerned with the transmission of one pitch from those concerned with neighbouring pitches.

One may say that all the positive evidence bearing on the mechanism of hearing is in favour of the resonance theory. The only positive support which any alternative theory can claim is that furnished by Ewald's camera in favour of the 'pressure pattern' theory. The writer's contention is that this support is entirely fallacious, seeing that Ewald's apparatus entirely misrepresents the physical conditions present in the cochlea.

For the rest, it may be said that the alternative theories —the 'telephone' theory, the 'pressure pattern' theory, and the 'travelling bulge' theory—have had their origin merely in attempts to evade certain difficulties (real or imaginary) in the resonance interpretation. There is not a particle of positive evidence in favour of any of them. The mechanical possibility of the cochlea acting as a simple recorder of wave forms (as supposed by the 'telephone' theory) or being thrown into different patterns by simple tones of different pitch (as according to the 'pressure pattern' theory) or transmitting wave disturbances to varying distances (as in the 'travelling bulge' theory) has never been demonstrated. These theories are purely speculative. Whatever evidence we possess is against these possibilities. With regard to 'central analysis' of pitch frequencies recent physiological research is more and more strongly against the possibility of such a supposition.

On the other hand, possibly none of the positive evidence in favour of the resonance theory is so absolutely conclusive that exception to it cannot be taken, but the accumulation of evidence all pointing to the same conclusion, viz. that the cochlea discriminates pitches by localised vibration and that the localised vibrations are disposed in a regular progressive series, constitutes a very strong case for the resonance theory.

THE COURSE OF EXPERIENCE[1]

By CHARLES A. DICKINSON, University of Maine

The results of a previous study indicated that it might be possible to secure an adequate description of the initial moment of visual experience,[2] that the transition from one item to another in a visual stimulus-field had not been fully described, and that contour appeared to play an important rôle in the accrual of logical meaning. With these points in mind the present investigation was undertaken.

Technique. The technique is very similar to that of the earlier experiment, consequently a detailed description of the apparatus and procedure is here not necessary.[3] The following differences, though unimportant, must, however, be reported. A Whipple tachistoscope was used in place of one of the pendulum disk type. This, adjusted so the lever arm made a complete revolution at every release, was used for the exposure apparatus. The operation of this apparatus was noiseless and offered no distraction to the *Os*. The separation of the tachistoscope from the ground-glass exposure field was also slightly different. The tachistoscope was located 123 cm. behind the ground-glass field; the projecting end of the reflectoscope was 5 cm. to the rear of the tachistoscope. The *Os* were seated at a distance of 144 cm. in front of the field. The time of exposure was 60σ.

Observers. The *Os* were J. P. Guilford (G), assistant in psychology; Dr. Harry Helson (H), instructor in psychology; Max Meenes (M), Sage fellow in psychology; and Dr. Elizabeth F. Möller (Ml), honorary fellow in psychology. All were trained in the method of introspection.

Stimuli. Letters, numerals, and playing card forms (both red and black forms of every suit—red as well as black spades, and black as well as red hearts, etc.), were used as stimuli. These materials were spaced equally in rows of three on ruled paper exposure fields. The playing card forms were used as individual items. This uniform arrangement was at times varied. Occasionally a field with an irregular distribution of items was exposed; and now and then a blank field (*i.e.* the ruled paper background entirely devoid of forms) was exposed. These variations were introduced as check or control experiments.

I. THE INITIAL MOMENT OF EXPERIENCE

The first phase of the problem that we considered was the initial moment of experience. For this part of the work the following instructions were used.

Instructions 1. "A stimulus-field will be exposed on the ground-glass before you. Immediately following its presentation you are to describe your visual experience at the initial moment of the exposure. Fixate the 'spot' on the glass, holding fixation through the period of exposure. A 'ready' signal, followed by a 'now,' will be given prior to the exposure of the stimulus-field."

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[1] From the Psychological Laboratory of Cornell University. I am indebted to Professor E. B. Titchener for the opportunity of carrying out this investigation, and to Dr. L. B. Hoisington for his valuable suggestions.

[2] C. A. Dickinson, Experience and perception, this JOURNAL, 37, 1926, 330-344.

[3] For details of the method see the earlier study, *op. cit.*, 330-332.

Results. (*a*) *Experience as bidimensional.* The *O*s at first described their experience as bidimensional, *i.e.* as a mere spread. Examples are:

H, "The whole appeared as if a flash of light had passed across the visual field." "The whole thing spread at once."

G, "The center lighted up first and clearest, and the light spread out in all directions."

M, "At the initial moment the center became illuminated, then it spread all over the field."

Ml, "Around where I am fixating there is an extent of gray with a yellowish tinge to it, around this area another expanse of bright yellowish gray, and beyond that again another area less bright and less yellowish gray. There is an impression of movement, and I think the brilliant yellowish gray spreads out to the periphery."

(*b*) *Experience as tridimensional.* The *O*s later push back their descriptions of the initial moment of experience to tridimensionality. In place of 'spread,' a 'depth' experience came first. The light goes forward to the ground-glass before it spreads. Examples are:

H, "The welling up of the light doesn't take place so quickly that it cannot be experienced, but as it goes on all parts of the field come up simultaneously and give it arrangement."

G, "It seems as if a ball of soft, white light wells up at the center of the field of vision, drops a little to the left, and then spreads to the whole field. [*E*, "What do you mean by 'welling up'?"] I mean by 'welling up' that the experience seems to come forward a little and get larger."

M, "The flash began with low intensity at the center, at the same time coming toward me, and spreading very quickly in all directions." "That liveliness comes somehow from behind the ground-glass in the third dimension."

Ml, "The gray which is very yellow and bright comes from back of the field into it, and at the same time that it springs forward it spreads out." "Before I see anything else there is this yellow luminosity which springs forward into the field from somewhere behind it."

(*c*) *Experience as unlocalized filmy gray.* The final descriptions of the initial moment of visual experience had not even yet been reached. The *O*s pushed back their descriptions until tridimensional localization also became vague. Examples are:

G, "First I caught—and I don't often notice this—the impression that something is coming, something beginning—a sort of prelude. Then comes the stage I report so often, a burst of light which spreads and lights up the whole field." "I experienced the 'prelude' again. I think it is a vague grayish light experienced before the field appears."

M, "At the initial moment there are no definite boundaries." "A sudden illumination, the beginning of which I failed to get. Then it localized itself in three dimensions, it was bulky."

Ml, "There's something small and its gray, through I can't see any boundaries, and in the very seeing of it, it is gone." "There again was that 'something;' it is gray, but a gray that seems to be not on the field. It is almost transparent and hasn't any boundaries at all. The only thing that I am sure of is that it is gray and that it 'jumps' into experience." "The only experience that I have at first is of a peculiar kind of gray. I see this gray, and yet it appears to jump to the yellow. It is not a gray on the field, but something new, another knid of gray. That's all I can tell about it."

"I think there are two factors there, one a gray, the other a pressure; I can't tell whether it's a gray pressure or not, but it looks like it. The gray part of it is an indefinite filmy experience."

These reports clearly indicate that the initial moment of visual experience is unlocalized, but that it still carries reference to a visual quality.

(*d*) *Experience as 'spread' on the ground glass field.* We now return to the reports in which the *O*s described their experience as bidimensional, in order to illustrate the quality of the experience of the ground-glass, and the manner in which the 'spread' occurred. The following introspections illustrate these points.

M, "First experienced a dark gray field which hadn't yet lighted up. The gray seemed to grow, and then it suddenly burst into light."

Ml, "There is a dark gray filmy 'stuff' which rolls out toward the periphery and flashes." "There is a thick dark gray."

The words 'flash' and 'movement' were frequently employed to denote the 'spread' of the experience.

H, "Not sure whether the flash or the figures came first." "The flash showed a clear bright flare."

G, "Saw only one flash and it seemed much shorter than before." "I was sure I saw everything the first flash." "That time more of the field became clear all at once, but there was a movement of light, I cannot describe it, there seemed to be movement, yet the whole thing stands still."

M, "The flash itself came forward in the center." "Then at the same time the flash spread." "I don't know whether the spread is a movement or not, it does not seem to move from one place to another."

Ml, "What I seem to get is a gray flash and a yellow brilliancy." "The gray flashes, seems to move."

Most of the reports concerning the movement of the 'spread' are mere statements of meaning, but now and then, as the following introspections show, the *O*s were able to give a description of the process.[4]

G, "The light seems to shoot from these spots, a sort of fluctuating and shifting of the light."

M, "There were differences of brightness or intensity of illumination."

Ml, "The gray flash seems to shoot out from the center toward the periphery and to get less intense and thinner as it goes. I say 'goes' because I have this bright area at the center and then afterwards this less intense quality beyond it, which gives the impression of movement." "The point of fixation is dark gray, around that is a yellowish gray which is very bright. I say that this moves out, but what I see is that the periphery is a less intense yellowish gray. It gives the impression of movement. The experience is what you see when you wink your eyes, it means that the intense yellowish gray has moved out to the periphery." "The whole thing moves, it isn't a stationary pattern, the yellowish gray seems to quiver."

II. The Appearance of Form

The second aspect of the problem to be considered was the description of the appearance or emergence of form. The *O*s were instructed as follows.

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[4] E. B. Titchener, Description *vs.* statement of meaning, this Journal, 23, 1912, 165-182.

Instructions 2. "A stimulus-field will be exposed on the ground-glass before you. In this field there will be forms. Immediately following the exposure you are to describe your visual experience relative to the manner in which these forms appear, giving special attention to the initial moment of their appearance. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. (*a*) *Without instructions for form.* Under the instructions to describe the initial moment of experience (Instructions 1), the *O*s frequently, as the following introspections show, reported the emergence of form.

H, "The flash was experienced first and then I saw some black figures in a square." "First a yellow light, and then three rows of three objects."

G, "The first thing experienced was a field uneven in clearness and in brightness. Then forms which were very unclear, almost scrabbled—a sort of mottled field filled with gray spots like clouds." "The figures were very vague and indistinct. I just got the mere grouping and the fact that figures were present." "The field was not uniformly clear. [*E*, "Describe the unclear."] The unclear appeared as though seen through a grayish cloud. Some figures or parts of figures seem to fuse with the background, their boundaries are indistinct. I mean by fusing that I can't tell where the background leaves off and the figures begin. Both contour and color are a sort of mixture of the two with little of no contour."

M, "First the bulky light, and then black indistinct forms; some light grayish splotches of low intensity, not forms at all, just opaque splotches. The center came first as black and then as form."

Ml, "By the time the gray flashed on there were some qualities present which later became figures." "I don't see any recognizable shapes or colors in the first instant. The clearest content is at the point of fixation and it is blacker than the rest of field, but even that isn't definitely outlined. It is just a visual quality and that's all. After that the forms then take definite shape."

(*b*) *With instructions for form.* In contrast to the foregoing reports compare the following given under the instructions (Instructions 2) for form.

H, "Experience seems to well-up into squareness all at once, comes at you, and then you begin to discern things within the whole."

G, "Forms were there immediately; the letters seemed to move upward to the right and then settle back. Only two of them were clear as definite forms."

M, "There were two forms of the playing card variety, but what ones they were I do not know. The rest of the field was just gray splotches."

Ml, "The field has on it things like shadows which are arranged all over. Suddenly they cleared up and showed definite form and color. At first they were just blurs."

(*c*) *The development of particular forms.* In the reports of 3 of the *O*s the emergence of form took on a temporal aspect, as the following examples indicate.

G, "The lower left was a sort of blackish red which became less black and more reddish, and then finally quite red."

M, "Near the fixation point I saw a dull red which later changed into a red heart."

Ml, "At first the whole outline of the figure was not taken in. I saw a little piece of the '9' which was just a gray quality and then this grayness went away and I had the figure."

III. THE RELATION OF CONTOUR TO LOGICAL MEANING

In the third section of the experiment we were concerned with the relation of contour to the logical meaning of the forms. The following instructions were given to the *O*s in this part of the work.

Instructions 3. "A stimulus-field will be exposed on the ground-glass before you. In this field will be forms. You are to name the center form and to verify some other—the name and location of which will be called just in advance of the 'now' signal. At times a form other than the one called will be exposed. Immediately following the presentation of the stimulus-field you are to describe your visual experience from the time of the naming of the center form to the completion of the verification of the form called. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. (*a*) *When one form is called and a different but similar form is exposed.* The tracing of form is at times quite regular, as the following reports show, when one form is called and a different but similar form is exposed.

G, (when the letter O was called and the letter D exposed),"The letter D flashed at the left, quite round at first, but recognized it as D as soon as it appeared; the two corners, however, were sharp. The experience was fleeting. I knew it as D although it looked like an O at first." (When the figure 8 was called and the letter S exposed),"The upper right became distinct and clear as a black S which grew quite large and tended to straighten out, *i.e.* the ends elongated."

M, (when Q called and G exposed),"I traced the form, that I first saw as round and O-ish, from the top to the left and down. When I started to go up to the right I was checked and then saw it as a G. I didn't see it at any time as a Q although that had been called out. The letter lost its O-ness as soon as I got to the gap with the cross bar."

Ml, (Y called and V exposed), "There seems to be a little gray darting quality which, as it travels, rolls up into itself so that it is always about the same size; it traces the letter which I expect. In this case it did not trace the whole letter but just the point where the two lines come together in a V, and a little beyond that to the right. The clearest part of the experience seems to be the part of the letter covered by this tracing. There in the background, was a more luminous and brilliant gray."

(*b*) *When one form is called and a different form is exposed.* The reports given below illustrate the course of experience when a form is exposed that is dissimilar to the one called out.

M, (M called and W exposed), "It appeared at first as a black form which was outlined which I could not make out except that it was black and had contours. Then the form changed, it expanded outward toward the top and inward toward the bottom; the center was merely a grayish spread. The center and the bottom were both very indistinct. Then the experience meant W. I was for some reason or another expecting a change in the opposite direction so this change seemed to be taking place in the face of opposition. It appeared as though a cloud-like experience had to be pushed away from it."

Ml, (K called and X exposed), "Where the two lines crossed there wasn't any mist or cloudiness. I traced the part of the K which would come at about that position on the X, and this tracing appeared as a little gray mist that formed an angle such as a K forms; instead of crossing it, it came back. This little gray thing was on top of the red, traced right over it, and appeared very clear. It almost stood out toward me just at that point where the K is particularly different from the X. It was there for just an instant and then was gone."

Sometimes the *O*s failed to catch this tracing process, and instead reported a later stage in the change of one letter into another. The following are examples:

H, (Q called and G exposed), "Not sure whether it was G or Q. There is a quirk there like a Q, yet the Q is not complete. There is a break in the contour so that it looks like G." (V called and A exposed), "Uncertain about that A, it looked like an inverted V."

G, (3 called and 8 exposed), "A figure 8 appeared at the upper right, rather large, with only the upper part of it clear. I am sure, however, that it was an 8 because the upper half was a complete circle."

M, (Q called and G exposed), "Lower left experience not a Q because somehow there was a gap in the contour. I also noticed a bulging or increase of black in the experience just below the gap, and too high for the tail of the Q. It then meant G."

(*c*) *Form momentarily completed by projected imagery.* Sometimes, when the letter called is similar in contour to the one exposed, the gray process over-runs the boundaries of the actual form and completes the perception. Instances are:

M, (O called, C exposed), "That first appeared as a well defined black C which immediately sent out a grayish process over the gap and completed the O. Suddenly this disappeared, the letter was definitely a C again, and it remained so."

Ml, (E called, F exposed), "One seems to wait just a second for that extra stroke to come out on the E. During that time that area seems clear; then there's a little dark gray streak across where the stroke should be as if a ball of cloud were thrown along that line and the extra stroke made. That little speck of gray is clearer than all the rest of the letter, and the yellowish gray background upon which it appears it is more brilliant than the rest of the background. It doesn't form a perfect stroke and fill the letter out—it doesn't stay long enough for that—it merely indicates where that extra stroke should be. As it unrolls ahead it rolls up into itself behind, so there is just a small area visible at a time."

IV. THE TRANSITION FROM ONE FORM TO ANOTHER

The fourth phase of the problem that we considered was the description of the content of the periods of transition between the naming of one form and the naming of another. In this part of the experiment the following instructions were used.

Instructions 4. "A stimulus-field will be exposed on the ground-glass before you. In this field will be shown various forms. Immediately following the exposure of the stimulus-field you are to describe your visual experience in the period of transition from the naming of one form to the naming of another. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. (*a*) *The emergence of the items.* The reports of the *O*s in this section bring out the fact clearly that there are no discrete additions in experience, that one item emerges from a preceding one, that it arises from a grayish mass into which, as it were, the preceding item had dissolved. Examples are:

G, "The letter F quite distinct, then expansion of the field in the direction of the upper right and O became clear. These shifts of clearness are very much like the shifts one gets with eye-movement, as if I were now looking at one place, now at another, but I feel certain that my eyes were fixed on the spot throughout the experiment. The field swells or expands toward the right."

M, "The field widened diagonally, and as it widened I cognized the letter X—not, however, distinctly. It was like a 'pull' toward that letter. The letter became clearer and more prominent. [E, "What do you mean by a 'pull' toward the letter?"] The letter becomes larger, clearer, and attracted to one side of the field."

Ml, "The first thing in going from one letter to another is this black or dark gray strip which moves in the direction in which I am going. Next to this black there is a bit of gray film, and that's all I see for an instant; then the black moves over and draws the film after it, or moves over the film, and I see that the film is spread out and through it certain dark qualities appear. These qualities are not definite letters, but merely indications of forms. That is true of the letter I just left, and of the one to which I am going." "For just an instant there is nothing present but a dark streak, then behind it, attached to it, or existing under it there is a transparent murky grayness. The figure appears to be behind this grayness, yet I seem to see through it. The thing I call a black streak isn't definite like a rod or pole but shades into light gray. While the core moves that is all I see."

(*b*) *The development of color.* The reports below show that color also developes from gray, and that the regression from color is to gray.

Appearance of color:

G, "The letter C was first dark, then it became reddish."

M, "Saw a gray, then a red of medium saturation, then a saturated red heart."

Ml, "At first there are only gray shapes present. The shape near where I fixate is the clearest and nearly black." "The forms were spread out around the fixation-point. The one to the left became red almost immediately, but it was at first gray." "At first there was a darker gray which marked the position of the letter. This gray was somehow less dark than some of the others. The first I knew of color was that this was different from the gray of a black figure. It did not seem red until later." "Even the red letters are gray before they are red, but they are of a lighter gray than that of the black letters."

Disappearance of color:

G, "In disappearing the form seemed to shrink toward the center and at last was an unclear mass, more black than red—really a dark gray." "At the lower left was a red W which, as it went out, shrank into a dark grayish ball, quite dark at the center and fuzzy at the edges."

M, "At the same time the figure contracted and in one jump it lost its saturation and was gray." "At the left was a red pear-shaped surfacy form. In disappearing it moved away toward the left and lost its form and redness. In its place was a grayish form with just the faintest trace of red. Then the redness disappeared and it became a smoky splotch, scarcely differing from the ground-glass field."

Ml, "The top and the bottom of the figure lost contour and color just a little; then it was there, not as a red heart, but as a very light gray. It seems that the black figures go out by way of a dark gray, and that the red figures go out by way of a lighter gray."

(*c*) *Effect of irregular arrangement of the forms on the stimulus-field.* Occasionally a field was exposed in which the stimulus-forms were arranged irregularly. The effect of this is shown in the following reports.

H, "The figure at the center was something upside down. I got it as a spade after sensing 'upside-downness'."

G, "It looked like a J at the left. It caught my attention because it was so peculiar, *i.e.* upside down." "Very peculiar feeling when that appeared; very uneven and it did not hang together. Shortly after the first flash the meaning was changed, *i.e.* it meant some letters were missing."

M, "First got the meaning of heart, then of an inverted spade, and then of red." "The first thing I actually observed was the redness, then the upside-downness, then I saw that it was a club."

(*d*) *Effect of the blank experiments.* The blank or check experiments, in which the ruled paper background alone was exposed, gave the following results.

H, "A flash first and then nothing but ruled squares."

G, "First a grayish movement near the fixation-point, then the cross lines which moved outward in all directions. The sides of the squares seemed to bulge and become curved lines, and these seemed to extend out toward me as if the whole surface was convex, and then to sink back again with the shrinking of the surface."

M, "I don't know how to describe this experience. The fixation-point stood out, and the background behind lighted up. The illumination did not remain in one place; it actually moved, spread out from the center and shrank back again."

V. The Disappearance of Form

The final moment of experience in which the stimulus-forms disappeared was the fifth and last phase of the problem considered. The experiments of this section, however, were divided into two groups: (*a*) the general experiments in which the *O*s were free to describe the total visual field or any of the forms within it; and (b) the particularized experiments in which the *O*s were directed to describe some particular form whose position was called out just before the exposure was made.

(*a*) *General experiments.* The instructions for the general experiments were:

Instructions 5. "A stimulus-field will be exposed on the ground-glass before you. In this field will be shown forms. Immediately following the exposure you are to describe your visual experience at the moment in which the forms disappear. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. Form disappears in a manner practically the reverse to that in which it appears as the following reports attest.

M, "The forms lost their articulateness gradually; they became more and more blurred, the contours became less and less distinct, the quality less and less saturated and bright. After the forms had lost their articulateness and saturation, they moved slightly and then disappeared."

Ml, "The field begins to contract and the forms lose outline. They are no longer forms but are diffuse, dull, opaque, cloudyish gray blurs. They moved a little toward the left and then disappeared."

(*b*) *Particularized experiments.* The instructions for the particularized experiments were:

Instructions 6. "A stimulus-field will be exposed on the ground-glass before you. In this field will be shown forms. Immediately following the exposure you are to describe your visual experience at the moment in which some particular form—whose location will be given you—disappears. Fixate the 'spot' on the glass," etc., as in Instructions 1.

Results. Samples of the reports obtained under these instructions are:

G, "The background around the F became gray and F disintegrated. The corners became round instead of sharp. The background was brightest at those points. Other parts became grayish and the boundaries roundish; saturation much decreased." "The left side seemed to grow upward and then the right side also. After that the figure seemed to disintegrate, to become indistinct and to disappear as an irregular mass."

M, "First lost distinct contour and saturation as it became gray. Then it contracted and thinned out until it was gone." "The black heart soon lost all contour and became less black, more and more dull, grayer, more diffuse, and indefinite in form. Finally disintegrated and vanished into the ground glass."

Ml, "The figure first lost a little of its blackness at the lower part, then at the upper, then its contour, and finally there was just a gray patch which was swallowed up by the darker gray of the background." "As it disappeared the black became nearer gray as if the mist had lightened it, and then the figure lost contour. The lower part of the 4 disappeared first and at one time it looked like a dark smoke ring in a lighter smoke. Then the gray mist which was getting darker and thicker swallowed up this circle."

Discussion of Results

The data we have presented carry through the course of visual experience from its initial moment to its termination. No attitudinal bias, either from the standpoint of process or from the standpoint of meaning, is set up by the instructions,[5] for the instructions mention neither process nor meaning, they simply call for description. In the early stages of the experiments, descriptions were limited to what appeared on the ground-glass field, but later the *O*s pushed their descriptions back beyond that. Either they were able to catch experience at an earlier stage and to retain it for description, or they developed the ability to ignore the stages which immediately followed the initial moment of experience and thus were able to give a more complete description of the initial moment itself.

We follow in our discussion the course of experience as it unfolded in the descriptions of the *O*s.

Relatively early in the experiments the *O*s report that the experience of form follows the appearance and the spread of the light which is bidimensional and which seems to "begin somewhere in the center and to throw itself over the whole field" (cf. I-a). Immediately following this "spread" the forms make their appearance (cf. II-a, b).

At a later stage in the experiment the *O*s report experience before the "spread." They push back their descriptions to a "kick of light," to a "welling up at the center of the field," to an experience which is tridimensional and which precedes the spread of light toward the periphery (cf. I-b). Such reports are given by all the *O*s, and some of them (H was unable to go back beyond this point) push their descriptions of the initial moment of experience back still further.

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[5] E. B. Titchener, *A beginner's psychology*, 1915, 26 f.

At this last stage the *O*s describe experience as an unlocalized filmy gray, as "something coming," "something beginning," which cannot be described. Something is experienced that is merely "a vague grayish light." This is not on the field, it precedes any distinctness of localization. It has at the first moment "no definite boundaries." Yet this something, indefinite and unlocalized as it is, carries a visual quality, which both directly and indirectly stated, is a filmy gray. Ml characterizes it specifically as a "pressury gray" which tends toward transparency and which appears to belong "more intimately" to herself "than out in space." The initial moment of visual experience according to the reports of our *O*s and under the conditions of our experiments is an unlocalized filmy visual experience light gray in quality (cf. I-c).

When we turn to the reports of experience as 'spread' on the ground-glass field we find that, as the spreading occurs, there is a "thick dark gray" which "moves out toward the periphery," which "rolls out," which "peels off," and leaves the luminous field exposed. This dark gray quality appears in many ways and at various times, but it is always recognizable by its opaque blue-grayness. After the field has become luminous "the brightness seems to shoot out" from various spots; there is "a fluctuating and a shifting of the light," often accompanied with the report that the "field itself is not uniform in brightness" (cf. I-d).

At this point we may note the use of the words 'flash' and 'movement' (cf. I-d). 'Flash' is used in a variety of ways and with a number of meanings. It may be "fairly large and circular," it may "continue all the time" of the exposure, it may be "gray" in quality, it may start with the 'spread,' or it may be localized behind the ground glass. 'Movement' is used in a similar manner. Few descriptions of these terms—as so frequently happens when categorical terms are used in report—were attempted; but from those that were given it seems fair to state that the words imply a rapid dimensional change in experience, *i.e.* a change of quality, density or texture.

Experience so far considered is devoid of meaning—other than the meaning of a filmy, unlocalized, visual quality. The meaning which later develops is an accrual, an overlay. The progression from this initial moment of experience to a particular meaning is by no means simple and direct; it is cut across and obscured in many ways. In any stimulus field we may find "eddies," "shifts," "quivers," "changes in intensity," and "changes in quality" before meaning is stabilized. When a highly specific meaning has accrued we may find that it has been organized around a very meager core of sense data—as illustrated by the reports in which the *O*s characterize their experience by the words, 'flash' and 'movement.'

The emergence of form, reports of which were obtained under two sets of instructions (II-a, b),[6] further illustrates the complexity of the development of meaning. For H, the least practiced of our *O*s, form appears very quickly, nevertheless he too at times reported the 'flash' which all the other *O*s consistently reported as occurring before the appearance of form.

G describes the appearance of the forms in the 'free' reports (II-a) as "just the mere grouping," as being "quite unclear," like "gray spots," and as being so indefinite that he is unable "to tell where the background leaves off and the figures begin." In the 'constrained' reports (II-b) he describes the experience as "a grouping of figures, but these figures are very vague," and says, "the forms seem to rise up out of the background."

M, in the 'free' reports, characterizes the forms as "dark gray splotches," as "splotches of a different quality from that of the background." In the 'constrained' reports he characterizes the forms as "just opaque splotches," as "barely formed spots, just something there besides the illumination."

Ml, in the 'free' reports describes the forms which emerge in experience as being "like formless grayish shadows," "mere bits of grayish qualities." In the 'constrained' reports she describes them as being "like fragments of dark gray," and as "shadowy things which later become forms."

Forms are differentiated from the field first as a mere differences in quality. It is only later that contour is added. The initial appearance of form is nothing more or less than the appearance of a shadowy quality or a gray splotch. The thing most certain about the forms at this stage is that they have a grouping on the field. One of Dallenbach's *O*s, in an experiment undertaken to differentiate between attributive and cognitive clearness,[7] pushed back his description to an approximation of this indefinite beginning of form.

Following this indefinite stage, the form at the fixation point very quickly assumes a definite contour and takes on a particular meaning. The accrual of this specific or logical meaning is clearly illustrated in the experiments of Section III in which the development of particular forms is contrasted with the development of forms in general. In those experiments the *O*s were required to describe experience, in its various stages of development, until it stands forth as a fully named object.

The 'shadowy grayness,' which first appears, takes on a "loose, matlike texture," whose edges are "fuzzy" and barely discernible from the quality of the luminous field. Saturation has not yet accrued, nor has the solidifying effect of contour been added, at most the form is still "gray splotch." Saturation and contour, however, very quickly appear; saturation increases first, slightly in advance of contour, and it continues to increase after the contour has become relatively stable. After this there is a hardening up from 'splotch' to particular form. The loose texture solidifies, it becomes more surfacy and smooth; the color 'fades'[8] into its maximum; contour differentiates sharply; and the particular form stands forth as specific object. It apparently does not matter whether the object-color is red or black, for both of these qualities develop from gray, the red from a light gray, the black from a dark gray.

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[6] The bias of instructions is often credited with being productive of considerable warp in the results of an experiment—and this may be true—but in our case the 'free' (II-a) and the 'constrained' (II-b) reports differed but little if at all, as a comparison of the protocols clearly shows.

[7] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 189 ff.

The tracing process, which was reported in the experiments of Section III, also plays an important rôle in the development of the logical meaning. The tracing is rapid, so rapid and fleeting that the *O*s sometimes fail to catch it and instead consider the accrual of logical meaning the result of factors which subsequently appear (III-b). Whether the contour be directly perceived as in the tracing process or indirectly perceived as in the observation of critical breaks and gaps, it is of paramount importance in the accrual of specific or logical meaning.

The experiments of Section IV clearly show that there are no discrete additions to experience, that the course of experience is characterized by gradual accretions. The letter called out sets up imagery which must be changed or accommodated, when a different letter is presented, to another contour. It is difficult for the *O*s to determine just what is taking place for very often nothing is, at certain periods in the change, clear or distinct. This is particularly true when the imagery aroused is wholly different in contour from that of the letter exposed. Distinctness of form resolves itself into a gray mass out of which arises another form that takes on a particular meaning.

For all the *O*s there appears to be a 'zero point' of meaning in the transition from one letter to another. This 'zero point' is visually a filmy grayness out of which arises the second letter. Here it is a question of establishing imagery to fit the sense data; in the previous section (III) it was a question of fitting the sense data to existing imagery. In both instances, however, there is a period of confusion or blur out of which steps the other letter.

In the transition from one letter to another, existing imagery must be dissipated and other imagery found to fit the sense data if the experience is to mean. In such cases, where experience is travelling in high gear, as it were, so many factors enter in that the actual tracing may be lost sight of and the filmy gray noted as a period of confusion. There is no break, however, in visual experience; the closest approximation to a break is the appearance of the visual gray.

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[8] Analogous to the 'fade-in' of motion pictures.

The progressive overlay, which begins at the moment of localization and continues to the culmination of experience in logical meaning, is extremely complex. Even before the forms have begun to make their appearance a host of factors—the 'spread,' changes in brightness, dimensional shifts, and qualitative gradations—have run their course. These factors become no less complex in the emergence of form or in the development of particular forms. A great deal of what we experience visually may be considered analogous to the rise and fall of the ocean swell—there is, as it were, a rise and fall in the overlay, a progressive dimensional change, and with the shift in focality an occasional rise to logical meaning.

The overlay exhibits itself in a somewhat different manner in the actual moment of naming, particularly when a field is exposed with the stimulus-forms arranged in an unusual manner, *e.g.* placed irregularly or upside down. Items so arranged are likely to be named first.

In general it may be said that the experiential overlay manifests itself either as a progressive dimensional change which takes place gradually, or as a quick rise to logical meaning. Up to a certain moment the slower change in the pattern of the overlay is non-focal, it belongs to the total pattern and is part of the same unity. When it becomes focal and takes on logical meaning, the whole meaningful complex is still a progressive total for the meaning accrues with the shift and the emphasis in the focality of the pattern. In its main direction its direction of progression—the meaning is perceptive, *i.e.* it means the quality that is there in experience. Yet it has the suggestion upon it of its place in the qualitative scale. Thus far it is comparative, but the specific concrete comparison comes only with the later shift in the total pattern, when the focus again dominates as at the beginning. At every shift in focality which culminates in logical meaning a new unity arises, not, however, by discrete additions, but by shifts inemphasis and in the direction of the focality of the meaningful complex.

CONCLUSIONS

From the results of our study we feel justified in concluding:

(1) That under our conditions the initial moment of visual experience is a filmy unlocalized semi-transparent gray.

(2) That visual experience is localized tridimensionally at first and that it later is localized bidimensionally, *i.e.* on the surface of the ground-glass field.

(3) That there is a continuous dimensional change with shifts in focality in this progressive overlay of experience.

(4) That meaning is a progressive overlay of experience, and that logical meaning is the climax of this overlay.

(5) That the accrual of contour is an important factor in raising this overlay of experience to logical meaning.

(6) That, in the progression to logical meaning, gray precedes color, and that, in the regression from this logical meaning, color recedes to gray.

(7) That, in the transition from the naming of one item in a stimulus-field to the naming of another, there is no discrete break in experience. One item disintegrates into a gray matrix out of which the following item arises as a new unity.

(8) That in the course of experience there is a gradual rise to a climax and then a gradual decay, and that the order of decay is the converse of the order of development.

(9) That, in the light of the data presented in this study, configuration as stated in *Gestalttheorie* has been emphasized beyond its legitimate bounds.

THE EFFECT OF ONE FORM OF REPORT UPON ANOTHER

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The purpose of this investigation is to determine the effect of one form of report upon another and totally different form of report. In witnessing an event or observing some objects, one might write out a narrative account of what he saw; or his report might take the form of answers to a series of questions based upon the event or objects seen. As is well known, the former is called narrative report, while the latter is known as interrogatory report. In this experiment, groups of subjects were asked to report under three different conditions; namely, simple narrative, simple interrogatory, and narrative plus interrogatory. With each condition an immediate recall and a delayed recall were taken. The delayed recall was taken after intervals of 30, 60, 90 and 120 days, depending upon the group.

The observational material used in this experiment was the Binet Object-Card, a rectangular sheet of orange-yellow cardboard, 33.5 × 40.5 cm. Attached to the sheet are two photographs, a red and white label, a white button, a one cent postage stamp and a penny.

Just prior to the observation of the object-card, the subjects were instructed that they were to be shown a cardboard with several objects attached, and that they were to observe it very closely, since immediately after observing the cardboard they would be asked to report what they had seen. The subjects came to the front of the room in groups of three to observe the material, and were given 30 sec. for inspection. After inspecting the cardboard they returned to their seats and made written reports of what they saw, in simple narrative form, simple interrogatory, or narrative plus interrogatory form, depending upon the group. The interrogatory consisted of 50 questions relating to the object-card.[1]

Twelve different groups of *S*s, mainly sophomores in Washington University, participated in this experiment. Four groups reported in the form of a simple narrative account of what they had observed on the cardboard. Four other groups gave answers to the interrogatory; while another four wrote a narrative account which was followed by answers to the 50 questions. It will be understood that with the narrative, interrogatory, and narrative plus interrogatory, every group wrote an immediate recall and a delayed recall, the intervals of delay being 30, 60, 90, and 120 days. The number of *S*s in each group was as follows:

	Form of Report		
Interval	Narrative	Inter.	Narr. plus Inter
30-day	39	37	49
60-day	46	41	54
90-day	31	35	26
120-day	26	42	24

At the expiration of the interval, the *S*s were reminded of their previous observation of the cardboard, and were now asked to make the same kind of report as they had made immediately after that observation; *i.e.* narrative, interrogatory, or narrative plus interrogatory report, according to the group.

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[1] The questions were those suggested by G. M. Whipple, *Manual of mental and physical tests*, 1910, 298 f.

The narratives were scored by counting each item, right or wrong, as one. Thus a report, "an oval picture of a horse-drawn sleigh" would be given a credit of 4 points. Interpretations and indefinite statements were not counted. The interrogatories were scored by counting the answer to each question, right or wrong, as one.

There are three phases of the problem which may concern us in a discussion of the results. They are: (1) The effect of the immediate interrogatory upon the subsequent narrative after the time interval; (2) the possible effect of the immediate and subsequent narratives upon the subsequent interrogatory; and (3) a consideration of wrong items. We shall discuss these in the order mentioned. No statement can be made relative to the effect of the immediate narrative upon the immediate interrogatory in the double condition, as the conditions were not controlled to permit any such statement. Since different groups were involved for the various intervals, and their original learning scores were different, a cross comparison is not permissible.

TABLE I

Mean Number of Correct Items in Immediate and Delayed Narrative

Form of Report	Group	Interval 30-day	60-day	90-day	120-day
	Immediate	58.6	61.0	60.3	64.7
Narrative	Delayed	52.1	44.5	36.3	35.4
	Diff.	6.5	17.5	24.0	29.3
	Sig. Diff.	2.82	3.17	3.76	4.64
	Immediate	65.5	57.3	56.7	50.1
Narr. plus Interrog.	Delayed	62.0	44.3	48.5	39.4
	Diff.	3.5	13.0	8.2	10.7
	Sig. Diff.	2.8	2.9	3.58	3.6

(1) Table I presents the means of the immediate and delayed recalls at each interval, with their differences and sigmas, for the two conditions of simple narrative and narrative plus interrogatory. It would appear that the differences are thoroughly reliable except at the 30-day interval for both conditions and at the 90-day interval for the narrative plus interrogatory condition. The general tendency, however, appears to be consistent. Table I, it must be stated, gives simply the means of the raw scores. Since there were four different groups involved in each form of report for the four different intervals, it will be more convenient to express the amount of retention by stating the ratio, expressed in percentage, of the number of correct items in the delayed recall to the number of correct items in the immediate recall. Table II gives these percentages, and the same data are represented in graphic form in Fig. 1.

TABLE II

Percentage Ratio of Correct Items in Delayed Narrative to Correct Items in Immediate Narrative

Interval	Form of Report	
	Narrative	Narr. plus Inter.
Immediate	100	100
30-day	88.8	94.6
60-day	73.0	77.3
90-day	60.1	85.4
120-day	54.7	78.7

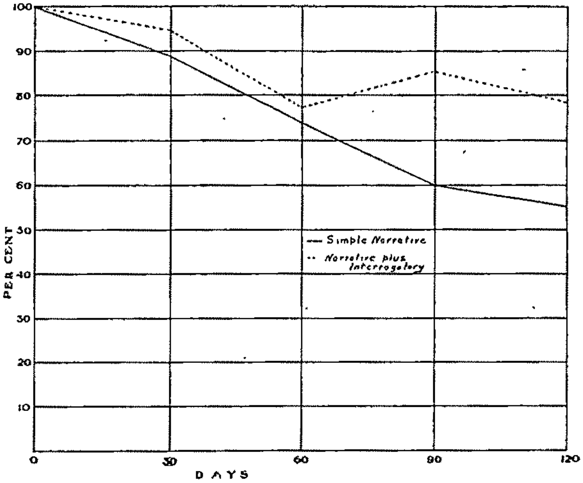

FIGURE 1

For the simple narrative condition, it will be observed that the curve of retention falls very nearly in a straight line from immediate recall to 90 days, and more slowly from 90 to 120 days, indicating a slight negative acceleration.[2] The narrative plus interrogatory curve is above the simple narrative curve at all four points. It is interesting to observe that, while the curve for the double condition runs at a higher level than that of the simple narrative condition throughout, the shape of the two curves is very similar up to the 60 days, after which the curve for the double condition shows a marked negative acceleration. It seems reasonable to conclude that the interrogatory report which followed the immediate narrative served to suggest items observed, and therefore contributed to facilitate spontaneous recall at subsequent intervals. This transfer effect, if we may call it thus, appears to be accentuated at intervals of 90 and 120 days, although the facilitating effect is consistent for all intervals measured, as suggested by a comparison of the two curves. The difficulty of revival would presumably be greater after the longer intervals, and there would be a greater opportunity for the operation of such associative cues as the interrogatory should furnish. Hence we might expect a greater effect after longer than after shorter intervals. This would be clearer, however, if the interrogatory immediately preceded the subsequent recall.

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[2] A complete discussion of the retention curves under the simple conditions appears in an article by us on The recall of observed material, *J. Educ. Psychol.*, 17, 1926, 419 ff.

(2) In the second phase of the problem we are to consider the effect of the immediate and subsequent narratives upon the subsequent interrogatory. Here we are to compare the simple interrogatory condition with an interrogatory that was preceded by a narrative form of report. The means of the gross scores, immediate and delayed, with their differences and sigmas, are presented in Table III. The amount of forgetting as measured in terms of interrogatory report is relatively slight, and the differences are statistically unreliable. Again, since a different group was involved for each interval, it is better to show what percentage the number of items right in the delayed interrogatory constitutes of the number of items right in the immediate interrogatory; as in the conditions previously discussed. Such data, for the two conditions, are shown in Table IV and presented graphically in Fig. 2.

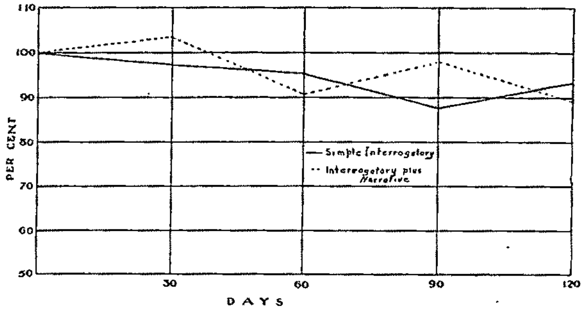

FIGURE 2

TABLE III

Mean Number of Correct Items in Immediate and Delayed Interrogatory

Form of Report	Group	30-day	60-day	90-day	120-day
		Interval			
Simple Interrogatory	Immediate	36.9	38.2	37.69	38.0
	Delayed	36.0	36.5	33.12	35.5
	Diff.	.9	1.7	4.58	2.5
	Sig. Dif.	2.0	1.72	1.63	1.5
Inter. plus Narrative	Immediate	39.2	38.6	34.3	36.2
	Delayed	40.7	35.1	33.9	32.4
	Diff.	1.5	3.5	.4	3.8
	Sig. Dif.	1.07	1.13	2.59	2.54

TABLE IV

Percentage Ratio of Correct Items in Delayed Interrogatory to Correct Items in Immediate Interrogatory

Interval	Form of Report	
	Interrogatory	Inter. plus Narr.
Immediate	100.0	100.0
30 days	97.7	103.8
60 days	95.5	90.7
90 days	87.8	98.4
120 days	93.4	89.5

The curves intercept at three different points, which indicates that no consistent tendency is manifested. We would conclude that the interpolation of the narrative report does not facilitate revival measured in terms of the interrogatory.

(3) Thus far we have said nothing about errors. In each recall for the various conditions there were a few errors; these errors, however, were highly variable and the differences indicate low reliability. We are not justified in drawing any conclusions regarding error scores, and for this reason tables showing error scores are omitted.[3]

CONCLUSIONS

(1) The interpolation of an interrogatory form of report immediately following a narrative report has a facilitating effect upon subsequent narrative recall at 30, 60, 90, and 120 day intervals. This effect is more accentuated at 90 and 120 days than at 30 and 60 days.

(2) Narrative report interpolated prior to interrogatory recall, however, does not facilitate recall or report measured in terms of answers to a series of questions.

(3) With respect to errors, no effects of one form of report upon another are observable for any condition measured.

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[3] Cf. K. M. Dallenbach, The relation of memory error to time interval, *Psychol. Rev.*, 20, 1913, 323-337.

THE EFFECT OF SERIAL POSITION UPON RECALL

By John G. Jenkins and Karl M. Dallenbach, Cornell University

The results of investigations undertaken to determine the effect of serial position upon memory and reproduction have been generalized, by many writers of elementary textbooks, into the 'laws' of primacy and recency. These generalizations have been justified on the ground that all the experimental results, from those of Bigham in 1894 to those of Warden in 1924, show that the first and last members of a series are best retained, and that the middle members are in positions of disadvantage.[1]

Recently, both the experimental results regarding 'primacy,' and the generalization based upon them have been challenged. Welch and Burnett, in 1924, obtained results (in a study that was undertaken specifically to determine the effect of primacy) that are diametrically opposed to the earlier findings.[2] They discovered that when the *S*s' tendency to 'think backward' during the exposure of a series is prevented, *i.e.* when the *S*s have no opportunity of rehearsal or review, "the alleged effect of primacy disappeared."[3] Primacy, according to their results, is effective only in so far as it makes for frequency. These unusual (one might almost say unorthodox) results have revived interest in the problem of primacy, and two investigations regarding it have been reported within the past few months.

In the first, that by Robinson and Brown,[4] results are reported which reaffirm the old findings. The authors worked over the data of two experiments on memory that had previously been performed in the Chicago laboratory, and discovered that the "advantages of both primacy and finality were pronounced," and that "primacy shows even a greater advantage than finality."[5] They criticise Welch and Burnett's conclusions on the ground that the data were not correctly interpreted. In the light of the work that has already been done on the problem they feel that these results can best be explained, since Welch and Burnett's *S*s gave their reports immediately after the exposures, "in terms of the greater time interval between memorizing and recall in the case of the first syllables"[6]—an explanation which places Welch and Burnett's investigation outside the realm of the problem in question. The net result of Robinson and Brown's investigation is to return us to where we were before the publication of Welch and Burnett's study.

In the second and most recent investigation of the influence of primacy, that by Thorndike,[7] the results of Welch and Burnett are confirmed: "primacy in and of itself has zero potency."[8]

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[1] John Bigham, Memory, *Psychol. Rev.*, 1, 1894, 457.

M. W. Calkins, Association: An essay analytic and experimental, *Psychol. Monog.*, 1, 1896, No. 2; Association, *Psychol. Rev.*, 3, 1896, 39, 44 f., 48.

W. G. Smith, The place of repetition in memory, *Psychol. Rev.*, 3, 1896, 21-31.

H. Ebbinghaus, *Grundzüge der Psychologie*, I, 1902, 624-626.

A. Pohlmann, *Experimentelle Beiträge zur Lehre vom Gedächtnis*, 1906, 92-138.

W. A. Owens, unpublished, reviewed by E. S. Robinson and M. A. Brown, this Journal, 37, 1926, 539 f.

E. O. Finkenbinder, The curve of forgetting, this Journal, 24, 1913, 8-32.

G. J. Rich, Directed attention and learning, *J. Educ. Psychol.*, 8, 1917, 239-240.

R. S. Woodworth and A. T. Poffenberger, *Textbook of experimental psychology*, 1920, 71 f.

C. J. Warden, Primacy and recency as factors in cul-de-sac elimination in a stylus maze, *J. Exper. Psychol.*, 7, 1924, 98-116.

[2] G. B. Welch and C. T. Burnett, Is primacy a factor in association formation, this Journal, 35, 1924, 396-401.

[3] *Op. cit.*, 401.

[4] E. S. Robinson and M. A. Brown, Effect of serial position upon memorization, this Journal, 37, 1926, 538-552.

[5] *Op. cit.*, 552.

[6] *Op. cit.*, 543.

[7] E. L. Thorndike, The influence of primacy, *J. Exper. Psychol.*, 10, 1927, 18-29.

[8] *Op. cit.*, 18.

Regarding the accuracy of Thorndike's experiments there can be no question, but serious objection can be raised to their relevancy, and to the conclusion that he draws from them. Thorndike's *S*s, in the first experiment, for example, estimated the lengths of 25 paper strips which varied in length by ¼ in. steps from 5 in. to 11 in. The series were presented 10 times, thus giving 10 estimates for every length from every *S*. Because the first estimates are not repeated more often in the last 7 of the 10 responses than the second or third, Thorndike concludes that primacy in and of itself has no potency. The experiments were carefully planned and executed, and the results most clearly establish the fact that the first estimate is not indicative of the later ones. So far Thorndike has established his thesis. But what have these results to do with memory and association? The *S*s were directed to estimate, not to remember their estimates. Unless an association was formed between the first estimate of a given length and that length, how could the efficacy of primacy be tested? And what guarantee have we that such an association was formed? Mere contiguity is no guarantee. If no associations were formed—and Thorndike's results clearly indicate that they were not—then the experiments are irrelevant to the problem in question and take their place with the experiments on judgment. We might just as well look to judgments in the lifted-weight experiment for data upon this problem as expect Thorndike's experiments to contribute to its solution.

The effectiveness of primacy in serial learning is not questioned by Thorndike, but the doctrine of primacy which has grown up around it is rejected as fabricated. "The experiments of Calkins.....," he says, "do not, as we interpret them, necessarily show that 'the earlier one element enters into association with another, the more likely it is to be recalled with that than with any other with which it has been associated at a later period;' but rather that being first in a series of twelve paired-association tasks has an advantage over being in the middle of such a series. It seems probable that the advantage is not due to primacy in and of itself, but to the factors which make both the beginning and the end of a series better remembered than the middle."[9]

The potency ascribed to primacy, Thorndike maintains, can be explained by two corollaries of his general law of learning. (1) "That, other things being equal, the stronger a connection is, the oftener, and so the earlier, it will show itself. Being first does not make a connection stronger, but being strong makes a connection likely to be first." (2) "That when responses are connected with situations to which there are no pre-existing connections of more than infinitesimal strength, the first experience raises the frequency from approximately 0 to 1, the second experience of the same connection raises it from 1 to 2, and so on. The *relative* strength of the connection with its competing infinitesimals is raised to 1-to-0 by the first experience, and only to 2-to-0 by the second experience. This is an effect of frequency caused by position and is allied to the facts of diminishing returns and overlearning. It is different from the alleged primacy effect. If, in such a case, we have two different responses, one in first position and the other in second position of recurrence, each will be raised to a relative strength of 1-to-0 compared with the infinitesimals; and the one in first place will be at no advantage over the one in second place."[10]

Without holding a brief for the law of primacy, we fail to see how these corollaries explain all the experimental findings. According to the first corollary the first member of a series is reported most frequently, and reported most frequently first, not because it is the first member encountered, but because it is the most strongly associated. But why is it the most strongly associated? If we read the second corollary correctly,

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[9] *Op. cit.*, 28.
[10] Thorndike, *op. cit.*, 28.

Thorndike holds that it is the most strongly connected because of the effect of frequency—a conclusion which corroborates Welch and Burnett's results.[11] But the effect of primacy, as Robinson and Brown[12] pointed out in answer to Welch and Burnett, appears even in those experiments that were specially designed to equalize the effect of frequency.[13] If the experiments were successful in this design, then the answer to the question must be sought elsewhere. Frequency will not explain primacy.

It was the hope of obtaining results significant for this problem that we worked over the data which we had previously gathered in our study on forgetting from the point of view of the effect of serial position.[14]

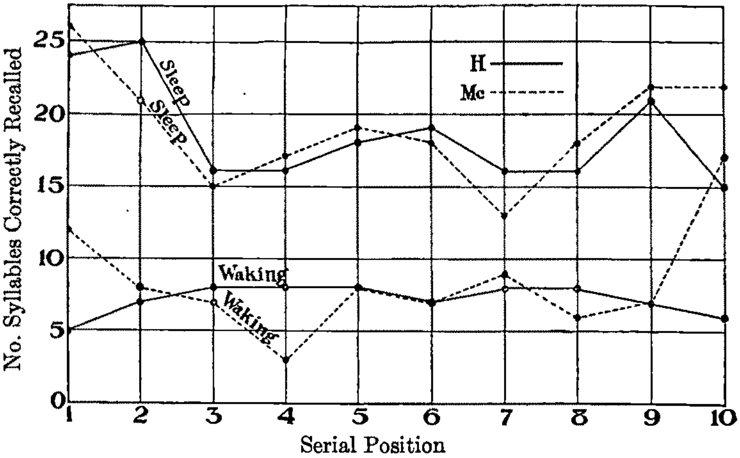

FIGURE I

Procedure. As the procedure is fully reported in the earlier study, to which the interested reader is referred for details, only the most important points will be mentioned here.

A series of 10 nonsense syllables was presented visually to two *O*s (H and Mc) until learning had come to the first correct recitation. The *O*s read the syllables aloud. Presentation was so rapid—at the rate of 0.7 sec.—that a review or rehearsal during learning was practically impossible. After learning the *O*s were instructed not to think of the series until they were asked to give a recall. After intervals of 1, 2, 4, and 8 hrs. of sleep or waking a free reproduction was called for. Except for 3 of the waking intervals, 8 experiments were completed at every one of these periods. Recall was always made in the experimental room with *O* seated at the apparatus in the position held during learning. *E*, the junior author, copied the reports as they were given orally by *O*.

Results. The total number of syllables correctly recalled by each *O* at every position within the stimulus-series and at every time interval during sleep and waking is shown in Table I. The total results, for each *O* and for sleep and waking, are also shown graphically in Fig. 1.

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[11] *Op. cit.*, 401. [12] *Op. cit.*, 543.

[13] W. G. Smith, *op. cit.*, 21-31.

[14] J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612.

TABLE I

Number of Syllables Correctly Reproduced at Every Time Interval after Sleep and Waking and their Serial Position within the Stimulus-Series

O	Recall after	Interval	Serial Position of Syllables Recalled									
			1	2	3	4	5	6	7	8	9	10
H	Waking	1 hr.	2	4	4	5	3	2	2	3	5	5
		2 hr.*	2	1	1	2	2	2	3	3	1	0
		4 hr.†	1	1	2	1	3	3	2	2	1	1
		8 hr.	0	1	1	0	0	0	1	0	0	0
		Total	5	7	8	8	8	7	8	8	7	6
	Sleep	1 hr.	6	6	6	4	7	6	5	5	8	4
		2 hr.	6	5	3	5	5	4	4	3	4	4
		4 hr.	6	7	4	2	4	3	5	4	4	3
		8 hr.	6	7	3	5	2	6	2	4	5	4
		Total	24	25	16	16	18	19	16	16	21	15
Mc	Waking	1 hr.*	6	3	2	2	3	3	2	1	2	5
		2 hr.	2	3	2	1	2	2	6	3	3	3
		4 hr.	3	1	2	0	2	1	0	1	1	6
		8 hr.	1	1	1	0	1	1	1	1	1	3
		Total	12	8	7	3	8	7	9	6	7	17
	Sleep	1 hr.	7	5	5	5	4	5	7	6	5	7
		2 hr.	5	6	3	3	6	3	3	4	7	3
		4 hr.	7	6	5	5	4	5	1	4	4	5
		8 hr.	7	4	2	4	5	5	2	4	6	7
		Total	26	21	15	17	19	18	13	18	22	22

*Results of 6 series. †Results of 7 series.

In computing the results partial successes were not counted; only those syllables correctly reproduced were considered. The associative fusions and partial successes were disregarded; such cases however, were so few in number,[15] that had they been counted the results would not have been materially affected.

The effect of serial position, as these data show, is different for the two *O*s. Mc gives results that we have learned to expect: the syllables which occupy the first and last positions are recalled more frequently than those in intermediate positions. This holds true for the recalls after intervals of both sleep and waking, but the relative importance of primacy and finality is different under the two conditions. During waking hours syllables in the final position are reported more frequently than those in the first position, whereas after intervals of sleep the opposite relation holds true. From Mc's results alone we should have to conclude that primacy and recency are effective in the recall, and that the relative importance of these two positions varies with sleep and waking.

H's results, however, are so very different that they negate whatever conclusions that we might at this point draw from Mc's results. H recalls, after intervals of waking, fewer syllables from the first and last positions within the stimulus-series than from any of the intermediate positions. The curve is highest in the middle and falls at the two ends. Although the absolute difference is small and the data are few in number, the results, as they stand, corroborate the findings of Welch and Burnett. After intervals of sleep, on the other hand, H shows a marked tendency in favor of the first part of the series. The curve of recalls is highest at the beginning and lowest at the end. If H's results were considered by themselves we should have to conclude that serial position was not effective after intervals of waking, and that primacy alone was effective after intervals of sleep.

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[15] *Op. cit.*, 608.

When the results of the two *O*s are considered together we are forced to Thorndike's conclusion that primacy (and also recency) is not in and of itself effective, and that whatever advantage it has it owes to other factors.

According to the first corollary of his general law of learning that Thorndike proposes, the specific factor which gives primacy its advantage is the greater strength of that associative connection. The question then resolves itself into determining what factor or factors gives this greater strength to the connections occupying the initial position within a series; and, specifically, what factors were present in the experiments with Mc, and in those with H after intervals of sleep, that were not present in the experiments with H after intervals of waking. Frequency—as we have already seen—will not account for it, because that factor, so far as we were able to determine, was eliminated by our experimental conditions; and even if it were not entirely eliminated it could not, in view of the similarity of Mc's results in sleep and waking, account for the difference between H's results in sleep and waking. The solution of the problem must be sought elsewhere.

TABLE II

Number of Syllables Correctly Reproduced after Sleep and Waking at Every Position within the Stimulus-Series and within the Recall-Series

O	Position within series	Recall after									
		Waking					Sleep				
		order reported					order reported				
		first	second	middle	next last	last	first	second	middle	next last	last
H	1	3	1	1	0	0	12	0	5	3	4
	2	4	0	1	2	0	2	6	6	4	7
	3	4	2	0	2	0	5	3	3	3	2
	4	3	2	0	0	3	0	2	12	0	2
	5	3	1	0	2	2	2	4	7	1	4
	6	2	0	1	0	4	2	1	7	4	5
	7	1	0	5	0	2	1	3	6	5	1
	8	2	1	0	1	4	1	3	6	4	2
	9	1	1	2	1	2	5	2	6	3	5
	10	1	1	0	1	3	2	4	7	2	0
Mc	1	12	0	0	0	0	24	0	1	0	1
	2	3	1	2	0	2	2	16	0	2	1
	3	2	1	1	1	2	1	3	9	1	1
	4	1	1	0	0	1	0	2	10	1	4
	5	1	1	1	1	4	1	1	13	1	3
	6	2	1	1	2	1	0	8	6	2	2
	7	0	1	2	2	4	1	1	8	1	2
	8	2	0	2	0	2	2	0	7	6	3
	9	2	0	1	2	2	0	1	6	12	3
	10	1	3	4	1	8	1	0	3	6	12

A casual inspection of the data showed that the *O*s very frequently, though they were not constrained by the instructions to do so, reported the syllables in the order in which they were exposed. To ascertain whether there was any relation between this form of report and the report of the initial syllable, we cast our results in sleep and waking, separately for each *O*, into the form of correlation tables. These are given in Table II.

In making these correlation tables, we divided the syllables in *O*'s reports into five groups, accordingly as they were reported first, second, in the middle, next to last, or last. A more extended classification was not practical, since the average number of syllables reported by H and Mc respectively was only 4.1 and 4.4.[16] When 6, 7, 8, or more syllables were reported, the first, second, next to last, and last syllables reported were entered under those headings, and the others under 'middle;' when only 4 syllables were reported, under 'first.' 'second,' 'next to last,' and 'last'—no entry was made under 'middle;' when only 3 syllables were reported, under 'first,' 'middle,' and 'last;' when only 2 were reported, under 'first' and 'last;' and when only 1 syllable was reported, it was entered as 'first' or 'last,' according as it fell within the first or last half of the stimulus-series.

Each *O* gave 4 of these single reports. All of H's cases, given in the waking experiments, fell within the first half of the stimulus-series—1 case at position 2, 2 cases at position 3, and 1 at position 5. Mc's single reports, likewise given in the waking experiments, were equally divided between the two halves of the stimulus-series—1 case in each of the positions 1 and 3, and 2 cases in position 10.

The coefficient of correlation between the stimulus- and recall-series, as computed by Adams' adaptation of Pearson's products moment method,[17] are, for H and Mc respectively, 0.37 and 0.38 for recalls after intervals of waking, and 0.72 and 0.77 for recalls after intervals of sleep. These coefficients, however, are not as significant for our problem as the distribution of the two arrays. If the arrays are studied from that point of view it will be noted that in the experiments in which the 'primacy effect' is evident the first syllable is most frequently reported 'first.' Mc, for example, recalls in the 'sleep' experiments the first syllable 'first' 24 times, once in the 'middle,' and once 'last.' Moreover, he recalls the first syllable 'first' 3 times as often as all the syllables in the other nine positions combined. He shows, in this series of experiments, a pronounced tendency to report the first syllable 'first.' A similar tendency is shown in his 'waking' experiments and in H's 'sleep' experiments.

The conclusion, which we draw from these results, that the 'primacy effect' is to be explained by this 'initial set' towards the first syllable, *i.e.* by the tendency to report the first syllable 'first,' is corroborated by the negative findings in H's 'waking' experiments. In this series of experiments, in which the 'primacy effect' was not obtained, the initial syllable was not reported 'first' as frequently as the second or third in the stimulus-series, nor more frequently than the fourth or fifth. There is no evidence of a 'set' or tendency to report any one position more frequently than any other, and the results verify this observation.

The only way, of course, of determining the accuracy of our conclusion is to appeal to introspection—which we are in the present instance unable to do; but since we can show that it explains the results which have thus far been reported, we think it very plausible. The serial mode of presentation will suffice, we believe, to arouse an 'initial set' toward the first syllable, and thus accent it and cause it to be reported more frequently than any other syllable within the stimulus-series.[18] In all the experiments upon this problem up to those of Thorndike, the stimulus-series have been presented serially; and, with the exception of the results of Welch and Burnett, the effect of primacy has always been noted. The single exception strengthens rather than weakens our thesis; for as long as the experiments in the Bowdoin laboratory followed the usual serial procedure, the primacy effect was noticed; but when instructions were introduced that cut across the tendencies usually established by the serial presentation, very different results were obtained. The differences cannot be explained, as we have shown above, by the presence or absence of the factor of rehearsal, but they can be explained in terms of our conclusions as follows: Welch and Burnett's instructions against the rehearsal of the early items not only destroyed the 'initial set' toward the first item that is usually aroused by serial presentation, but, in addition, they established an 'initial set' toward the last item. If this explanation is true the strongest connection would be with the last item; and this item in accordance with Thorndike's corollary would show its strength by being reproduced most frequently and most frequently first—and these are just the results that Welch and Burnett report.[19]

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[16] *Op. cit.*, 609.

[17] H. F. Adams, An easy method of determining the coefficient of correlation, *Psychol. Bull.*, 15, 1918, 456-459.

As we have already discussed Thorndike's experiments, we shall not return to them now; but aside from what we have said, we should, since he presented his materials in a random order, expect his results not to show the 'primacy effect.' If our conclusion is correct, a random presentation, everything else being equal, would always give negative findings.

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[18] The efficacy of 'initial set' was tested in a short series of class experiments in which the *Ss* were instructed to "report the 4th [5th, 6th, or 7th] syllable in the stimulus-series first." The syllable occupying the position to be named first was invariably reported more frequently than the syllables occupying the other positions—primacy and recency included. A result that any one could have predicted! Surely! Its importance to us is that it is a result contingent upon the 'initial set'—upon the tendency to report first, and it is just this factor, differing perhaps in degree but not in kind, which we believe is established by the serial presentation of the stimulus-series and is responsible for the alleged 'primacy effect.'

[19] *Op. cit.*, 398.

APPARATUS

AN INSTRUMENT FOR MEASURING THE BREADTH OF THE PUPIL

By C. E. FERREE and GERTRUDE RAND, Bryn Mawr College

There has long been a need in laboratory and clinic work for a convenient and precise means of measuring the size of the pupil and of studying its reactions. The importance of increasing our knowledge of the normal size of the pupil, its pathological deviations, and its normal and pathological reactions is too well recognized to require much attention here. It will be sufficient perhaps to point out that there is scarcely a function of the eye in which the pupil does not play an important part and scarcely a functional test in connection with which the pupil is not an important variable factor. The meager knowledge which we have of the pupil's reactions and of their effect on the various visual functions can not, therefore, be attributed to a lack of incentive for investigation. Our own research program, for example, contains a long list of problems, work on which has been delayed or not even begun because of the lack of an instrument which has the desired precision and flexibility of use.

Some of the essentials of such an instrument may be noted in passing. (1) Because of the diminutive size of the pupil, measurement should be possible to fractions of a millimeter within a small percentage of error. (2) The method of making the measurement should not reduce the amount of light entering the eye, or in any other way disturb the reactions of the pupil to the conditions being studied. (3) It should be possible to continue the measurement without interruption through a considerable period of time. (4) Measurement should be possible over a wide range of intensities of illumination from low to high. (5) The instrument should be easily and readily adaptable to a wide range of conditions to use.

The pupillometer to be described fulfills the above requirements to a reasonably satisfactory degree and is extremely simple in principle and construction. It consists of a telescope provided with a measuring scale extending diametrically across the field, and the auxiliary devices for adjustment needed for its precision and convenience of use. The telescope is furnished with a 12.5 cm. achromatic objective, 3.2 cm. in diam., and with a Ramsden eye-piece. The measuring scale is ruled on a disc of thin glass, 70 divisions to the centimeter. This scale is located in the image plane of the objective lens and is therefore in the same plane as the image of the pupil to be measured. Image and scale are thus magnified the same amount by the eye-piece and the breadth of the pupil can be read directly in terms of scale divisions. The telescope is provided with a range of focus which permits of its use at distances ranging from 25 to 200 cm. The scale must of course be calibrated for the distance at which the instrument is used. This can be quickly and easily done by viewing a millimeter scale at the distance chosen and determining the relation between the two scales. In order to give a high visibility to the scale divisions as seen against the dark pupil as background, an independent source of illumination is needed for the scale. The illumination of the scale is provided for as follows. A straight edge is ground on the disc at one end of the scale. This edge is illuminated by a miniature lamp inserted in the tube shown in Fig. 1. The light passing through the glass between its two surfaces and specularly reflected from the scale divisions, renders these divisions luminous and plainly visible without impairing the distinctness of the image of the iris and pupil. In order that the illumination provided for the scale may not be too high when the pupil is viewed under a low light and conversely, this lamp is connected in circuit with a rheostat the coil of which

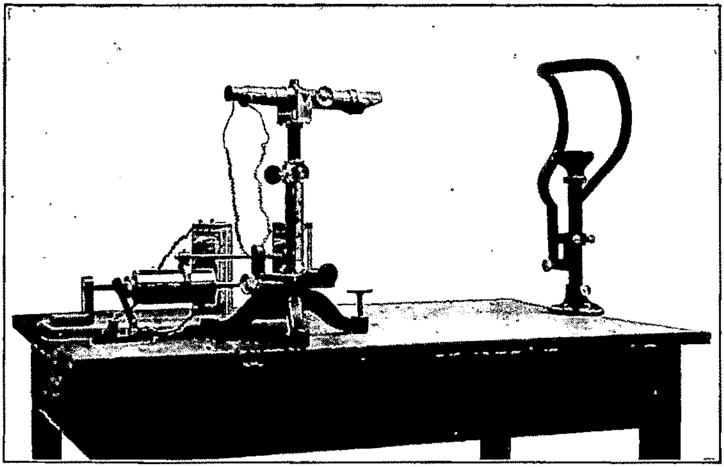

Figure I. Pupillometer

rotates under the contact to give small changes of resistance. The mount containing the scale is also made to rotate. This provides for the measurement of the pupil along any diameter. If viewed from a position slightly to either side of the line of sight, the vertical diameter of the pupil should be measured; if viewed from a position above or below the line of sight, the horizontal diameter is measured.

Often in the use of a telescope of this type the visibility of the object viewed can be increased considerably by shielding the objective lens from extraneous light, particularly when there is a strong component of illumination in any direction as occurs, for example, in overhead lighting. A shield for this purpose, shaped from a section of tubing and blacked on the inside, is shown in the figure. It is held in position by a ring at one end which slips over the end of the tube of the telescope. This shield can be rotated to cut off the light from any direction and can be attached and removed at will.

For the convenience of use of the instrument the following adjustments are provided. The stem supporting the telescope is screwed to a plate which can be moved to right or left in a grooved housing by a triple-pitch screw, shown in Fig. 1. This housing is in turn fastened to a plate which can be moved forward or backward in a similar housing by a second screw. The first of these adjustments provides for the placement of the scale on the image of the pupil at divisions convenient for reading, and for following slight movements of the eye to right or left; the second, for the precise regulation of the distance of the telescope, set for a fixed focus, from the eye. The telescope itself is mounted in a collar provided with two short studs or bosses extending out from either side. These bosses rest in adjustable friction bearings in the U-shaped housing shown in Fig. 1. This type of mounting permits of tilting the telescope up or down, as may be desired, in viewing the pupil from below or above. The telescope can be raised or lowered in the usual way by means of a rack and pinion. Finer movements are provided for by a levelling screw as shown in the figure. By means of these adjustments the instrument can be used in a variety of positions without interfering with the use of the eye or with the reactions of the pupil to the conditions under which the eye is being used.

The instrument is simple and inexpensive to construct. We have found it to be a very valuable addition to our optic and ophthalmic equipment. Of so great a value is an instrument of this type that it has been our hope that the measuring scale and the device for its illumination will be added to the telescope which at present forms one of the auxiliary attachments to the perimeter which we have described in earlier papers.[1]

The expense of this addition would be slight and the pupillometer would constitute a valuable adjunct to a perimeter, particularly in the study of pathologic cases. Size of pupil is also one of the factors which makes perimetric determinations differ from individual to individual in case of non-pathologic eyes. That is, it is one of the factors which should be specified in forming a diagnostic scale and be allowed for in rating a case in terms of such a scale or other device used for classification. A foundation structure similar to a perimeter would be of great advantage too in furnishing the conditions and controls required for many of the purposes for which a pupillometer is needed. The details of the combined instrument—its needs and advantages—will be discussed in a later paper.

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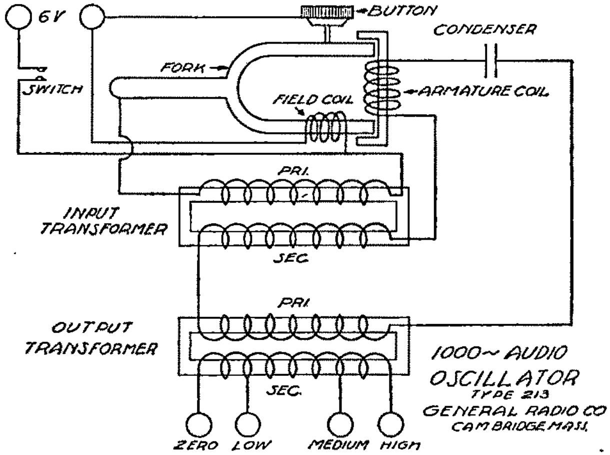
[1] C. E. Ferree and G. Rand, A new laboratory and clinic perimeter, *J. Exper. Psychol.*, 5, 1922, 46-67; Some contributions to the science and practice of ophthalmology, *Int. Cong. Ophth.*, 1922, 488-525; Further contributions to the science and practice of ophthalmology, *Trans. Ophth. Soc.*, 45, 1925, 815-847.

THE AUDIO-OSCILLATOR

By H. M. Halverson, Yale University

For some time the writer used in the laboratory at the University of Maine audio-oscillators for the production of tones of medium pitch (600-1400 d. v.). These tones may be sustained indefinitely without change of intensity and are practically free from harmonics. A note on the instrument for the information of psychologists therefore seems desirable.

The audio-oscillator is essentially an electric tuning-fork and two transformers (see diagram). Unlike the ordinary electric tuning-fork, this fork is operated noiselessly without a spark gap by a microphone button (marked 'button' in the diagram), which is vibrated by one prong of the fork and varies the resistance without a spark and its attendant difficulties. This circuit passes around a coil that encloses the other prong of the fork with enough air-space to permit vibration. A hooked armature is placed about the ends of the prongs, and it is possible that the fork would vibrate thus without further connections, with the armature simply completing the magnetic circuit created by the field.



Actually, however, the armature is itself magnetized electrically. The primary coil of the 'input transformer' is shunted about the fork, and hence carries a current fluctuating as the microphone current fluctuates. The secondary circuit of this transformer is wound about the armature to produce opposite polarity with the fork, and, since the fluctuations are synchronous with the fluctuations in the field, the action is enhanced. Capacity is introduced into this circuit in order to establish electrical resonance for the frequency of the fork.

The secondary circuit of the 'input transformer' is also the primary circuit of the 'output transformer.' From the secondary circuit of the output transformer four leads pass to binding-posts, making possible three voltages and therefore three intensities in the telephone receiver that can be attached.

The instrument is best used with a receiver, although the fork without the receiver gives a fairly loud tone which can be heard for 25 ft., but which can not be varied in intensity. If it is especially desired to omit the receiver, the sound can be satisfactorily conducted to ear-pieces through a rubber tube if the end of the tube is placed near one tine of the fork.

This instrument is the audio-oscillator, type 213, manufactured by the General Radio Company, 30 State St., Cambridge, Mass., and described in their Bulletin 713, January 1925. It is ordinarily made for 1000 d. v., and comes in a cubical box about 6 x 5 x 5 in., weighing 4½ pounds. The stock article is sold for $32, but the manufacturers can make to order frequencies from 600 to 1400 d. v. at little additional cost. It is obviously possible to have more leads brought out from the 'output secondary,' so that a larger number of intensities can be obtained.

The primary circuit requires a storage battery of 4-8 volts.

ELECTRICAL CONTROL WITH THE METHOD OF CONSTANT STIMULI

By J. M. McGinnis, University of Maine

It is well known that judgments in the method of constant stimuli are made quickly and that the progress of an experiment is limited principally by the time required to present the stimuli. The rotating table of Urban and Fernberger was designed to facilitate presentation, and also to reduce the time-interval between the standard and the variable stimulus. It is well, however, to call attention to the fact that many stimuli can be presented electrically and that an electrical control with the proper switches is a very convenient and efficient method of presentation.

The writer has been using such a control for the presentation of pairs of auditory intensities, established electrically in telephone receivers. He has found that a very satisfactory arrangement consists of two circuits, one for the standard and one for the variable. The circuit for the variable includes a rheostat with point contacts, so that the setting for the variable is made simply by moving the arm of the rheostat. The circuits are respectively connected with the mercury cups of the metronome, which, when thrown into circuit, presents the standard and the variable successively with a constant time-interval determined by the rate of the metronome. By inserting between the metronome and the two stimulus-circuits a double-throw double-pole switch, diagonally cross-connected, or any similar commutator familiar in the technique of the reaction experiment, it is possible, by throwing the switch, to reverse the time order of the variable and the standard. The two sides of throw of the switch can be labelled 'variable first' and 'standard first,' and the changing of time-order can be made readily and noiselessly.

By this arrangement it is possible to obtain approximately 200 judgments from an observer in a 45-minute session.

It is obvious that the arrangement is adaptable to any experimental situation where the stimuli can be presented electrically. If the variable changes with discrete stimuli (instead of electrical resistance), a multipoint switch may be substituted for the rheostat. Any time apparatus that will give successive contacts may replace the metronome, *e.g.* a pendulum or a rotating drum with contacts.

In the experiment mentioned a circular radio switch with 7 contacts was used. A spring under the shaft makes it possible to lift the knob in turning, so that the setting of the switch is noiseless and does not afford secondary criteria. It seems probable, however, that slightly more uniform contact will be obtained by a heavier dial switch such as used on resistance boxes.[1]

The principle of the commutator switch is well known. The metronome leads pass to the knife blades and the two circuits are taken off from two points at the one side, each of which is connected diagonally to the opposite point.[2]

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[1] Such a switch is part of 'Reaction Apparatus,' catalogue no. 23301, C. H. Stoelting Co., Chicago.

[2] A convenient form is the 'Block Handle Pohl's Commutator,' Knott and Co., Boston.

BOOK REVIEWS

Cosmic Evolution. By JOHN ELOF BOODIN. New York, The Macmillan Co., 1925, pp. xi, 484.

It is probably due to the theory of relativity and the remarkable discoveries of the new physics that there has been so great a revived interest of late in philosophy. Mathematicians and physicists are, after the manner of philosophers, actually studying the universe as a whole, seeking some principle of unity in it. Psychologists, too, no longer pride themselves on limiting their studies to mental phenomena. They are trying to find out what intelligence *is*, or even what the mind *is*, and have made remarkable progress in this hitherto philosophical task.

The new book by Professor Boodin supplements his *Realistic Universe* by a more explicit attempt to do just these two things—namely, to get some unitary conception of the world as a whole, and to bring together some definite conclusions respecting the philosophy of mind. A further section of the book deals very clearly with the theory of relativity for non-mathematical readers.

As regards the mind and its relation to the body, Professor Boodin's book recalls to us how far we are getting from all the old interaction, double-aspect, and parallelistic views. What we have is not mind and body, but a *minded body*, a minded organism. "Mind and body," suggests an artificial bifurcation. Mind "marks a new level of organization, a new quality of behavior, a new pattern control in which the whole organism functions differently from the levels of reflex and habit." "Meaningful behavior is a new level of organization."

But all of this, it appears, is just as far from physiological materialism as it is from psychological idealism. If it is a question of reality, mind is quite as real as matter. "The mental level superimposes a new set upon the whole hierarchy of physical mechanisms." "The physiological organism with its hierarchy of structure does not function in the same way when it is in integral relation with the mental level as when it is dissociated from that level." Minded behavior is thus a new actuality, "a creative addition in the course of evolution of a new pattern control, a new energy system, not resolvable into the categories of physico-chemical, nor into those of neural systems."

When Professor Boodin gets into his metaphysical swing with his ambitious cosmical system of hierarchical control, the going is not quite so easy. Yet his sweeping guess at the method and meaning of the universe is also interesting. The universe is a sort of harmonic system in which every part is in integral relation with every other; the lower levels being controlled by the higher as much as the higher by the lower. We wonder how matter can enter into higher and higher forms of organization. We wonder at the creative synthesis which is somehow displayed. But this is all "due to its moving within the field of cosmic control, to its interaction with the larger universe." Cosmic evolution is not due to any fortuitous concourse of elements but is ever under the control of the cosmic field.

Reality is "a rhythm of multiple histories, which exist at different levels and which interact." There is therefore a cosmic guidance of evolution which is "the incarnation of the energy patterns of the cosmic environment into material bodies." If there is an evolution towards life, mind, goodness, beauty, divinity, it is because these are all active throughout the cosmos, stimulating every part in their own direction. Thus "the world as we know it is a pluralistic world of multiple energy patterns, histories, and levels."

So it comes about that neither materialism nor a psychological idealism do justice to the complexity of the world. "The claims of idealism are given due recognition if it is shown that life, thought, the sense of beauty, communion with God, are eternal and intrinsic aspects of the whole of reality and that the higher levels exercise a measure of guidance over the lower, while they also require the lower for their realization."

University of Iowa G. T. W. PATRICK

Biological Memory. By EUGENIO RIGNANO. Translated with an introduction by E. V. MacBride. New York, Harcourt, Brace & Co., 1926, pp. vi, 253.

"The final result of all action is really present from the beginning in the form of a mnemonic accumulation: the medium, or the environmental relations towards which the animal strives, operate now like a *vis a fronte* solely because they have formerly been a *vis a tergo*, and because the physiological activities which they have then aroused in the organism have left behind them a mnemonic deposit, which now acts like a veritable *vis a tergo* in moving the living being. Every manifestation of purposefulness, from ontogenetic development, which builds up organs which can only be used in the adult state, to the property of physiological states in general, corresponding to certain environmental conditions, to become active at the first appearance of phenomena which ordinarily precede, but do not constitute, the whole of these environmental conditions; from the perfect morphological adaptation of the organism to the environment, completed before the environment has been able to exert its formative action, to the wonderful arrangements and specialized structures so nicely calculated in view of certain probable conditions to which, in the future, the organism may be exposed; from simple reflex actions 'mechanized' for the purpose of the preservation and prosperity of the individual to all the most complex instincts by which animals provide beforehand for future conditions of which they know nothing: all these purposeful aspects of life can be explained as so many manifestations of mnemonic nature."

The above quotation contains the gist of the thesis (and exhibits, as well, the style of the translation) which Rignano elaborates in the present volume. The work may be considered as an extension and clarification of positions already developed by the author in his books, *On the Inheritance of Acquired Characters* and *The Psychology of Reasoning* (English translations 1911 and 1923 respectively).

Biologists have been confronted for a long time by three dilemmas. (1) Is development effected by preformation or epigenesis? (2) Is the germ-plasm composed of preformistic gemmules or is it made up of substances which are altogether devoid of the power of representing and of determining each a single morphological character or a special process of development? (3) Is there specialization of nuclei or is nucelar division qualitatively equal? Many important and impressive experimental facts may be adduced in support of both sides of all three of these questions—hence the dilemma. But since—in view of the factual evidence—each side of the different questions is about equally tenable, Rignano suggests that the problem has been wrongly stated, or, what is more likely, that the dilemmas are really non-existent. The facts on both sides should be envisaged in the framework of an hypothesis which shall be able to resolve the apparent conflict. Such an hypothesis is the one which Rignano presents under the name of the centro-epigenetic doctrine.

Centro-epigenesis tries to meet the above dilemmas by postulating (1) the existence of a 'plasmatic' or nervous energy which proceeds from a special part of the organism (central zone of development); (2) the presence in the germ-plasm of a number of specific accumulators which are capable by discharging themselves of giving rise to specific deposits of energy and which become active one after another in a definite sequence from the beginning of development until its completion; and (3) the accretion to nuclei outside the central zone of elements of specific potential different from the original ones. These processes of nervous action and accumulation are related to mnemonic phenomena, or more strictly, all mnemonic (in the narrower sense) and vital phenomena are special cases of nervous action and accumulation, which are mnemonic (in the most general sense) in nature, in that each discharge gives rise to a nervous current of a definite specific character and that the substance which produces this current is the same substance which this current can and does in its turn deposit when acting as the current of charge. This property confers on the accumulators a true mnemonic nature.

With this hypothesis as a working basis, Rignano proceeds to a treatment of the mnemonic character of the purposefulness of life. Every organism strives to maintain a physiological equilibrium or to restore it every time it is disturbed by a change supervening in either the external or internal environment. To each factor in the environment which is capable of preserving or restoring the normal condition there corresponds an impulse of 'longing,' 'desire,' 'attraction,' etc. To these states of the organism Rignano applies the term 'affective tendencies.' The primary affective tendency has as its function the maintenance and restoration of the physiological equilibrium. The author gives an elaborate account of the manner in which the number of environmental factors which serve to maintain a normal condition of the organism is ever increasing, and of how factors towards which or away from which the organism strives operate like a *vis a fronte* solely because they have formerly been a *vis a tergo*. The secondary affectivity serves as a check upon the primary in order that the latter may not lead the organism into harmful situations. The proper balance between these two affectivities results in a caution which is beneficial in maintaining the most satisfactory relation with the environment. The same affectivities are made use of by the author in explaining higher mental activities in man, such as attention, reasoning, and intelligence. Reasoning, for example, turns out to be a series of inter-connected experiments *conceived*, but not actually performed. If they were performed they would conform to the operation of the two affectivities, the primary one urging the individual on to discoveries which shall bring a satisfactory *mental* equilibrium and the secondary one acting as a check upon the primary in order that errors may be avoided.

The psychologist will find the latter part of the book, together with the author's *Psychology of Reasoning*, of very great interest. Attempts to account for higher thought processes in terms of mental sets, neural dispositions, and other vague determinants are familiar enough. Here we have an effort to follow the lead given by these vague phrases. Rignano has started with simple principles derived from the observation of the characteristics and behavior of lower organisms and has given a consistent picture of the evolution of behavior and consciousness within the framework of these same principles.

Harvard University C. C. PRATT

Mind and Its Place in Nature. By DURANT DRAKE. New York, The Macmillan Company, 1925, pp. xv, 259.

The work, written from the viewpoint of *critical realism*, is epistomological in that an attempt is made to answer questions concerning the possibility of knowledge under the general limitations of conscious states, and metaphysical in that a theory is set up for the nature of mind in a setting of objective reality. The author holds that the denial of the existance of an "outer" world is no more justified than the denial of other minds, that there can be no logical evasion of solipsism if we abandon the existence of an objective reality. The author attempts to show in what way and under what limitations consciousness might be considered as knowledge of an objective reality. He holds that there are certain aspects in the sequence of mental life which warrant the assumption of controlling conditions from an objective world. The fact that part of our mental operations are susceptible to the knower's will, and other parts proceed on their course in spite of the knower's will, is one suggestive fact. The coherence and predictability of certain aspects of mental life, which reaches a precise organization in spite of the known laws of perceptual illusion, warrants the belief that certain phases of conscious processes constitute knowledge about objective reality. Our knowledge of other minds is just as firmly limited to spatio-temporal terms as our knowledge concerning material objects. The fact that some other observer sees my mind only in space and time factors does not in the least prove that my consciousness, which is unobservable to him, does not exist.

The author seems to object to the pragmatic test as the ultimate determination of belief in objective reality. He says, "It would not be accurate to say, as some pragmatists have said, that verification made our beliefs true. If they were true at the time of verification, they were true before the verification, and would have been true if they had never been verified." In the face of this apparent denial of the pragmatic test Professor Drake seems to rest the validity of his theory of mind on the fact that it works. It is difficult to see in what way he has escaped the pragmatic test after all. It would seem that he has drawn too sharp a line between belief and the test for belief, a line which probably had its origin in the precise demarkation of the various parts of the reasoning process by the older logicians, and which has been abandoned by the modern psychologists writing on the actual nature of reasoning. The pragmatic test was written by a psychologist. The smooth and successful operation of the mental processes, accompanied by pleasant feeling-tone, is *both* a condition for belief and *at the same time* the pragmatic test.

For the psychologist, interested in the functional point of view, Professor Drake's theory of consciousness as a function of an objectively physical organism, a qualitative aspect of an objective world otherwise measurable only in spatio-temporal terms, is amply satisfying. He has considered, in a more adequate manner, the facts of psycho-physical relations and has presented a really valuable contribution by showing that the philosopher has never been free from psychological determinants.

University of Michigan ADELBERT FORD

The Language and Thought of the Child. By JEAN PIAGET. Translated by M. Warden. New York, Harcourt, Brace & Co., 1926, pp. xix, 246.

Piaget here reports the results of a study of the language, conversations, explanations, and questions of children. The methods used are all modifications of a basic plan consisting in taking verbatim reports of the conversations of children under natural conditions. The author is a careful student and displays great ingenuity in classifying his results and in finding general principles exhibited by them. The reader will be pleased also by the fact that Piaget is not given to the weakness so rampant among psychologists of today which consists in building up personal systems with a lordly disregard for the results of others. He, a biologists of distinction, is versed in 'knowledge about knowledge' and exhibits a caution which one would expect to find only among veteran students of syllogistic thinking and of epistemology.

At first though it might seem a hopeless task to bring order out of the chaotic talk of children. Piaget, however, is ingenious. His primary principle, as Claparède points out in an introduction, is the theory that the thought of the child evolves from an ego-centric and autistic type to the logical and socialized type of adulthood. His facts certainly support his principle. Analysis of the sentences of the 6 year old child shows dominance of ego-centric speech of which he finds three types: (1) repetition (echolalia) a "remnant of baby prattle" in which the child repeats words and phrases oblivious to hearers and to meanings; (2) monologue, "the child talks to himself as though he were thinking aloud," he addresses no one; and (3) dual or collective monologue, "where an outsider is always associated with the action or thought of the moment but is expected neither to attend nor to understand." Of socialized speech, Piaget enumerates 5 types. Formulae are also developed to measure the index of ego-centric language and to give the index of understanding.

Chapter I is a detailed study of 2 children. In Chapter II Piaget's principal thesis is confirmed by the results from 20-odd children between the ages of 4-7 years. In Chapter III, a study is made of 30-odd between the ages of 6-7 years regarding their ability to understand one another. One child is given a story or an explanation of the workings of some simple mechanism to repeat to another. A study of hundreds of these reports reveals the ego-centric character of the child's thought. The child fails so miserably in communicating what he understand because he disregards the 'how' of phenomena, substituting for it some autistic schema of his own. To become mentally mature is to realize that thought must be conveyed in categories which others can understand. This does not appear in the child until about the age of 8 years.

A proverbs test given to 70 children (boys and girls, 9-11 years of age) exhibits 'verbal syncretism,' in which a child having one proverb in mind finds a 'corresponding' one by fitting both into a semi-archaic schema of his own. These results especially are wonderful devices for exhibiting the intimate workings of the child's mind. If the child does not understand the proverb, that makes no difference to him, he must justify himself, so he pairs proverbs on the basic of the most bizarre and unseeming associations of verbal resemblance. As usual Piaget relates his findings to attained facts, *e.g.* Decroly's success in teaching words before the alphabet, Bergson's criticism of associationism, and the results of the linguistic studies by Schuchart, Lelande, and Cousinet.

Wells College C. O. WEBER

Note Sopra la Originalita del Pensiero. By LEONE VIVANTE. Rome, P. Maglione and C. Strini, 1925, pp. 295.

From the theoretical point of view, especially in connection with the ever-recurring problem of the mind-body relation, this book makes a valuable contribution. As has been the usage in our own language, the author assumes under the term "thought," a great variety of conscious activity, somewhat in the sense of James's "stream of thought" or his still wider use of the word "feeling." This is, however, not the best use in present-day experimental psychology, but theoretically permissible in books of semi-psychological persuasion. The specific problem which confronts the author is the best explanation of a 'free' mind that is at the same time conditioned by a physiological system. The initial cause of mental processes is an original synthesis which assimilates to itself either initially or progressively in time a series of material conditions that become intentionally regulative but never constitutive. Externally these conditions appear to be causal, but in reality they are only secondary though essential concomitants. The doctrine is a form of idealistic monism. The synthesis is experienced directly, therefore it is not an abstraction, The problems of time and space afford an unfolding of the true essence of consciousness. Often the medium through which consciousness expresses itself whether it be the body itself or a product of bodily movement like language is quite inadequate to the reality of consciousness that lies behind it. Detailed examples of this fact are given throughout the book especially from the poetic literature of Italy. While the presentation of his point of view labors somewhat at times and at other times appears more romantic than scientific in attitude, this difficulty in making his experiences or "thoughts" clear may itself be the best illustration of the point that he is trying to make. No doubt, too, the double loss of meaning through translation into another language still further obscures the issue. On the whole the book shows much serious preparation for the task in hand and many novel insights into the problem.

University of Iowa — Christian A. Ruckmick

Das Stottern (*Assoziative Aphasie*). By Dr. Emil Froschels. With 12 figures. Leipzig and Vienna, Franz Deuticke, 1925, pp. iv, 134.

The author secures his evidence on the nature of stuttering and stammering from clinical cases secured in private practice. After reviewing the various theories in relation to symptoms he openly disagrees with those of his associates who have previously held that such speech defects are to be considered as a disturbance of the reflex centers. The prevalent classification of stammering disturbances into tonic and clonic types is not supported, he says, by clinical examinations. Often the patients exhibit a mixture of both tonic and clonic convulsions, or may change from one to the other without warning in successive disturbances. The same subject, repeating a given sentence over and over, does not repeat the same type of stammering movement, but shows constant variation in the form of the stammering convulsion. Patients exhibit a tendency to avoid certain particular words consistently, to invert the order of words in sentences containing these difficult words, and to show the motor abnormality most when they are conscious of being observed by friends or other listeners. When the subject can be induced to forget his self-consciousness the abnormality quite often disappears.

Dr. Froschels says he follows a suggestion of Hoepfner that the motor manifestations to be observed in stuttering are to be considered as an abnormal functioning of the voluntary controls rather than a disturbance of the reflex centers. The diffuse motor concomitants are to be classed as willed acts and the origin of the speech defect is in the higher centers more directly concerned with the manifestations of consciousness. The cause of the disturbance goes back to atactic conditions in the learning of the motor coördination of speech and becomes aggravated by affective elements—shame of displaying the abnormality before the social group. Associative inhibition is established between the idea of the movement and the thought expressed by the words to be uttered. In this conflict repressions occur for a portion of the words in the patient's vocabulary and this accounts for the consistent avoidance of certain particular words. A review is given of the contemporary methods of curing stutterers by means of exercises, rhythmical stimuli, etc., and this is followed by the author's psychoanalytic method of diagnosis and treatment.

University of Michigan — Adelbert Ford

Crime and Custom in Savage Society. By Bronislaw Malinowski. New York, Harcourt, Brace & Co., 1926, pp. ix, 132.

This book is a field worker's study of the laws and customs of the people living in a single ethnographic province—north-west Melanesia. These Melanesians inhabit an archipelago of flat coral islands which lie to the north-east of New Guinea. Malinowski has the virtue of reporting details and of illuminating his facts with a constant fire of judicious evaluation. He does not abjure theory, but he does criticise the academic tendency to simplify the account of savage life. This tendency expresses itself, for example, in the wide-spread notion that the life of the savage is dominated by automatic slavish, and unwitting submission to tribal custom and taboo, the view supported in the writings of Hartland, Rivers, Lowie, and others. The author's study of the Melanesians is of great psychological interest. He shows that these savages live in a surprisingly complex network of law, motive, inducements, and evasions. It is in vain that any one should seek to make routine machines out of them, moved by only one or a few drives. He demonstrates, in the major incidents of their lives (marriage, ownership of property, religious and family duties, death, suicide, crimes of other sorts, mourning, etc), that the notion that their life is dominated by mere gregarious instinct and unwritten communism is mistaken.

A superficial view might lead to the notion that ownership of property is communistic. Closer examination shows that, on the contrary, there is marked individual ownership. To listen to the testimony of these savages, one might well get the idea that mother-right, the unwritten tribal laws regarding incest, and so on, are things they would never think of violating, but, considering their practices, we find recognized, sophisticated, and effective subterfuges and evasions for violating these confessed philosophies. Far from being like a communistic hive of bees, individualism is rampant, the ego-centric element manifesting itself in the struggle to excel in wealth (the ownership of yams), in generosity, in sexual prowess, etc. Suicide among these people, is often merely an inability to endure life because of the memory of personal insults. Malinowski represents these savages neither as the care-free children Rousseau describes, nor as the hide-bound slaves to custom that Havelock Ellis portrays, but as people very much like ourselves, with constant friction between the demands of individualism and the demands of communism.

Wells College C. O. WEBER

The Physiology of Mind: An Interpretation Based on Biological, Morphological, Physical, and Chemical Considerations. By F. X. DERCUM. Philadelphia, W. B. Saunders Co., 1925, 2nd. ed., pp. 287.

In the preface the reader is informed that "the object and scope of this book is to present the basic facts of those reactions of the organism to the environment which under given conditions manifest the qualities which we speak of as 'mind.'" Physico-chemical processes are said to account for reactions called mental; the assumption of a psyche is unnecessary. "In any event psychology can only be regarded as a department of brain physiology."

In the early chapters the author presents in a simple and direct manner some of the more important facts regarding the development of the nervous system from the diffuse to the synaptic type. Receptors have been differentiated to respond to specific types of stimuli and protoplasm is said to be 'transparent' to many forms of energy. Stimulation is an automatic and mechanical process and transmission of excitation along the neurones is electrical in nature. The hypothesis that the neurones move is discussed.

The treatment of consciousness is generally too dogmatic and speculative to be of abiding scientific worth. Terms such as sensation, perception, thought, consciousness, etc., are used without definition and the great mass of factual material which constitutes modern experimental psychology is almost wholly ignored. Physical and mental concepts are jumbled up together and their relationship to each other is left in obscurity. If one would write a physiology of mind, one needs first to examine critically the phenomena called mental and to present adequately the facts whose physiological interpretation is desired.

After discussing consciousness Dercum returns to the more objective material but consciousness remains as a fly in the ointment. Loeb's theory of the tropism is accepted but in discussing instinct consciousness is invoked as an explanatory principle. A chapter on pleasure and pain presents a little speculation in which pain is said to be related to the destruction of the neurone molecule. A final chapter entitled "The Nervous System in the Light of Einstein's Interpretation of Energy" points out that living substance is part of the universe and that principles of explanation valid in the physical sciences are probably also valid in biology.

There are two addenda: a chapter on the pathological physiology of mind and an appendix on Freudism. The latter is a clear and brief criticism of psychoanalysis and the 'Freudian mysticism.' The book as a whole is very readable and in many places thought-provoking, but it does not represent any genuine advance in physiological psychology for the reason that its psychological perspective is inadequate.

University of Illinois PAUL THOMAS YOUNG

Learning to Typewrite. By W. F. BOOK. New York, Gregg Publishing Co., 1925, pp. 463.

The first sentence in the preface states that this book "will appeal most strongly to teachers of typewriting and to those preparing to direct learners of this art," and so it will. Although the author elsewhere in the preface suggests that the book might be used in classes in educational psychology "as a text for the intensive study of the learning process," the psychologist will find that it is far too repetitious to serve as a college textbook. By the device of calling the learning process different names, the author goes over and over the same ground. Chapter 1 of Part I is called "Psychological Basis for the Acquirement of Skill;" the same laws are treated in Chapter 2 under the heading, "General Nature of the Learning Process;" an Chapters 3 and 4, the same principles reappear under the captions, "Laws which Control the Origination and Fixation of Habits in the Acquisition of Skill," and "Laws which Control the Selection and Improvement of Habits in Learning;" and finally, Chapters 5, 6, and 7 take up the same process under the title "How Habits are Originated," etc. Thus Part I, which contains 154 pages might easily and profitably have been reduced to one-half or one-third that compass.

Part II, "Scientific Analysis of Learning to Typewrite," is the heart of the book and is thorough and informing. Part III, "The Rôle of the Teacher in Selecting and Directing Learners of Typewriting," is for the most part a repetition from a slightly different angle of the material in Part II. For example, there is little difference in treatment between "Proper Attitudes towards Success" (Part III, pp. 358 ff.) and "Influence of the Learner's Mood and Attitude towards his Task" (Part II, pp. 302 ff.). This re-hashing of the material is typical of the book. This very weakness of inordinate repetition may not be a fault if one considers that it will serve to establish certain habits on the part of teachers of typewriting.

The learning process as described in the book is familiar to psychologists. The principles of habit formation put forth are those that James made popular; Book gives us nothing new. The rôle of will and incentive in learning is repeatedly emphasized. The original experiments referred to, especially those undertaken to determine the voluntary motor ability of the world's champion typists, are exceedingly well reported. The book will be useful not only for teachers of typewriting, but also as a reference for all who are interested in the learning process.

Louisiana State University PAUL CAMPBELL YOUNG

The Measurement of Emotional Reactions: Researches on the Psychogalvanic Reflex. By DAVID WECHSLER. New York. Archives of Psychology, No. 76, 1925, pp. 181.

Dr. Wechsler has enjoyed exceptional opportunities in working both in Paris with Professors Lapicque and Piéron, and at Columbia with Professor Woodworth. His book reflects these opportunities. Not only does it provide a clear, complete, and concise exposition of the basic facts concerning the psychogalvanic reflex, but it further supplies much new material secured by the writer from his own experiments in this country and abroad.

In an introductory chapter the author sketches briefly but with ample documentation the development of the methods of expression in the study of emotions, and of the method of the psychogalvanic reflex in particular. In the five succeeding chapters he defines the psychogalvanic reflex, describes the technique used for its measurement, discusses its physiological aspects (conductivity of the skin, etc.) and proposes a theory to account for the phenomenon. The two next chapters are entitled "Psychological Correlations" and discuss the relations between the psychogalvanic reflex and mental processes. One chapter is devoted to clinical and practical applications. A final chapter contains a general summary of the work and some generalizations with respect to the nature of affection and emotion. A bibliography of 120 titles is appended.

In the new material derived from Dr. Wechsler's personal experimentation, particular significance attaches to his results on the relation between intensity of stimulus and intensity of reflex, and those on the specificity of emotional reactions. With respect to the first of these questions Dr. Wechsler finds that "the psychogalvanic reflex appears to vary roughly as the intensity of the affective tone elicited. It does not vary as the intensity of the stimulus, except in so far as increasing the intensity of the stimulus tends to evoke a proportionately greater affective reaction" (p. 166). With respect to the latter of these questions he finds that "results from a number of intercorrelations between various types of stimuli would indicate that individuals tend to be *specifically emotional*, that is, react emotionally to specific types of situations rather than uniformally to all types of situations" (p. 166).

By no means least among the many merits of this book is the fact that it is by an experimentalist for experimentalists. In his description of technique Dr. Wechsler does not confine himself to generalities, but supplies detailed information concerning electrodes, wiring, recording of deflections, etc., which cannot but be of value to anyone undertaking a piece of research which involves the use of the psychogalvanic reflex.

Harvard University J. G. BEEBE-CENTER

Beginners' Logic. By R. H. DOTTERER. New York, The Macmillan Co., 1924, pp. 335.

It is the opinion of the reviewer that Mr. Dotterer has given a treatment of elementary logic somewhat superior to that which commonly occurs. He has included very little irrelevant discussion of psychological matter, and has confined himself fairly closely to a discussion and analysis of logical facts. One is not to be misled by his definition "logic is the science of valid thinking." The book is not about thinking. It is a difficulty with all books on elementary logic that they do not make sufficient use of what is known concerning elementary logical relations. At present the theory of general propositions is in a position to replace the whole of the traditional treatment of propositions, of immediate inference, and of syllogism by a much more satisfactory analysis; yet writers of elementary texts continue to present this theory in its traditional form, much of which is inadequate and false.

Mr. Dotterer has a good introductory treatment of the algebra of classes and of the logic of elementary propositions (Chap. VII), but he is confused when (p. 119) he implies that from the fact that a proposition is false it follows that it is absurd or self-contradictory. Propositions which are self-contradictory are, of course, false, but obviously the converse does not hold.

The latter half of the book is given to a study of hypotheses, induction, and probability. The exercises and illustrations connected with this part of the book are particularly good.

Harvard University C. H. LANGFORD

Transcendental Values. By R. P. BYERS. Boston, Richard G. Badger, 1925, pp. 69.

If originality of thought and obfuscation of language either separately or conjointly merited the reviewer's attention this monograph would deserve detailed notice here. The author aims to show "the applicability of this great Law [of induction] to ranges of Philosophy that are utterly abstruce [sic]." To avoid the erroneous meanings of words he uses algebraic symbols which "have the further advantage of not having any connotation attached to them." But he has already warned us that they must not "be taken in any algebraic sense!" After repeated re-readings the reviewer is forced to admit that he has not yet attained to the high rank prescribed in the author's closing remarks: "I shall consider myself fortunate if able to describe it [the method] without at the same time creating misunderstanding. For its sphere of final operation is near the margin of consciousness and the successful use of it demands a delicacy of attention unusual to ordinary thinking."

University of Iowa CHRISTIAN A. RUCKMICK

Children's Drawings. Edited and compiled by STELLA AGNES MCCARTHY. Baltimore, Williams and Wilkins, 1924, pp. 164.

This study by a group of well known educators, composing the Child's Study Committee of the International Kindergarten Union, was undertaken for the purpose of constructing a scale for the drawing ability of children of the primary grades. Since the Thorndike and the Kline-Carey scales are best suited for older children, the new scale fills a need.

As a test it has the great merit of being standardized on the basis of 31,239 drawings from children in various parts of the country. The three most frequently represented objects in children's drawings were used in constructing the scale, and thus the test has the quality of engaging the child in subject of particular interest to him. Moreover, practice is in some measure controlled, since probably all children have had some experience in drawing the particular objects required by the test.

The investigators have taken full advantage of the implications of their data on many questions of particular interest to the art educator. Although conclusions arrived at are often confirmations of previous workers, or notions current about art, there is a certain newness in having these ideas about art supported by the quantitative evidence that is to be found in this book.

Harvard University T. KARWOSKI

Ueber Begriff und Formen der Intelligenz. By OTTO LIPMANN. Leipzig, J. A. Barth, 1924, pp. 48. Reprinted from *Zsch. f. angew. Psychol.*, 24, 1924, 177-224.

The author presents and discusses a number of definitions of intelligence and stresses the point that a given intelligent reaction to a situation does not necessarily indicate intelligence on the part of the individual reacting, since the response may be purely imitative or learned. According to Lipmann, intelligence depends (1) upon the ability to make *Gestalten*—in the sense of sizing up a complex situation; (2) upon the disposition to make

Gestalten at a given moment; and (3) upon the capacity to make valid and purposeful *Gestalten*. It is really a three factor theory depending (1) upon general ability to adapt to new situations; (2) upon special ability to meet definite situations; and (3) upon specific memory and language ability.

Lipmann criticises the method of averaging scores or mental ages obtained from several tests, and suggests that a profile showing the results is a much more desirable method. The Binet tests are characterized as primarily tests of school adjustment and not tests indicating ability to adjust to new and changing conditions of life.

Lipmann differentiates three types of *Gestalt* formation—*Nachgestalten, Neugestalten*, and *Umgestalten*. That is, he distinguishes between the mere reproduction of situations, the creation of new situations, and the arrangement of situations by either or both of the first two methods.

University of Chicago SIMON H. TULCHIN

La Defense Psychique. By I. BOAS. Paris, Félix Alcan, 1924, pp. 253, 18 pl.

In the first part of this book Dr. Boas inquires into the psychical concomitants of the volumic changes recorded with the plethysmograph. His conclusion, based partly on his own data but mostly on data drawn from the literature, is that vaso-constriction correlates not with pain, attention, or mental activity, but with a 'psychical defense,' the chief characteristic of which is to overpower and render innocuous an otherwise harmful perception.

The second part of the book is devoted to a discussion of 'psychical defense.' From the physiological point of view Dr. Boas considers this mechanism to be essentially an inhibition. From the biological point of view it ranks with attack and flight as one of the major defense mechanisms. Its utility resides in the fact that it enables the organism, by delaying and fractionating its reactions, to cope more efficiently with its environment.

In a concluding chapter the author discusses the relation between 'psychical defense' and suppression. Analytic therapy, he believes, in doing away with suppressions may in certain cases deprive the organism of a perfectly normal defense mechanism, and thus sacrifice an ulterior but permanent adjustment for an immediate but temporary one.

Harvard University J. G. BEEBE-CENTER

The Gang Age. By PAUL HANLY FURFEY. New York, The Macmillan Co., 1926, pp. xii, 189.

Furfey presents an interesting study of the pre-adolescent boy in which he displays an understanding of youth culled from intimate contact with its problems. During the period between the age of eight and the onset of puberty boys show a characteristic team spirit that draws them into gangs. Among the various ages in life which invite the research of the psychologist, there is none more difficult and yet more charming than the "gang age." In his sympathetic picture of the needs and satisfactions that throng the experience of the pre-adolescent boy, Furfee adds a valuable chapter to genetic, and social psychology.

Harvard University W. F. VAUGHAN

NOTES

THE ACCURACY OF THE METHOD OF CONSTANT STIMULI: A REPLY TO DR. URBAN*

By courtesy of the editors I have seen Dr. Urban's article in proof and have been conceded a few days for reply. The facts at issue may be briefly and simply set down.

That the reader may have a rôle in the discussion, instead of viewing it as a private debate, let him consider a standard stimulus (S_0) of 100 grm. with which a series of other stimuli (J), ranging below and above S_0, are to be compared, in an effort to find the differential capacities of the organism. That value of J (say 97 grm.) which is reported 'lighter' by an observer just 50 times in 100 is called by Urban S_1; the correlative point for 'heavier' (say 102 grm.) is S_2. If we measure from the standard ($S_0 = 0$), S_1 (=3 grm.) is the limen for 'lighter' and S_2 (=2 grm.) likewise for 'heavier.' The question is: *Does Urban's formula give the probable error of these limens?* The argument has three heads: A, B, and C.

TABLE I

	Observers						
	I	II	III	IV	V	VI	VII
1. S_1	93.34	95.08	97.91	95.44	94.22	95.29	95.80
2. h_1	.1258	.1053	.1389	.1234	.1274	.1102	.1145
3. p.e. (Urban)	3.933	3.142	6.607	4.484	6.635	3.791	4.436
4. p.e. (M) (empirical)	.262	.214	.458	.382	.454	.208	.285
5. p.e. (Md) (empirical)	.328	.268	.574	.479	.569	.261	.357
6. S_2	99.68	99.27	99.39	98.24	97.16	100.75	101.17
7. h_2	.1361	.1109	.1452	.1180	.1157	.1150	.1155
8. p.e. (Urban)	9.477	5.491	8.282	6.232	7.518	5.765	7.385
9. p.e. (M) (empirical)	.259	.198	.299	.367	.632	.371	.375
10. p.e. (Md) (empirical)	.325	.248	.375	.460	.792	.465	.470
11. Range of S_1	7.87	6.28	13.21	8.97	13.27	7.58	8.87
12. ±1 p.e. (Urban) S_2	18.95	10.98	16.56	12.46	15.04	11.53	14.77
13. Extreme range of S_1	3.96	3.30	5.60	3.53	5.55	2.32	3.11
14. fractional limens S_2	3.61	3.11	4.26	3.98	6.29	4.72	3.88

(A) To clear the air, let us enquire a moment into the nature and function of any probable error. A p.e. is designed to inform us how a given magnitude (the limen) may be expected to vary if we repeat the observations again and again and thus derive a whole set of values instead of only one. Everybody knows that the best way of finding the dispersion (p.e.) of a limen would be to make, say, 100 or more independent determinations under stated conditions and then note how these 100 values are disposed; that is, compute the p.e. of their central tendency. That is ideal; but life is short and work is hard, so we try to infer a theoretical value that will serve our scientific needs and yet will save time and labor. Well-known methods exist for predicting the probable scatter of an average from the dispersion of the individual measures which define it. A theoretical p.e. then fulfills its function only when it forecasts with usable precision the empirical variation of the average to which it applies; any value which fails under this primal test is not a probable error at all, no matter how it may have been derived nor by what name it is called.

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*NOTE. Since time and distance make it impossible to submit this note to Dr. Urban before its publication, I have taken the responsibility of including Dr. Culler's rejoinder along with Dr. Urban's criticisms. The reader will find it convenient to consider both comments at the same time and in the same place.—Madison Bentley

Let us apply this cardinal criterion to the figures which Dr. Urban himself gives and which it is the purpose of his article to defend. Look at Table I.

S_1 here is the limen for 'lighter' (S_0 being 100 grm.) and S_2 likewise for 'heavier' (because of a large time-error, many S_2's are below the standard); h_1 measures the 'precision' of a given *O*'s reports for 'lighter,' and h_2 for 'heavier' ($h = \frac{1}{\sigma\sqrt{2}}$, where σ is the standard deviation of the normal percentile curve as fitted to the observed percentages). In cross-rows 3 and 8 are mustered Urban's values for the probable errors of the several limens; it is these figures which are in dispute and which form the subject of our two papers. In rows 4 and 9 appear the *empirical* p.e. [by the usual formula: p.e. (M) $= \frac{.6745\sigma}{\sqrt{n}}$] for the mean of nine (*O*s I, II, III) or six (*O*s IV to VII) independent values; in rows 5 and 10, likewise, the actual p.e. for the median of the same figures [p.e. (Md) $= \frac{.8454}{.6745}$ p.e. (M) in a normal dispersion]. In rows 11 and 12 we see the range, for each value, of ± one p.e. (Urban); for *O* I, S_1 the figure 7.87 equals twice the p.e. 3.933. In 13 and 14, finally, we see the *extreme* range of the partial limens; for *O* I the lowest S_1 is 91.68 and the highest 95.64, the two being separated by 3.96 grm.[1]

TABLE II

The first figure for each *O* stands for S_1, the second for S_2

	Observers													
	I		II		III		IV		V		VI		VII	
1.	.50	.19	.53	.28	.42	.26	.39	.32	.42	.42	.31	.41	.35	.26
2.	$.26^9$	$.10^9$	$.28^9$	$.15^9$	$.22^9$	$.14^9$	$.21^6$	$.17^6$	$.22^6$	$.22^6$	$.17^6$	$.22^6$	$.19^6$	$.14^6$

What do these figures have to say? If we made 100 separate determinations of a given limen in lieu of only one, Urban would have us believe that about 50 would fall beyond the limits (in I, S_1) of ±3.933 or 7.87; in I, S_2 they would scatter still more and one-half would fall outside a range of ±9.477; and so on.[2] Is there any way of testing this prediction? Happily for our needs, Dr. Urban fractioned his original data, comprising 450 reports on each pair of stimuli,[3] into nine groups of 50 and computed h and S for each; these limens, based on 50 reports, we may call 'partial' or 'sublimen' and those from the whole number (450 or 300) 'total.' Knowing how much the partial limens actually diverge, we have a useful empirical check upon the accuracy of any proposed theoretical figure. To this end we may use either p.e. (M) or p.e. (Md); since the limen, in this method, is defined as the median of the cumulative percentile curve, we may properly use p.e. (Md); being higher than p.e. (M), these are also more generous to the Urban-hypothesis. Let us compare the p.e. (Urban) and p.e. (Md) in two ways.

(1) In rows 11 to 14 we see how far ± one p.e. (Urban) extends and also the limits without which *every* partial limen actually falls. Were

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[1] Rows 1,2, 6 and 7 are copied from Urban (*7*, 192), 3 and 8 from *ibid.*, p. 226; rows 4, 5, 9,10,13 and 14 were computed from the figures for S_1 and S_2 in Tables 1-7 (*8*, 276-8). Rows 11 and 12 are obtained by multiplying the corresponding p.e. (3,8) by two. We may note here that the mean values of S_1 and S_2 in the Tables differ, by a few hundredths, from S_1 and S_2 as recorded in rows 1 and 6; the latter figures are computed directly from the whole number of observations. These trifling disparities have nothing to do, of course, with the argument here. We may observe also that the first number of row 9 (I) is .259. In my earlier paper (*2*, 85) this was given as .276 from Thomson (*6*, 52); upon recomputation it proves to be .259.

[2] If the limens are normally disposed, one-half will lie more than one p.e. from their average.

[3] With *O*s I, II and III; six sets of 50 (300 in all) with *O*s IV to VII.

Urban's p.e. reasonably true, one-half of these 102 sub-limens ought to lie beyond the bounds of ± one p.e.[4] But not a single one does actually fall outside; they all cluster far within his theoretical boundaries. Table II brings home the facts.

In row 1 we discover that the partial limens (for I, S_1) are crowded within .50 p.e. (Urban) ($=7.87/3.96$) instead of spreading over three or four p.e., as they ought to do were the Urban p.e. reasonably reliable. Of a normal population 26% lies within .50 p.e. of its average; if we take limens at random from an urn, the chance of drawing one from this area is clearly .26. The likelihood that nine successive draws will fall therein is $.26^9$ (row 2); and that all the sub-limens in the 14 cases will cluster so narrowly within the Urban limits is the product of all fourteen numbers in row 2. This equals about 10^{-75}; the probability that the empirical limens would spread no more than they do, were the Urban-values correct, is 1 in 10^{75}. The reader may not be aware how incredible this quantity is. Let us consider it a moment. Of all objects in the visible universe, the nebula Andromeda is said to be of the most remote (*4*, 27), whence light travels one million years before it reaches the eye of a terrestrial astronomer; and yet it is 'only' 10^{24} centimeters away. The whole known universe could be housed within a cube containing 10^{75} cc. That's how 'probable' Urban's p.e. are.

(2) Approaching the matter again, we divide the Urban p.e. by the empirical p.e. (Md) of the partial limens, for the 14 cases, and have:

TABLE III

The first figure with each *O* stands for S_1, the second for S_2

	Observers													
	I		II		III		IV		V		VI		VII	
1.	12.0	29.2	11.7	22.1	11.5	22.1	9.4	13.5	11.7	9.5	14.5	12.4	12.4	15.7
2.	15	80	15	44	15	44	9	19.	15	9	23	15	15	28

In row 1 (*O* I) we find 12.0; that is, Urban's p.e. (3.933) is 12.0 times the empirical p.e. (Md), .328. What is the likelihood that even one limen may be found, upon repeated testing, which will depart from the average by as much as 3.933 (12 p.e.)?[5] From Pearson's table (*5*, 11 and xxi) a value which deviates 12 p.e. (some 8 s.d.) from its average will occur about once in 10^{15} trials; with I, S_2 the chances reduce to 1 in 10^{80}. Row 2 contains the successive exponents of 10. The probability of the whole system of deviations [that even a single limen for each *O* will depart one p.e. (Urban) from the average] is the product of these fourteen quantities, or 1 in 10^{346}. How big this number is I have no idea; but in view of the evidence Urban's figures might well be called the 'im-probable errors.'

(B) Urban's p.e. changes in magnitude as we alter the arbitrary zero or point of reference. This may be tested by the reader if he cares to do so. The complete formula is found in *7*, xvi, 226, with a correction given in *9*, 253, footnote; or better yet, in Thomson, *6*, 55. Let the reader calculate a p.e. using, as Urban did, the regular value of the stimulus-weights (84, 88, ... ,108 grm.) and again with origin referred to 80, so that the stimuli become 4, 8, ..., 28. Since the actual steps remain the same (4 grm.), the change will have no effect whatever upon the actual dispersion;[6] but it makes a big difference in the Urban p.e. By merely shifting the origin Thomson was able to prove that Urban's p.e. for *O* I, S_2 could be reduced from 9.477 to .39; whereupon he concludes; "Professor Urban's formula can be made to give us any value from this [0.39] upward by merely moving the origin, a procedure which ought to have no effect on the probable error, and has indeed no effect on the corrected formulae given in this paper" (*6*, 55). We see that Urban's formula is not even consistently incorrect; it yields no one determinate value at all for the probable error; what you happen to get depends upon the origin you happen to choose!

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[4] For *O*s I to III, nine S_1 and nine S_2; for *O*s IV to VII, six S_1 and six S_2.

[5] As before, one-half of all future limens ought to deviate from the average by this amount were the Urban-figures true; we merely ask whether a single *one* (out of nine or six) will do so. It is clear, furthermore, that the total limens will scatter less than do the partials; by the common theory of measures, sub-limens ought to spread $\sqrt{9}$ or $\sqrt{6}$ times as far as do the whole limens, S_1 or S_2. The p.e. (Urban), as computed for a sampling case, proved to be 6.90 for *O* I, limen S_1, sub-limen I 2 a (*7*, xv, 268), the figure for S_1 itself being 3.93. We are extremely generous therefore to Urban in applying his p.e. for S_1 and S_2 to the partial limens.

[6] The absolute magnitude of S_0, S_1 and S_2 will of course be reduced by 80 grm., but the distribution itself will be unaffected.

(C) Dr. Urban writes: "My presentation of the method of constant stimuli is in agreement with the method of least squares. . . . There exists just one way to refute my formulae: it must be shown that my deduction is in contradiction with one of the rules of least squares. None of my reviewers has done this. . . . At present I no longer hope that an error will be found in my determination of the probable error, which is rather a pity" (*9*, 256).

Professor Thomson long ago made known two errors in Urban's procedure; the more important of these we may briefly examine here. In Urban's method, two values, c (=ah) and h, are first derived; the quotient c/h then yields the limen, a. Knowing the probable errors of c and of h, Urban proceeds to deduce (*7*, 226) the p.e. of their quotient [the limen a (=c/h)] by means of the customary general formula (involving a Taylor-series) for propagation of error by independently measured quantities whatever their functional relation (for the method, see my earlier article, *2*, 86; or any book on least squares *e.g.* Merriman, *3*, 78). This procedure is perfectly sound when we are dealing with *wholly independent variables*; but c and h, far from being unrelated magnitudes, vary with a concomitance that is well-nigh complete. Consider Table IV, which arrays the values

TABLE IV

Correlations of h with c

Observers

	I	IIa	III	IV	V	VI	VII	IIb	VIII
S_1	.983	1.00	.983	1.00	.883	1.00	1.00	.996	.997
p.e.	.008		.008		.052			.001	.001
S_2	1.00	1.00	1.00	1.00	1.00	1.00	.983	.994	.998
p.e.							.010	.002	.001

of ρ (Spearman's rank-correlation) between c and h for Urban's 7 *O*s and two others (*8*, 297-8).[7] It is clear that the functional dependence of c and h is virtually perfect; this is all the more striking in the longer series, IIb and VIII. That h and c (=ah) should change as a rule *pari passu* would seem to be reasonable in theory and here proves to be true in fact. We are told that Urban, in private correspondence with Thomson, "urges that c and h are independent" (*6*, 53). In view of these figures, this claim can no longer be defended, at least not with the above data; and yet, despite their well-nigh perfect correlation, Urban treats c and h as magnitudes which vary in complete independence of each other! If he has been looking for a flaw in his formula, he might do well to renew his quest right here.

A few words may be devoted to sundry comments by Dr. Urban.

(a) "Culler does not do justice to my argument, when he asks how the $\Phi(\gamma)$-hypothesis can be less exact than the arctan-hypothesis. I did not calculate the probable error of the interval of uncertainty by this latter hypothesis," etc. (*9*, 253). The statement is correct. I find that the arctan-procedure is not included among the "vier psychophysischen Methoden" of which the $\Phi(\gamma)$-process is said to be "die ungenaueste" (*7*, xvi, 227). My original question then (*2*, 85) is hardly relevant and is hereby withdrawn.

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[7] For I, IIa, III, n = 9; for IV to VII, n = 6; for IIb and VIII, n = 28.

(b) "Culler then compares his results with those obtained by Thomson. This is a mistake if it is meant that these data are different from mine. The point of Thomson's criticism was settled five or six years ago" (*9*, 253). To the last, rather cryptic, remark let me adduce a statement published by Thomson in 1925 (*1*, 74):

"Professor F. M. Urban, some twelve years ago, brought forward reasons which in his opinion showed that the Method of Limits was much more exact than the Constant Method: but his mathematics contained certain errors.[8] The various processes do not differ very widely in this respect if each is used in circumstances favourable to itself, but on the whole the Constant Process is most reliable, and Linear Interpolation least." We see that, as recently as 1925 when the third edition of his book was issued, (1) Thomson still adhered to the criticisms of his 1914 article; (2) he still believed, in categorical opposition to Urban, that "on the whole the Constant Process is most reliable, and Linear Interpolation least." It is clear that, as late as two years ago, Thomson had in no wise modified his opinion about the inaccuracy of Urban's formula nor retracted his original objections; one may be curious to know, therefore, in what sense "the point of Thomson's criticism was settled five or six years ago."

(c) "There is no real difference between his results and mine, provided that one does not stop halfway, as Thomson did" (*9*, 253); "his final results are not much better than mere guesses" (255); "his idea is right, but he did not work it out" (255); "my formulae and those of Thomson are essentially identical" (256). Dr. Urban of course should know whether his formulae are "essentially identical" with Thomson's; but it is clear that Thomson, as late as 1925, did not so consider them, even thought Urban had "communicated his results" (256) to him (presumably five or six years back). Surely anyone may be excused for wondering how Thomson's formula, which yielded .151, where Urban's gave 9.477 (over 62 times as large!), can be essentially identical with his (*6*, 51).

(d) Dr. Urban's book on *Wahrscheinlichkeitsrechnung* did not reach me until a few days ago, since writing this paper. It was not available when I prepared the other article (*2*) and there is no time for me now to read it thoroughly. The book has no special place, however, in our discussion. Dr. Urban is defending and I am attacking the procedure and figures given in the original articles (*7*); there is no need for a 'change of venue.'

To conclude: we have tried to prove that Urban's formula for the probable error of the limen is fatally defective in no less than three ways.

(1) His own figures, when tested by the actual dispersion of the fractional limens, are fantastically improbable.

(2) The formula yields no single, defined value at all. By the simple expedient of shifting the origin, we can get, over an extensive range, any figure whatsoever.

(3) The quantities h and c, whose mutual dependence is virtually perfect, he treats like wholly unrelated variables, as if they were heads and tails in successive throws of a coin.

In closing, I must avow a deep bewilderment over Dr. Urban's paper. I yield to none in admiration of his work in this toilsome and difficult field, and regret his loss to our science. That he, of all people, should seriously defend a procedure whose results are clearly erroneous is almost incredible. Many of these facts have been adumbrated or developed before, notably in Thomson's brilliant critique; but I have searched Dr. Urban's paper in vain for any sign that he is aware of their crushing force. An air of unreality pervades the whole business, which recalls the ancient word of Horace: *bonus dormitat Homerus!*

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[8] Thomson here refers to the article of 1914 (*6*) and to another of 1913.

Of course, it may be I who nods! But the reader has both papers in hand; let him decide who is dozing.[9]

University of Illinois ELMER CULLER

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(8) ——— ———, Der Einfluss der Übung bei Gewichtsversuchen, *ibid.*, 29, 1913, 271-311.
(9) ——— ———, The accuracy of the method of constant stimuli, this JOURNAL, 38, 1927, 252-256.

THE THIRTY-FIFTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The thirty-fifth annual meeting of the American Psychological Association was held at the University of Pennsylvania, December 28-30, 1926. There was a total registration of three hundred and ninety-nine. Two hundred and forty attended the banquet, and a number of persons came in later to hear the presidential address. The final attendance of the evening was three hundred and fifty. There were fourteen ex-presidents of the Association present at the meeting, and members came from thirty-three states and three foreign countries. About one hundred and fifty persons attended each of the main sessions. One hundred and fifty persons were at the first, and sixty at the second experimental round table. The large attendance was entirely unexpected, and the rooms were in consequence frequently over-crowded, but the sessions were started promptly and without confusion, and the local committee deserves sincere praise for its efficient management.

We can expect a considerable increase in the attendance in the future, due to the growth in the membership, and the increasing importance of the annual meeting. Even at the present time, the task of the program committee is not easy. A number of papers had to be rejected, as it is against the policy of the Association to run too many parallel sessions. It is unfortunate for individual members that such elimination must be made, but in the long run the Association will profit by a program of carefully selected papers from a large preliminary list. The program committee worked with great care and foresight, and a well-balanced program was arranged. As usual, there were parallel sessions, which were staggered in order to allow the members to hear a good proportion of the papers. There were never more than two parallel sessions, except on Wednesday afternoon, when three conferences were in session at the same time. I do not include, of course, the corridor meetings which had their usual full attendance. There were six papers in most sessions; a few sessions had seven papers. The latter number makes rather a long program, and leaves little time for discussion, which, even under the best conditions, is seldom very spirited.

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[9] The h-method, developed in my former article, has not been treated above. It was originally proposed as a *Notbehelf*, which gave usable values, with full recognition of its approximate character. I have not been satisfied with its present form and shall publish shortly a revised procedure which will make it more accurate and useful.

As the abstracts will appear shortly in the *Psychological Bulletin*, I shall give only a general account of the papers. Instead of beginning the meeting with the customary session on general psychology, the opening sessions were upon applied psychology and experimental psychology. The session on applied psychology was devoted chiefly to personality studies and vocational problems.

There were ten papers on experimental psychology, distributed over a session and a half. The reports covered a wide range of subjects—the temperature sense, conditioned reflex, memory, sleep, emotions, and learning. The papers on comparative psychology were upon brain localization, auditory sensitivity of the rat, and the factor of age and nutrition in the behavior of the rat.

There were only six papers upon general psychology, in contrast to twelve last year. At the previous meeting, the theory of *Gestalt* was prominent. This year there were no general papers on that subject. A plea was made for instruction in anthropology. There were also papers on instincts, anthropoid research, religion, and equipment for teaching psychology.

The papers on mental measurement dealt with the intelligence of Indians, mental growth, mental organization, and specific problems of intelligence tests. The program for clinical psychology ranged from intelligence tests in the case of brain injury to color preferences of white and negro children.

In the session on abnormal psychology, hypnotism was the subject of several papers. Further work was reported on the perception of words by means of tactual stimuli, and upon certain phases of nerve regeneration. Other topics were compulsions, aphasia, and automatic drawings.

Methods of teaching and the learning process were the chief problems of the sessions on educational psychology. The use of the term attitude in education was criticized. A report was also presented of four tests to determine sex differences.

The session for graduate students was very successful last year, but the one session of fifteen papers was too long. Twenty-one papers were presented this year, but they were divided between two sessions, which was very much of an improvement. These papers have become an important feature of the annual meeting.

Conferences and round tables have grown in popularity. There were three this year, upon psychological research, clinical psychology, and experimental psychology, respectively. The experimental round table extended over two sessions. The first session was so largely attended that it became a general meeting, instead of an occasion for informal discussion.

At the business meeting, it was voted to extend an invitation to the International Congress to meet in America in 1929.

After the banquet, the president, Professor Harvey A. Carr, of the University of Chicago, read an interesting paper entitled 'The Interpretation of the Animal Mind,' which consisted in large part of a critical exposition of Miss Washburn's fundamental concepts of animal behavior.

Princeton University H. S. LANGFELD

'WOLF CHILDREN' OF INDIA

In the autumn of 1926 the writer of this note came across a brief report in one of our daily newspapers to the effect that two children had been discovered in Bengal, India, living in a den with a family of wolves. This report stated that the children had been removed from the wolf-den by the Rev. J. A. L. Singh, head of the orphanage at Midnapur, Bengal, India, and that the children had been taken into this orphanage. Interested in the account, the writer communicated with the Rev. Singh and received from him a letter dated December 6, 1926, containing a short sketch from the diary that he has kept on the subject of these strange children, together with a short history of his orphanage and various personal references that might be used in case further examination should be desired as to his personal and official status.

The Rev. Singh commenced his work among such natives as Santals, Lodhas, and Kurmis from the time he was head master of the local mission school at Midnapur, this school being affiliated with Calcutta University. Feeling a call for the ministry, Mr. Singh left the education department and in 1912 was ordained to the ministry. From that time onward he came more and more into contact with the natives. In every village that Singh visited he found orphans roaming about in want of food and shelter. Gradually he gathered together a number of orphans and in this manner there evolved Singh's orphanage at Midnapur. This orphanage was started on Singh's own initiative and responsibility and depended upon his personal income. Among those who have taken active interest in the Midnapur orphanage is Mr. W. N. Delivingne, District Judge of Midnapur.

The Rev. Singh informs the writer that he hopes to publish a detailed account of the 'wolf-children.' This projected publication is to include photographs of the children, taken at various intervals after the removal of the children from the den of the wolves to the Midnapur orphanage.

The following paragraphs are quotations from the Rev. Singh's letter referred to above:

"The wolf-children were first seen by natives on various occasions. I heard of these children for the first time on August 26, 1920. The same 'ghost story' was repeated to me on September 24, 1920. The children were seen through a field glass by several people (Europeans and Anglo-Indians), from a distance of about one hundred yards, on the 9th and 10th of October, 1920.

"Three wolves were observed to come out of a tunnel-like passage from their den, closely followed by two cubs; then there appeared a human head covered with bushy hair, with a ghastly look about the face. This head tarried for a little while looking to this side and that side, then a human form came out of the den followed by another human being at its heels. The two children crawled on all fours.

"Excavation of the wolf-den took place on October 17, 1920. I took charge of the wolf-children on November 1, 1920. I brought them home on November 4, 1920. I guessed their ages to be, the elder about eight years and the younger about two years. The younger one died September 21, 1921. She was baptized some time before she expired. We named her Amala. Kamala, the surviving one, was baptized January 1, 1925.

"At the present time Kamala can utter about forty words. She is able to form a few sentences, each sentence containing two, or at the most, three words. She never talks unless spoken to; and when spoken to she may or may not reply. She is obedient to Mrs. Singh and myself only. Kamala is possessed of very acute hearing and evidences an exceedingly acute animal-like sense of smell. She can smell meat at a great distance. She was never known to kill any domestic animal but is fond of pouncing upon any killed animal if found anywhere by her. Never weeps or smiles, but has a 'smiling appearance.' Shed a single tear when Amala died and would not leave the place where she lay dead. She is learning very slowly to imitate. Does not now play at all and does not mingle with other children. Once, both Amala and Kamala somewhat liked the company of an infant by the name of Benjamin while he was crawling and learning to talk. But one day they gave him such a biting and scratching that the infant was frightened and would never approach the wolf-children again. Amala and Kamala liked the company of Mrs. Singh; and Kamala, the surviving one of the pair, is much attached to her. The eyes of the children possessed a peculiar glare, such as that observed in the eyes of dogs or cats in the dark. Up to the present time Kamala sees better at night than during the daytime and seldom sleeps after midnight. The children used to cry or howl in a peculiar voice, neither animal nor human. Kamala still makes these noises at times. She is averse to all cleanliness, and serves the calls of nature anywhere, wherever she may happen to be at the time. Used to tear her clothes off. Hence, a loin-cloth was stitched to her in such a fashion that she could not open or tear it. Kamala used to eat and drink like a dog, lowering her mouth down to the plate, and never used her hands for the purpose of eating or drinking. She would gnaw a big bone on the ground and would rub it at times in order to separate the meat from the bone. At the present time she uses her hands for eating and walks straight on both legs, but cannot run at all."

University of Vermont PAUL C. SQUIRES

PRELIMINARY REPORT ON A DEMONSTRATION OF EXPERIMENTS ON HYPNOTISM SHOWN BY GUSTAF WALLENIUS, FIL. MAG. (UPSALA)

During the eighth International Congress of Psychology at Groningen' Holland, (Sept. 6-11, 1926), Mr. Wallenius gave a demonstration of experiments, which were a repetition of those performed by the late Professor Alrutz. These experiments were claimed to show that hypnotism is the effect of a 'nervous effluence,' and that it is not, as ordinarily supposed, a psychological function.

At the first sitting (Sept. 9) were present—besides Mr. Wallenius and the subject of the experiment (a Swedish metal-worker)—Professor Janet, Professor McDougall, Professor Roels, Dr. van Loon, Professor Spearman, Dr. Thouless, and (for the last part of the demonstration) Mr. Bartlett. The experiment, which was claimed by Professor Alrutz to be a crucial proof of his theory, was performed under strictly controlled conditions. The next day the same experiment was performed twice more in the presence of Mr. Bartlett, Professor K. H. Bouman, Dr. van Loon, Dr. Myers, Dr. Thouless, and Professor Zwaardemaker.

The methods and the results obtained will be fully reported in a longer communication which we hope to publish in the *Proceedings of the Society for Psychical Research* (London). We can, however, say definitely that no phenomena occurred in this demonstration which could require the theory of nervous effluence.

All those before whom the experiments were demonstrated agreed that the results obtained pointed to the ordinarily accepted theory of suggestion in hypnosis as an adequate explanation of all the phenomena observed.

University of Glasgow F. G. H. VAN LOON
Scotland R. H. THOULESS

THE SEVENTH ANNUAL EDUCATIONAL CONFERENCE

The seventh annual educational conference sponsored by the Ohio State University will be held at that institution on April 7, 8, and 9, 1927. As in former years the general sessions will be held on Thursday evening, Friday evening, and Saturday morning. Men of national note, such as Professor E. L. Thorndike of Teachers College, Columbia University; Professor Wallace W. Atwood of Clark University; Judge Olson of the Juvenile Court of Chicago; Payson Smith, Commissioner of Education for Massachusetts; and others, will address the general sessions.

Both forenoon and afternoon of Friday will be given over to sectional meetings of which this year there will be more than thirty. Here again specialists in their fields will contribute as, for example, Professor S. A. Courtis of Michigan in the Educational and Intelligence Test Section;

Professor S. A. Leonard of Wisconsin in English; Professor Hill of Illinois in Non-biological Science.

The keynote of this year's conference is 'Expertness in Teaching' and the whole program, both general and sectional, will center around this idea. The uniform testimony of the thousands who have attended past conferences is that each year the conference is outstanding for its helpfulness, its abundance of ideas and its lack of mere talk. There is every reason to believe that this year's program equals, if it does not exceed, all former ones.

Ohio State University E. J. ASHBAUGH

A NEW DEPARTMENT: APPARATUS

A new department, devoted to descriptions of psychological apparatus has, with this issue, been added to the JOURNAL. A change in policy is not involved, as the pages of the JOURNAL have always been open to articles of this kind. Up to the present, however, articles on apparatus have been scattered among different departments of the JOURNAL; sometimes they have appeared among the major and minor articles, at other times among the Notes. Under the present plan these articles will be grouped together. This arrangement, we hope, will be of convenience to our readers, and will receive the support of our contributors.

PSYCHOLOGICAL ABSTRACTS

Since the appearance of the January number, the JOURNAL has received the first three issues of the *Psychological Abstracts*, a monthly magazine published by the American Psychological Association. The new journal publishes abstracts of papers and books, in all languages, on psychology and cognate subjects. The abstracts are non-critical; the contributors do not attempt to evaluate or appraise the articles they review, but merely act in the capacity of reporters.

The journal is edited by W. S. Hunter, of Clark University, with the assistance of R. R. Willoughby, of Clark University, and the coöperation of an international board consisting of F. C. Bartlett, of Cambridge University, England; V. M. Bekhterev, of Leningrad, Russia; Ed. Claparède, of the University of Geneva, Switzerland; G. C. Ferrari, of the University of Bologna, Italy; A. Michotte, of the University of Louvain, Belgium; H. Piéron, of the Sorbonne, Paris, France; M. L. Reymert, of Wittenberg College, Ohio; and W. Wirth, of the University of Leipzig, Germany.

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TYPES OF DEXTRALITY AND THEIR IMPLICATIONS[1]

By JUNE E. DOWNEY, University of Wyoming

The conventional classification of individuals into right-handed and left-handed furnishes very little information about their manual habits. Yet detailed information concerning the way in which the hands are used has considerable practical importance and may possibly throw light upon certain theoretical problems related to speech, hand-writing, nervous stability, and temperament.

It has been my observation that all degrees of unidextrality (the use of one hand in preference to the other) exist and I have sought for some simple method of determining the degree of such unidextrality without recourse to time consuming individual tests. The most fertile suggestion concerning dextrality types that I have seen is that of Rife.[2] I have classified some 1500 individuals according to Rife's scheme which has been slightly modified by myself. In addition to the handedness formula I have obtained other data, such as the sighting eye, the spade foot, the thumb uppermost when the two hands are clasped with fingers interclasping and the like. I have sought to determine whether dextrality types are tied up with degrees of unidextrality or, even, with psychic patterns.

(1) *Handedness, inherited or acquired?* The statistical survey outlined above has value quite apart from the problem of the origin of handedness in native or acquired tendencies. If handedness is socially conditioned it may be well to ask what society achieves by its dictates and whether it might use its power in conditioning manual reactions more effectively. Moreover, a survey of manual habits as they exist in society as organized at present may sharpen our observation in genetic studies, although it cannot, of course, take the place of such genetic study.

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[1] This investigation was made possible by a grant from the National Research Council through its Committee on Scientific Problems of Human Migration.

[2] J. M. Rife, Types of dextrality, *Psychol. Rev.*, 29, 1922, 474-480.

Watson has recently stated very clearly his conviction that right-handedness is a habit socially instilled. "Our whole group of results on handedness leads us to believe," he writes, "that there is no fixed differentiation of response in either hand until social usage begins to establish handedness. Society soon thereafter steps in and says 'Thou shalt use thy right hand.' Pressure promptly begins. 'Shake hands with your right hand, Willy.' We hold the infant so that it will wave 'bye bye' with the right hand. We force it to eat with the right hand. This in itself is a potent enough conditioning factor to account for handedness."[3] "The main problem is, I believe, settled: handedness is not an 'instinct.' It is possibly not even structurally determined. It is socially conditioned. But why we have 5% of out and out left-handers and from 10-15% who are mixtures—*e.g.* using the right hand to throw a ball, write or eat, but the left hand to guide an axe or hoe, etc.—is not known."[4]

The present paper makes little contribution to the solution of the nature *versus* nurture problem, although I believe that little is in favor of a native factor operating in manual preferences. Watson asks for structural differences as basal to functional differences and he is not apparently willing to concede that a little difference in structure may make a great difference in function. Later I shall cite some evidence of structural differences between the extremely right and left-handed but at present I am willing to leave open the question of nature versus nurture. But, to repeat, the present statistical survey furnishes a number of suggestions concerning lines of approach in a genetic study. It suggests also that it is necessary in studying infants or even apes to consider each case on its own merits.[5] If there is only a small percentage of extremely right or left-handed and a large percentage of mild cases the former may be obscured by a massed treatment.

(2) *Dextrality Types.* Rife states "there are at least three distinct types of right-handedness, with three corresponding types of left-handedness, making in all 6 forms of dextrality." These types of dextrality are determined by observation of the use of the hands in unimanual operations such as throwing, writing, eating and the like, and bimanual activities such as batting, sweeping, spading, and using the axe. In bimanual operations handedness is determined, says Rife, by the hand "nearer the business end of the instrument."

For many individuals the bimanual preference is the opposite of their unimanual preference. They throw, for example, right-handed, but they bat left-handed and they use a broom or shovel left-handed, that is, with the left hand placed below the right. Other individuals have a divided bimanual preference. They bat right-handed but they use a broom or shovel left-handed.

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[3] J. B. Watson, What the nursery has to say about instincts, *Ped. Sem.*, 32, 1925, 321

[4] *Op. cit.*, 322.

[5] As was done by Wolfgang Köhler in his book *The Mentality of Apes*, English trans., 1925.

I have followed Rife's classification with a few additions. The following formulae are used for the various types. If the right hand is the preferred hand in both unimanual and bimanual activities, an individual is labelled RRR. Such a right-handed person uses the right hand for eating, writing, throwing, etc., bats right-handed and places the right hand below the left in sweeping and spading.

If an individual is right-handed in unimanual activities but has an opposite preference in bimanual activities he is labelled RLL. He writes, throws, eats, etc. right-handed but he bats and sweeps left-handed.

If an individual is right-handed for unimanual activities but has a divided preference in bimanual activities, that is, bats right-handed but sweeps left-handed, he is labelled RRL.

The corresponding formulae for the left-handed are LLL, LRR, and LLR. The LLL individual is left-handed for both unimanual and bimanual activities, that is he both throws left-handed and bats and sweeps left-handed. The LRR man is left-handed for unimanual operations but right-handed for bimanual activities; he throws and writes left-handed but bats and sweeps right-handed. The LLR type is left-handed in one-handed operations and has a divided preference in the bimanual activities, for example, is left-handed in batting but right-handed in sweeping.

So far I have followed Rife. There are, however, refinements in observation that extend the list of formulae somewhat. Let us consider, for example, not only the relative position of the hands in sweeping but also the movements by which the broom is manipulated. There are individuals who place the left hand below the right on the broomstick and who are called left-handed sweepers but who employ the left hand merely to steady the broom; the right hand exerts the pressure and does the guiding. I have labeled this group RRL_1. I believe this group sweeps mainly toward the left but there are many exceptions to this statement.

There are other individuals who not only place the left hand below the right on the broomstick but exert the main pressure on the left; the right hand steadies the implement. I have labeled this group RRL_3. In shifting the direction in which they sweep they may reverse the movement used by the left hand; there is a push toward the right or a pull toward the left.

Sweepers of both the RRL_1 and RRL_3 types, instead of shifting from push to pull in manipulating a broom, may revolve the body as they work. One sees frequent illustrations of this, a rhythmic revolution of the whole body of which the sweeper is scarcely conscious.

There is still a third type of sweeper, the sweeper who reverses the relative position of the two hands, now placing the left hand below the right and now the right below the left. I have labeled this individual RRR-L. He may be an individual who has little preference as to which hand takes the initiative in sweeping, and to this extent is ambidextral. Boys who have done a considerable amount of janitor's work tell me they alternate the hands in this way in order to avoid fatigue.

The RRR individual who sweeps right-handed, that is, with the right hand below the left on the broomstick, should, theoretically, afford as great variety of subtypes as the left-handed sweepers. I have found, however, only a few cases in which the right-handed sweeper thought that the left hand was doing the work, but there are a few, labeled RRR_4.

Among left-handed individuals of the type LLR, it is also possible to distinguish those who report that the left hand does the work in spite of the dominant position of the right hand, and those who report that the right hand exerts the strength.. I have, however, made no attempt to sort out these individuals since my left-handed groups are so small.

It may be said that observation of the way in which a broom is wielded is particularly enlightening since there appears to be no particular social pressure exerted (apart from unconscious imitation) to make one adopt a particular method of manipulation. Many individuals are completely unaware of how they use a broom and are obliged to handle one in order to report their method. But their procedure may be none the less highly stabilized. To get some idea of the time of such stabilization it is necessary to make observations on young children. Records which I have from kindergarten children indicate that the procedure is frequently stabilized by five years of age, in which case it is difficult to think of such stabilization as an outcome of practice or habit, since five-year olds have little serious experience with sweeping.

Batting habits are probably much more influenced by social pressure than are broom habits. Boys, particularly, are subjected to this pressure. In determining the formula for batting some difficulty arises in the relatively infrequent cases where the wrists are crossed. In classification of individuals I have consistently used the relative position of the two hands as the deciding factor. Thus, an individual whose left hand is above the right as the bat is raised is classified as left-handed in batting even though he swings the bat over the right shoulder and as a right-handed batter if the right hand is above the left and the bat swung over the left shoulder.

There are reports of mild bimanual preference; there are also reports of mild unimanual preference. In such cases social pressure usually determines that such individuals shall use the right hand in the conventional way but under certain circumstances, such, for example, as an accident to the right hand in childhood or imitation of a left-handed relative in the home, they may use the left hand or show an inconstant preference, use, for example, the left hand for eating and the right hand for throwing.

These cases of mild preference in the use of the hands raise the question of the meaning of ambidextrality. Among my 1500 cases I have perhaps 5 or 6 which might be labeled ambidextral in the sense that there exists scarcely any indication of a preferential use of the hands. Either eye, moreover, may be the fixating eye and when the hands are clasped with the fingers alternating, now the right and now the left thumb is uppermost—a very rare phenomenon. To repeat, cases of ambidextrality in this sense are exceedingly rare. There is, however, another use of the term. Individuals who belong to the type RLL or LLR frequently describe themselves as 'ambidextrous.' It will be recalled that for these individuals the bimanual preference is opposite to the unimanual. They throw right-handed and bat left-handed or the reverse. Among men this group is a fairly large one—about 14 out of 100 cases fall into it. Among 100 women not more than 7 or 8 cases occur. This group is a most interesting one as will be seen from later discussions.

It will also appear from later discussions that the two ambidextral types described above seem to be quite unlike so far as nervous or emotional stability is concerned. The first type, in which no fixation of function is evident, is a curiously unstable type, a statement which does not apply in the least to the second group.

A possible formula, which Rife asserts is never found except when natural inclinations have been tampered with, is RLR, or in the case of the left-handed, LRL. An RLR individual is one who is right-handed in unimanual operations, bats left-handed, and is right-handed in sweeping or spading, that is, he is left-handed in a difficult bimanual performance but right-handed in a minor one. The reverse is true for an LRL person. The latter is left-handed in unimanual operations, a right-handed batter, and a left-handed sweeper.

My results confirm Rife's. Only 15 individuals out of a total of 1251 gave formula RLR. This observation is an important one since it points to a native as against an acquired preference in the use of the hands, for it would seem that in the acquisition of social habits, accidentally conditioned—at least in certain bimanual forms—all or any combinations might occur.

(3) *Statistical Survey of Dextrality Types.* Some 1280 right-handed persons and some 250 left-handed have contributed to the data presented in this and following sections of the paper. Before presenting the tabulated material, it will be necessary to describe the way in which the material was collected and to comment on certain sources of error. Questionaries so worded as to elicit information about matters of fact, not of opinion, were sent to the members of the American Psychological Association, whose coöperation I most gratefully acknowledge.[6]

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[6] The questionary in somewhat abbreviated form ran as follows:

QUESTIONARY ON DEXTRALITY

(A) Name.......................... Date.................. Man or Woman..........

(B) Are you right or left-handed? Or ambidextrous?..........................

(C) Check which hand you throw a ball with. Right ( )—Left ( ). Which hand do you write with? R ( )—L ( ). Which hand do you hammer with? R ( )—L ( ). Do you swing a baseball bat over your right or left shoulder? R ( )—L ( ).

(D) Close your eyes. Clasp your hands with fingers interlacing. Which thumb is outside?.................. Repeat. Which thumb is outside?..................

(E) Fold your arms. Which arm is uppermost?.................. Repeat. Which arm?..........................

(F) Encircle one of the following numbers to show the way in which your ordinarily employ your hands in using a broom.
  (1) Right hand on top, left hand at center of broom stick. Work done primarily by right hand.
  (2) Left hand at top, right hand at center of broom stick. Work done primarily by right hand.
  (3) Right at top, left hand at center. Work done primarily by left.

(Continued on page 322)

From this source I obtained 251 adequately filled out returns; 216 from men, 35 from women. Many of the women psychologists had never used a baseball bat or a golf club so that it was impossible to determine from their records their handedness formula. The questionary returns from the psychologists bear every evidence of having been carefully and expertly filled out and have been accepted at their face value except in some instances in the matter of the sighting eye or spade foot to which reference will be made later.

A few additional questionaries were also obtained from scientists, other than psychologists, and from professional men. In particular, the questionary was used in the collection of information from left-handed individuals. In a number of these cases it was necessary to follow up the questionary by a letter asking for more detailed information or for a rechecking of the report. These questionaries are probably less accurately filled out than those returned by psychologists.

Questionaries have also been filled out by some 500 college and high school students, but in these instances, the questionaries have been personally checked by an examiner.

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(4) Left hand at top, right hand at center. Work done by left hand.

Encircle one of the following numbers to show the way in which you ordinarily employ your hands in using a baseball bat.

(5) Left hand at end of bat, nearest the body. Right hand toward the center of bat. Bat extended to the right.
(6) Right hand at end of bat, nearest the body. Left hand toward the center of the bat. Wrists crossed. Bat extended to the right.
(7) Right hand at end of bat, nearest the body. Left hand toward the center of the bat. Bat extended to the left.
(8) Left hand at end of bat, nearest the body. Right hand toward the center of the bat. Wrists crossed. Bat extended to the left.

In the same manner draw a circle about the number that indicates how you use your hands in manipulating the following: Refer to either the illustration of the broom or the bat.

Axe	1	2	3	4	5	6	7	8	Golf-club	1	2	3	4	5	6	7	8
Rake	1	2	3	4	5	6	7	8	Spade	1	2	3	4	5	6	7	8
Shovel	1	2	3	4	5	6	7	8	Pitchfork	1	2	3	4	5	6	7	8

(G) Which foot do you use on a spade? Right ( )—Left ( ). Which stocking do you put on first? R ( )—L ( ).

(H) Were you slow in learning to talk?.................... Have you ever had a speech disturbance?.................... What kind?....................

(I) Is your right or left eye the sighting eye, if either?....................
Can you use your non-preferred eye in a microscope?....................

(J) Try the following experiment. Use a small finger-ring; hold it at arm's length, and encircle a small object a few yards away. Focus on the object. Now close your right eye. Check what happens.
(A) No change
(B) Ring moves to right of object
(C) Ring moves to left of object
Refocus. Close your left eye. What happens?
(A) No change
(B) Ring moves to right of object
(C) Ring moves to left of object
Repeat this test until you are confident of the accuracy of your report.

(K) Are there any left-handed persons in your immediate family?....................
Yes ( )—No ( ). Name the relationship....................

(L) Have you any left-handed relatives, near or remote, on your father's side? Yes ( ) —No ( ). If so, name the relationship....................

(M) Have you any left-handed relatives, near or remote, on your mother's side? Yes ( ) —No ( ). If so, name the relationship....................

(N) Write your name and address and age with your right hand, below.
(O) With your left hand, below.

When the questionary was not usable, cards with blanks for record of the desired information were filled out by examiners who had actually tested the subjects in their manual habits and tried out the sighting eye. This procedure was, of course, necessary in handling delinquent and feeble-minded groups.[7]

Although the work has been done carefully there are several sources of error, apart from individual inaccuracy in report. It is obviously not always an easy thing to determine which hand does the most work in manipulating the broom. Formulae RRL_1 and RRL_3 certainly overlap to some extent.

Another difficulty was encountered in determining the batting habits of women and girls. Their information concerning this matter was very limited as compared with that of the men, except in the case of the young college women who play baseball.

The questionary as first formulated and the instructions sent out to collaborators made no reference to the possibility of the relative position of the hands being shifted in the use of the broom and various tools. My attention was called to this possibility by returns received from the members of the American Psychological Association. It is probably of some importance in determining subdivisions of the various classes.

Tables I and II give the distribution of handedness formulae, RRL_1 and RRL_3 being grouped together. Table I for 1287 cases in which all types of dextrality were included, and Table II for 251 left-handed individuals.[8] In Table I the percent of left-handedness was determined solely by the number which were found in groups in which the selection of left-handers played no part. For Table II a definite effort was made to secure as many records as possible from left-handed individuals. If, on the average, 5% of the population are left-handed, Table II represents a survey of about 5000 individuals.

In Table I there is a fourfold classification. The percentages for men and women are entered separately and, under each, percentages for superior and inferior groups. The latter classification is only a rough one. The superior group includes professional and college men and women, high school and public school boys and girls. The inferior group includes adult and juvenile delinquents, inmates of institutions for the feeble-minded and insane, and boys and girls in special classes for subnormal children. Some of the public school children included in the superior group are possibly below average in intelligence or character.

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[7] I am indebted to the following individuals for their help in obtaining data on which Tables I-XXVI are in part based: Mr. L. Yepsen (127 cases, Vineland, N. J.); Miss Agnes Hill (100 institutional cases, Mt. Morrison, Colorado); Miss Lillian Portenier (52 high school students, University of Wyoming); Dr. Richard S. Uhrbrock (161 penitentiary inmates; 89 Whittier School Boys; 125 Indians; 49 inmates of the Wyoming State Training School; and 34 inmates of the Wyoming State Hospital); Miss Winnie Wheeler (139 Indians and 39 Chinese). A number of records on left-handed individuals have been sent me by Miss Beatrice McLeod and by Miss Hazel Cushing of the State Department of Education, Wyoming. Those who collected material have been enabled to do so by the kindness of the superintendents in charge of the institutions concerned. We take this opportunity to express our thanks to Dr. J. Harold Williams, Dr. E. Sullivan, Mr. Knox, and Miss Mahon for their help with the California subjects; and to Dr. C. T. Jones former Superintendent of the Wyoming State Training School; Dr. C. H. Solier, Superintendent Wyoming State Hospital; Mr. R. P. Haase, Superintendent Shoshone Reservation, Wyoming; and Mrs. Cornelia B. Mills, former Secretary of the Wyoming State Board of Charities and Reform. I am also indebted to Miss Reba Davis, Librarian of the University of Wyoming, for her help in obtaining library material.

[8] Records from Indians and Chinese are not included.

TABLE I
Handedness Formulae, 1287 Cases

Formula	Men				Women			
	Superior Group		Inferior Group		Superior Group		InferiorGroup	
	No.	% of Total	No.	% of Total	No.	% of Total	No.	% of Total
RRR	173	41.09	150	35.62	110	36.66	47	32.41
RRL	146	34.67	194	46.08	154	51.33	79	54.48
RLL	73	17.33	42	9.97	26	8.66	8	5.51
RLR	3	.71	11	2.61	0	0	1	.68
Left-Handed	26	6.17	24	5.70	10	3.33	10	6.89
Total all Formulae	421	99.97	421	99.98	300	99.98	145	99.97

TABLE II
Handedness Formulae—257 Left-handed

Formula	Men		Women	
	No.	% of Total	No.	% of Total
LLL	38	25.00	34	32.38
LLR	41	26.97	41	39.05
LRR	71	46.71	27	25.71
LRL	2	1.31	3	2.85
Total	152	99.99	105	99.99

Table I permits a comparison of percentages for each formula within each group and between any two groups. Possibly the most impressive feature is the large number, more than half, who use the left hand wholly or partially for bimanual operations. This confirms Rife's remark, in the article already quoted in which he writes: "From extended observation I am sure that the majority of the human race use the broom and shovel left-handed, and bat right-handed."[9] This left-handed use of the broom is more pronounced for women and for the group of inferior men than for the group of superior men. The superior men give, however, a larger percentage for the RLL formula. This is the type that is right-handed unimanually and left-handed bimanually. Rife speaks of this as "the ideal type, from the standpoint of symmetrical bodily development."[10]

Table II summarizes the formulae of 257 left-handed individuals, without division into superior and inferior groups.

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[9] *Op. cit.*, 475.
[10] *Op. cit.*, 479.

The collection of cases for this table required considerable effort, a statement which applies with particular force to the data on left-handed girls and women, who were much more difficult to locate than the men and boys and are, possibly, on the whole, more frequently of a superior level.

A comparison of Table II with Table I is most instructive for the same three handedness types appear in reversed form, although in strikingly different proportions, especially for the LRR formula as compared with the RLL. The very great increase in the proportion of LRR individuals in Table II over the RLL individuals of Table I is certainly curious. If appeal is made to social pressure to explain the constitution of such opposed preferences in unimanual and bimanual activities, it must not be overlooked that such pressure would need to operate in opposite directions for the two contrasting groups (RLL and LRR). That the two formulae (RLL and LRR) probably point to two actually distinct types is suggested by facts to be reported in the next sections, for example that significant differences are found between them in the matter of the dominant thumb.

No calculations of significant percentage differences between Tables I and II was made since the left-handed group is a highly selected one. But such calculation was made for differences which appeared promising *within* Tables I and II. The calculations of these percentage differences were made for me by Mr. Harold Benjamin of Stanford University after consultation with Dr. Kelley.[11] They are tabulated in Tables III and IV. Table III shows the results for those differences which appeared promising in the tabulation for right-handed subjects.

It appears that the chances are more than 999 in 1000 that the percentage differences are significant for the relative frequency with which the formulae RRL and RLL occur in the case (1) of superior and inferior men, and (2) of superior men and superior women. That is to say a significantly greater proportion of inferior than of superior men are RRL in type and a significantly greater proportion of superior than of inferior men RLL in type; furthermore a significantly greater proportion of superior women are RRL in type as compared with superior men and a significantly greater proportion of superior men than of superior women are RLL in type.

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[11] Mr. Benjamin's procedure was as follows: squared standard errors were computed for each of the categories under consideration (RRR, RRL, and RLL) using the formula $\sigma^2 p \frac{pq}{n}$; and then the standard error of the difference between any two categories was computed by the formula $\sigma p - p_1 = \sqrt{\sigma^2 p + \sigma^2 p_1}$. For example, σ^2 for RRR superior men $= \frac{(.4109)\ (1 - .4109)}{421} = .0005749$ and σ^2 for RRR inferior men $= \frac{(.3562)(1 - .3562)}{421} = .0005447$. $\sigma d = ' \sqrt{.000579 + .0005447} = .0335$. The Kelley-Wood table of the normal probability integral was used in estimating the chances that the differences were significant.

I shall attempt no explanation of this outcome. It may be the result of socially instilled habits; it may point to fundamental differences in nervous organization or functioning. I myself am inclined to believe the latter. Particularly with respect to sex differences in handedness I have been impressed by the consistency of the results throughout the whole investigation.

TABLE III
Chances that Percentage Differences of Table I are Significant

Formula	σd	Difference	Diff. / σd	Chances in 1000 that difference is significant
Between superior men and inferior men				
RRR	.0335	.0547	1.63283	949
RRL	.0336	.1141	3.39583	More than 999
RLL	.0236	.0736	3.11864	More than 999
Between superior men and superior women				
RRR	.0363	.0443	1.22039	889
RRL	.0377	.1666	4.55191	More than 999
RLL	.0220	.0867	3.94091	More than 999

TABLE IV
Chances that Percentage Differences of Table II are Significant

Formula	σd	Difference	Diff. / σd	Chances in 1000 that difference is significant
Between men and women				
LLL	.0576	.0738	1.28125	900
LLR	.0597	.1208	2.02345	978
LRR	.0588	.2100	3.57143	More than 999

It is rather curious that there is no evidence of significant handedness differences between superior and inferior women such as those found in the case of the two groups of men.

Within the left-handed group the question may be raised concerning the presence of significant differences in percentages between left-handed men and women. The calculations are given in Table IV.

As was true for the corresponding dextral types, so for the sinistral, a significantly greater proportion of men than of women belong to the LRR formula with a good chance that with a larger group a significantly greater proportion of women than of men would belong to the LLR type.

In Table II no division was made into superior and inferior subjects but the tabulation in Table V of sinistral tendencies for

21 male college professors and 15 subnormal boys is certainly impressive, since it is in harmony with the differences found in the dextral groups, particularly in the excess of LRR formulae for the superior group.

TABLE V

Formula	Male College Professors		Subnormals	
	No.	%	No.	%
LLL	7	33.3	2	13.3
LLR	3	14.2	10*	66.6
LRR	11	52.4	3*	20.0
Total	21	99.9	15	99.9

*In one case the batting habit was not stabilized.

The tabulation suggests the need of a resurvey of the left-handers in various institutions in order to determine the type of left-handedness most prevalent. It might also be worth while in the case of the feeble-minded to tabulate separately the manual habits of primary and of secondary aments.

(4) *Eyedness and handedness.* In the literature of handedness the question has frequently been raised concerning its connection with eyedness.

Recently Parson has reformulated in a bold unequivocal way the hypothesis that the preferred hand is determined by the sighting eye.[12] He states that "handedness results from an innate fault or limitation of binocular vision, on account of which all sighting or aiming operations are necessarily carried on unilaterally."[13] The necessity of monocular vision in sighting determines the dominant hand unless social pressure interferes to establish a crossed relationship. In spite of this determination of the preferred hand by the dominant eye Parson believes that handedness is *inherited,* probably according to Mendelian laws, left-handedness being a Mendelian recessive. Using an instrument of his own invention (the manuscope, recently renamed the manuoptoscope) Parson reports that about 30% are left-eyed; about 70% right-eyed; the same proportion would hold for right- and left-handedness, were it not for the operation of social pressure, that is to say nearly 30% of the population would be left-handed except for social interference.

Parson's hypothesis is supported by Mills who reports a very delicate technique for establishing eye-dominance.[14] He reports that "dextrality, aside from its recognized connection with a dominant right eye and a cyclophoric left eye, often is associated with more or less left hyperphoria and with the characteristic divergence of the left eye when a suitable test object is brought nearer than the convergence point. The reverse exists in sinistrality." Modifying conditions are described but the author concludes that a test for cyclophoria, or divergence inside of the convergence near point, may be considered as an almost infallible test for ocular dominance.

People fall into four groups, pure or crossed dextrals, and pure or crossed sinistrals. In the pure dextral there is right-handedness and a dominant right eye; in the pure sinistral, left-handedness and dominance of the left eye. In the crossed dextral there is right-handedness, but a dominant left eye; in the crossed sinistral left-handedness and a dominant right eye. The crossed dextrals, Mills explains, are "nearly always fundamentally left-handed but reversed by early training." The crossed sinistral is much the rarest combination and a "history often is obtained of injury to an arm, or of ocular disease or injury which explains the modification in ocular or manual dominance."

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[12] B. S. Parson, *Left-Handedness,* 1924, 1-185.

[13] *Op. cit.,* 51.

[14] Lloyd Mills, Eyedness and handedness, *Amer. J. Opth.,* 8, 1925, 1-9.

For five different series of 200 cases Mills reports the following percentages: "I found an average of pure dextro-ocularity in 76%; crossed in 13%; pure sinistro-ocularity in 9.3%; and crossed in 1.7%, which would place the total group of left-handedness at over 20%."[15]

Before stating the results of the present investigation that bear upon the relation of handedness to eyedness, I wish to state briefly points which seem to me inadequately developed by both Parson and Mills and which leave me unconvinced with respect to their conclusion that some 20-30% of the population are or should be left-handed.

(1) There is some inconsistency involved in the statements that the sighting line is a "direct instrument in the production of handedness" and that handedness is an inherited tendency. If handedness and eyedness are both dependent upon the same factor in heredity, an unambiguous test for eyedness might reveal native handedness, but it is difficult to see how eyedness can be said to produce handedness if the latter is already set by heredity.

(2) The relationship of both handedness and eyedness to a dominant cerebral hemisphere is left in doubt. One may, of course, confine oneself strictly to the *facts* revealed by the tests for eye-dominance and handedness and shelve the problem of hemispheral control. We are informed by anatomists that the macular portions of the two retinae, which are involved in sighting, are connected with both hemispheres of the brain. This may or may not be a fact of importance in the problem of relations between eye, hand, and brain. Parson states: "At least some of the maculary fibres of each eye are connected with both hemispheres."[16] He also remarks of his manuoptoscopic test "not the least important use to which it can be put is to enable us to find out *which cerebral hemisphere is the dominant one.*"[17]

(3) Both Parson and Mills state that their left-eyed subjects revealed, when questioned, sinistral tendencies so that there is strong evidence, they believe, that these left-eyed persons were naturally left-handed individuals whose native handedness had been tampered with. One would wish that these authors had been more explicit as to what tendencies they listed as sinistral. It is true that Parson describes many of his cases as shiftovers while Mills, in addition, writes, "some residual effect of an inherited tendency to sinistrality remained, such as batting or sweeping or wringing clothes with the left hand."[18] A glance at Table I (formula RRL) indicates that such an argument might prove too much since about 40% of men and over 50% of women sweep left-handed according to the usual notion. In addition, about 14% of men bat left-handed. One would scarcely dare to conclude that the left-handed outnumber the right-handed.

In fact, if Mills had questioned his right-eyed subjects concerning their manual habits as he questioned his left-eyed ones, he would have found many of them reporting sinistral tendencies such as left-handed sweeping and the like, and if such tendencies were considered significant he would have found his percent of crossed sinistrals greatly increased. From the standpoint of nervous instability, as we shall see, crossed dextrals and crossed sinistrals need intensive study. Genetic studies should also determine the relative time at which stabilization occurs for the sighting eye and for manual habits.

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[15] *Op. cit.*, 5. [16] *Op. cit.*, 23. [17] *Op. cit.*, 87 (italics Parson's). [18] *Op. cit.*, 5.

Neither Parson nor Mills considers the possibility of a sex factor operating in eyedness but Rider, in an earlier article, presented some interesting data with reference to this matter.[19] Rider emphasized the significance of monocular vision in many occupations such as those of the mechanic arts in which accurate sighting is necessary and argued that unilateral winking (or voluntary closure of one eye alone) serves an important function in connection with such monocular vision. He investigated with great care the degree to which unilateral winking could be used as a test of comparative visual acuity, since, a priori, one would expect the poorer eye to be closed during monocular vision.

Name, eye, sex, manner of winking, acuity of vision without mydriatic and with, in cases in which its use was required, correcting lenses with resultant vision and various other facts which would tend to throw light upon the history of visual conditions were recorded for 2000 eye patients, seen mostly in private practice.

Among the equal-eyed, there were two classes, those winking equally and those winking unequally. The first class was numerically very much greater than the other. Habit, in the second class, operates to determine the closing of the left eye even when there is equality of vision in order that the right eye may work in harmony with the right hand. Rider apparently holds that in such case *right-handedness determines right-eyedness,* the reverse proposition to Parson's. He adds that this cause is more potent in men than in women because of differences in occupation. "While 80 per cent, or four-fifths, of the equal-eyed men who wink one eye better than the other unconsciously wink the left eye, the proportion among women is but 58 per cent." [20]

Three subdivisions occur among the unequal-eyed; those who wink equally, those who wink the better eye more easily, and those who wink the poor eye more easily. The last class is numerically much larger than the other two. If those who wink equally are eliminated, 86% are found to close the poorer eye. If cases are selected in which there is great difference in visual acuity the percent of those winking the poorer eye increases. We are pretty safe in saying that in the case of a man winking his right eye more easily than his left, his right eye is the poor one. Of unequal-eyed individuals who wink the better eye more easily a large proportion are myopes.

Rider's results are worthy of study, particularly with reference to the sex-difference he reports, to which reference will be made later.

(5) *Eyedness and handedness formulae.* In connection with the handedness formulae cited in the first part of the paper, I endeavored to obtain the sighting eye. In the questionary which I used the sighting eye was determined by the following simple experiment.[21]

Try the following experiment. Use a small finger-ring; hold it at arm's length, and encircle a small object a few yards away.

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[19] Wheelock Rider, Unilateral winking as a test of comparative visual acuity, *Trans. Opt. Soc.*, 8, 1898, 434-452.

[20] *Op. cit.*, 447.

[21] Cf. references cited by Clarence Quinan, Sinistrality in relation to high blood pressure and defects of speech, *Arch. Int. Med.*, 27, 1921, 257-261.

I. Focus on the object. Now close your right eye. Check what happens.

(A) No change
(B) Ring moves to right of object
(C) Ring moves to left of object

II. Refocus. Close your left eye. What happens?

(A) No change
(B) Ring moves to right of object
(C) Ring moves to left of object

Repeat this test until you are confident of the accuracy of your report.

In the above experiment, it should be noted, the right-eyed check I B and II A; the left-eyed, I A and II C; the either-eyed individual may check I B and II C.

The experiment is a simple one but there are numerous sources of error, for example:

(1) Inaccuracy in following directions, such as a slight movement of the head or arm after eye-closure so as to bring the object into focus.

(2) Inaccuracy in report, sometimes due to a tendency to report on the movement of the object rather than movement of the ring. In some cases there is a real confusion in discrimination of right and left. I have noted many of these in my work on subjects and one eminent psychologist penciled a note on the margin of his questionary to the effect that his first report had been inaccurate just because of such spatial confusion.

(3) Voluntary control of the sighting eye, due to expectation or other form of mental set. Sometimes, for instance, the use of right or left arm suggests the line of fixation. The most common error is, I believe, the report of I B and II C.

Because of the possibility of errors as listed above, my percentages are undoubtedly subject to some discount. In the work with groups less well trained than the professional psychologists results have, usually, been checked by an experimenter. Moreover, after the appearance of Parson's book, while the experiment was in progress, his instrument, the manuoptoscope, was obtained and used in connection with the ring test. In general, the results from the two were consistent although a small group was found who sighted *with one eye for far vision and with the other for near vision.* Moreover, Parson's test exhibits the same sources of error, except (2), that were found in the ring test. In extremely right- or left-eyed persons, however, it was possible for the experimenter to judge the sighting eye by the direction in which the tube of the manuoptoscope was pointed. Besides the ring and manuoptoscope test, unilateral winking and eye-opening was appealed to in doubtful cases.

In spite of the care used the records are far from accurate, particularly in the case of the left-handed, where only a small proportion of subjects could be personally tested. None the less, the attempt to bring together handedness formulae and sighting eye is of great importance since it indicates the complexity of the problem at which we are working.

Table VI summarizes the sighting eye for 585 right-handed men and 417 right-handed women, superior and inferior groups being indicated as before and Table VII for 131 left-handed men and 100 left-handed women. The numbers are smaller than in the preceding tables since in some cases, for example the Vineland Group, the sighting eye was not checked.

The probability of significant percentage differences in Tables VI and VII is summarized in Tables VIII and IX.

TABLE VI

Eyedness and Handedness—1002 Right-handed

Formula	Men						Women					
	Superior Group			Inferior Group			Superior Group			Inferior Group		
	Total No.	No. R-Eyed	% R-Eyed	Total No.	No. R-Eyed	% R-Eyed	Total No.	No. R-Eyed	% R-Eyed	Total No.	No. R-Eyed	% R-Eyed
RRR	138	111	80.4	119	83	69.7	110	77	70.0	46	27	58.7
RRL	120	88	73.33	118	74	62.7	151	92	60.9	79	53	67.1
RLL	61	48	78.70	29	23	79.3	24	16	66.7	7	5	71.4
Total	319	247	77.4	266	180	67.7	285	185	64.9	132	85	64.4

TABLE VII

Eyedness and Handedness—231 Left-handed

Formula	Men			Women		
	Total No.	No. L-Eyed	% L-Eyed	Total No.	No. L-Eyed	% L-Eyed
LLL	37	22	59.5	36	29	80.6
LLR	30	18	60.0	39	26	66.7
LRR	64	34	53.1	25	11	44.0
Total	131	74	56.5	100	66	66.0

TABLE VIII

Chances that Percentage Differences of Table VI are Significant

Formulae	σd	Diff.	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1000 that difference is significant
Between superior men and inferior men				
RRR	.0540	.1070	1.98148	976
RRL	.0601	.1063	1.76872	961
RLL	.0928	.0060	.06465	525
Total	.0370	.0970	2.62162	996
Between superior men and superior women				
RRR	.0607	.1040	1.71334	956
RRL	.0597	.1243	2.08208	981
RLL	.1221	.1200	.98290	837
Total	.0402	.1250	3.10945	More than 999

TABLE IX

Chances that Percentage Differences of Table VII are Significant

Formula	σd	Diff.	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1000 that difference is significant
Between men and women				
LLL	.1042	.2110	2.02495	978
LLR	.1170	.0670	.57265	716
LRR	.1172	.0910	.77645	781
Total	.0642	.0950	1.47975	930

The chances are more than 996 in 1000 that more right-handed men of the superior group (total of all formulae) are right-eyed than those of the inferior group, and that more superior men than superior women are right-eyed, with an excellent chance (981 in 1000) that RRL superior men are more apt to be right-eyed than women of the same formula.

It is, of course, possible that this sex difference is determined by the training men receive in sighting with the right eye in shooting, in various games, and in using tools such as the plane. Rider, who found a similar sex difference in his test on unilateral winking, was, as stated before, inclined to explain it in terms of the different pursuits of the two sexes.

The fact that the same difference is found when superior and inferior groups of right-handed men are compared should, however, incline us to caution in drawing such a conclusion. 'Crossed dextrals'—to use Mills' terms for the right-handed but left-eyed—are by certain investigators described as nervously unstable. The group of inferior men (psychopaths, defectives, and delinquents), might well be characterized by an undue amount of crossed-dextrality and in consequence by nervous instability. At least the possibility is worth further extensive investigation.

The summary for the left-handed shows some variation from that for the right-handed. As between left-handed men and women there is an excellent chance (978 in 1000) that more LLL women are left-eyed than LLL men. Again, the appeal to training is possible, more men than women being shifted to the right eye.

It is important here to note how definitely right-eyed the RLL group of men is, although the corresponding type (LRR) is not similarly left-eyed.

It is interesting to utilize the percentages given in Table VI and VII in determining the degree to which the present investigation agrees with former ones as to the amount of crossed dextrality and crossed sinistrality, that is, where dominant eye and hand are on opposite sides of the body.

The first summary relates to crossed dextrality, or the percentage of presumably right-handed persons who are left-eyed. Ballard in various series of tests estimates left-eyedness in right-handed children from 20-55%.[22] Quinan reports a great difference in the amount of left-eyedness depending upon the group tested. Among 572 right-handed inmates of a home for the aged he found about 24% left-eyed.[23] Two other observations of 100 each on machinists and on musicians gave, for 96 right-handed machinists, about 4% of left-eyedness and for 92 right-handed musicians about 26%.[24] This outcome is in harmony with Quinan's belief that crossed dextrality is symptomatic of a psychopathic constitution.

It is difficult to estimate from Parson's and Mills' figures the percentage of left-eyedness among the right-handed since their percentages are given in terms of the amount of right- and left-eyedness in total groups with only incidental observations on the manual habits except for out and out left-handers. They believe that all the left-eyed are natively left-handed, except in cases where ocular fault has developed. Parson's figures indicate that he found 180 left-eyed children (23%) among 784 presumably right-handed children.[25] From his total of 877 I have deducted not only 36 left-handed and 12 of impartial vision but also 45 who are listed as shiftovers or ambidexters. Mills reports 13% of crossed dextrals in a total of 1000 cases.[26] If the calculation is based on the 900 right-handed in this group of 1000, this percent is raised a trifle. Mills states, however, that about half of his crossed dextrals were actually ambidexters or shiftovers, and if the correction indicated is made, he actually found less than 8% who were crossed dextrals.

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[22] P. B. Ballard, Sinistrality and speech, *J. Exper. Ped.*, 1, 1911-12, 298-310.

[23] *Op. cit.*, 257.

[24] C. Quinan, A study of sinistrality and muscle coördination in musicians, iron-workers, and others, *Arch. Neur. & Psychiat.*, 7, 1922, 352-360.

[25] *Op. cit.*, 106-113. [26] *Op. cit.*, 5.

My own figures agree fairly well with those of other investigators who have sought to determine merely the 'sighting eye,' not necessarily eye dominance, at least in the records for the superior men, where I found about 25% of crossed dextrals. The groups of inferior men and of women had about 34% crossed dextrals, which is high, except as compared with some of Quinan's figures.

A second summary relates to crossed sinistrals, or left-handedness associated with right-eyedness. Ballard found that of 51 left-handed individuals 57% proved to be right-eyed.[27] Quinan found 50% of a group of 28 left-handers, right-eyed,[28] and about 21% of another group of 14.[29] Parson apparently found only 4 cases of left-handed children among his 608 right-eyed children.[30] It is not, however, clear that he questioned these children in order to discover how many had been forced to use the right hand in the same way that he questioned the children who proved to be left-eyed. Mills reports 17 cases, 15.4%, of left-handed individuals having a dominant right eye in a total of 110 left handed.[31] My estimate of crossed sinistrality (based on Table VII) is high, from 43.5% in men to about 34% in women.

Whatever conclusions may be drawn with reference to the origin of crossed dextrality or crossed sinistrality, the condition itself certainly deserves attention. Possibly the condition originates from the interference with native handedness by training or disease or from the development of ocular faults, or is even influenced by eye habits determined by social pressure. On the other hand, it is also possible to believe that crossed dextrality or sinistrality may be due to asymmetrical functioning of the nervous system. In any case Quinan's hypothesis that such crossed functioning is bound up with psychopathic tendencies is worth consideration. The general pattern of mental organization certainly appears to be influenced greatly by such asymmetry as having an opposite preference for hand and eye. The experimental results to be presented later show this.

One group of *S*s has been very inadequately handled in the present report namely those who are either-eyed, or according to Parson's terms 'impartial in vision.' My own observations on this group led me to discriminate two sub-groups (1) those where there was a fluctuation in the sighting eye, and (2) those who were right-eyed for near vision and left-eyed for far vision or the reverse. My RLL handedness type gave me a number of the latter cases but I am not yet ready to draw any conclusions with reference to them.

(6) *The dominant thumb.* Lutz was, so far as I know, the first person to call attention to the fact that the same thumb is

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[27] *Op. cit.*, 304. [28] *Op. cit.*, *Arch. Int. Med.*, 27, 1921, 259.

[29] *Op. cit.*, *Arch. Neur. & Psychiat.*, 7, 1922, 359.

[30] *Op. cit.*, 107. [31] *Op. cit.*, 5.

for any given individual usually outside when the hands are clasped with the fingers alternating.[32]

On the basis of his returns from about 600 individuals Lutz concluded that the position of the thumb had apparently no relation to right or left-handedness and that no significant sexual dimorphism exists, but that the mode of clasping the hands is inherited. I have reported elsewhere my conclusions with reference to the dominant thumb.[33] I wish to repeat them here and to give a table covering a greater number of cases than that recorded in the article cited.

Contrary to Lutz's conclusions, I believe I have some evidence that the position of the thumb is related in some obscure way to right or left-handedness and that sex differences do exist. His data concerning the inheritance of the trait, however, appear conclusive. My results are summarized in Table X. In Table XI calculations are given concerning the chance of various percentage differences in Table X being significant.

TABLE X

Thumb Out When Hands are Clasped

Formula	Men			Women		
	No.	No. R-Thumb Out	% R-Thumb Out	No.	No. R-Thumb Out	% R-Thumb Out
RRR	242	114	47.1	129	79	61.2
RRL	249	130	52.2	213	118	55.4
RLL	98	58	59.2	32	17	53.1
Total	589	302	51.3	374	214	57.2
LLL	34	10	29.4	34	16	47.1
LLR	41	19	46.3	39	19	48.7
LRR	68	24	35.3	26	11	42.3
Total	143	53	37.1	99	46	46.4

It will be seen from Table X that 51.3% of right-handed men and 37.1% of left-handed men have the right thumb up. The chances are more than 999 in 1,000 that this difference is significant, that is, that more right-handed then left-handed men clasp the hands, right thumb out. The approximately 50-50 proportion for right and left thumbs out in right-handed men must be interpreted in the light of a 37-63 proportion for left-handed men. Not only is there a significant difference for the total of right-handed and left-handed men but also a significant difference in the case of RLL and LRR men, where again the chances are 999 in 1,000 that the percentage difference is significant, that is to say that RLL men in a significantly greater number of cases place the right thumb out than LRR men do, an outcome uniform to that found for the dominant eye. There is also an excellent chance, 982 out of a thousand, that the percentage difference for RRR and LLL men is significant.

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[32] F. E. Lutz, The inheritance of the manner of clasping the hands, *Amer. Natur.*, 42, 1908, 195.

[33] J. E. Downey, Further observations on the manner of clasping the hands, *Amer. Natur.*, 50, 1926, 387-391.

Significant differences are not found in the records of the women, although possibly with larger groups they might occur. In the case of the total percentages for right-handed and left-handed women the chances in 1,000 that the difference is significant are 972.

TABLE XI
Chances that Percentage Differences of Table X are Significant

Formula	σd	Diff.	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1000 that difference is significant
Between right and left-handed men				
RRR and LLL	.0845	.1770	2.09467	982
RRL and LLR	.0841	.0590	.70155	759
RLL and LRR	.0763	.2390	3.13237	More than 999
Total rights and total lefts	.0453	.1420	3.13466	More than 999
Between right and left handed women				
RRR and LLL	.0958	.1410	1.47182	930
RRL and LLR	.0870	.0670	.77011	780
RLL and LRR	.1310	.1080	.82443	795
Total rights and total lefts	.0563	.1080	1.91829	972
Between men and women				
RRR	.0536	.1410	2.63059	995
RRL	.0465	.0320	.68817	754
RLL	.1012	.0610	.60277	727
Total	.0328	.0590	1.79878	964
LLL	.1159	.1770	1.52718	937
LLR	.1117	.0240	.21486	585
LRR	.1129	.0700	.62002	733
Total	.0644	.0930	1.44409	926

A comparison of the records for men and women indicates one significant difference, namely in the percentage of RRR men and women who put the right thumb out, more women than men being right-thumbed.

It is possible that the placing of the left thumb outside points to a slight degree of latent sinistrality since Lutz has presented evidence that the manner of clasping the hands is a heritable trait. The problem raised as to the significance of this trait is interesting particularly in the light of the sex differences that are suggested.

The experimental investigation to be reported later gave only slight indication that the dominant thumb deserved consideration. However[1] my groups became so small when subdivided on the basis of (1) unimanua and bimanual preference, (2) dominant eye, (3) dominant thumb, that I frequently ignored the latter basis of division and hence failed, perhaps, to reckon with it adequately.

Possibly a word should be added about the very small number of individuals, a fraction of one percent, who are either-thumbed, that is, place indifferently either thumb out when clasping the hands. Such individuals are usually highly unstable in other respects also, that is, they exhibit only a mild unimanual and bimanual preference and are apt to be either-eyed. They represent one type of ambidextral individual described previously.

(7) *The spade foot.* In my questionary investigation I asked for a checking of the spade foot. Rife says as regards the spade foot, "the fact is always overlooked that the foot used in spading is determined entirely by the bimanual dextrality,"[34] that is, individuals of RLL or RRL formulae will use the left foot on the spade, and LRR and LLR, the right foot. RRR individuals will use the right foot, and LLL, the left.

In checking over my questionary returns I found that for about 70% the spade hand was the same as the broom hand, that is, in the use of both instruments the same hand was placed toward the center. According to Rife we expect *complete agreement between the spade hand and the spade foot.* I found out of 226 respondents (men psychologists) some 44 cases in which the spade hand and spade foot were reported as opposed, that is, the right foot would be placed on the spade although the left hand was below the right in the use of this implement, or the reverse. Accordingly I sent out a second questionary to these 44 individuals, asking for a second report on the spade foot and hand. Thirty-seven of these questionaries were returned. I found that in 20 cases some error had been made in the earlier returns. The number of men placing the right hand down on the broom but the left hand down on the spade was increased over the first estimate. A number of others alternated right and left hand and foot on the spade.

Eleven men reported (two not confidently) that spade hand and foot were on opposite sides of the body. Their handedness formulae were as follows: 7 RRL, 1 RR$_R$, 2 RRR, and 1 RLL. In all of these cases the right spade foot was used with the left hand down on the spade, never the reversed situation. It is quite possible, as one respondent suggests, that spading for these individuals is really a unimanual performance, the right hand on the handle being competent to manipulate the implement. Shovelling would be a wholly different matter.

In general, Rife's contention that the spade foot follows the bimanual dextrality was strongly confirmed. LRR and LLR individuals place the right foot on the spade just as RRR individuals do. Curiously enough, 5 left-handed psychologists, LRR in type, reported in the first questionary that the left foot was their spade foot. All of them corrected this report in the second questionary. One RLL individual reports that the right is the spade foot.

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[34] *Op. cit.*, 479 f.

Dextropedality has been very little investigated, certainly not to the extent it deserves. Ballard remarks that Lewis found 12 left-handed boys to be also left-footed. He himself reports "that 88 per cent. of the pure sinistrals were left-footed, and 86 per cent. of the dextro-sinistrals."[35] He adds: "It is not impossible that if acts of greater skill were required of the foot a still higher degree of correspondence between hand and foot would be revealed."

Rife makes the interesting suggestion that possibly the "faculties of springing and kicking are as independent of each other as uni- and bimanual dextrality and possibly occur in somewhat the same ratio, that is, the great majority of people kick with the right foot while most of them spring with the left."[36]

Apart from my observations on the spade foot I have little information to give. In an experiment to be reported by Dr. R. S. Uhbrock, a record of tapping by both the right and left foot was obtained for 100 boys. Dr. Uhrbrock found 14 boys who tapped more rapidly with the left than with the right foot. One of these was a left-handed boy, formula LLR. The formulae of the other 13 have not been determined.[37]

(8) *Handedness formulae and degree of unidextrality.* Statistics have been given to show the relative frequency with which different forms of dextrality occur and the possibility of significant percentage differences in their frequency for certain formulae and for preferred thumb and eye when comparisons are instituted between the sexes and for different groups within the same sex. The next question that arises is this: are the handedness formulae related in any way to variations in degree of unidextrality?

In an attempt to answer this question, three tests were made of groups of *S*s belonging to different handedness types. The tests chosen were (a) pegging, (b) dynamometer, and (c) dexterity tests.

If the handedness formulae are significant one would anticipate finding RRR individuals more definitely unidextral than either RRL or RLL persons, although quite probably individuals of mild manual preference are most influenced by social pressure and so forced into the RRR group, a possibility which would blur results somewhat. Moreover the group of RRR subjects whose records are to be cited here contained some left-eyed and left-thumbed persons and was, therefore, not made up of pure dextrals. This defect in method was regrettable but unavoidable if the groups were to be kept of any size.

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[35] *Op. cit.*, 306. [36] *Op. cit.*, 480.

[37] For an interesting discussion of right- and left-sidedness in relation to rhythm see M. F. Meyer, *Psychology of the Other One*, 339 ff. Meyer concludes, "So, whenever there is any need or occasion for division of labor, the left leg does the heavy and the right leg the light (but skillful) work" (341).

The present series of tests were tried only on women. Comparable records should also be made with men. This is desirable especially for a comparison of RRR and RLL groups. So few women belong to this latter group that the results in this case are inconclusive.

In all tests the technique of procedure was carefully standardized. One great source of error was, however, evident throughout, namely, the failure of the *S*s to work at their greatest possible speed at all times. Sometimes a desire to excel with one or the other hand appeared to influence the effort put forth. Records are given in terms of total performance for both right and left hands and in terms of a ratio obtained by dividing the right hand record by the left hand one.

(*a*) *Pegging Test.* For the pegging test the ordinary primary pegging board was used with wooden pegs. The board was 10 in. sq. and contained 100 shallow holes in 10 parallel rows. As many pegs as possible were set up in the holes during one minute's continuous work. A peg that fell after placement was counted. The right hand moved toward the right; the left hand toward the left. Three 1-min. tests were given to each hand in the following order: right, left, left, right, right, left. An average was obtained for the 3 trials with each hand and the ratio of performance for right and left hand calculated by dividing the average for the right hand by the average for the left hand. A long practice series was run for one individual of each handedness formula. This test will be reported by Miss Hazel Bowman.

The procedure in pegging was carefully standardized. The hand that was not pegging held the pegs and fed them to the other. To a certain extent the operation proved to be a bimanual one, rather than a unimanual one, a circumstance which reduces its value as a test of unidextrality, since skill with the left hand would speed up the pegging. Inequalities in the performances with the two hands are, however, evidenced by the fact that the coefficient of correlation for speed of the two hands in a group of 60 *S*s was only +.43.

Table XII gives the results for the different formulae in terms of the mean speed for each hand and the mean ratio when the record for the right hand is divided by that for the left. There is some indication that the RRR group has the advantage in this test both with respect to the average total number of pegs placed and the ratios indicative of degree of unidextrality. The differences are small but consistent. The m.v.'s on the mean ratio indicate, however, considerable overlapping of the groups.

Reversals of handedness, that is, cases where the left hand made the better record (ratio under 1.00) occurred 3 times in the RRL_3 group but in no other group. There were also 3 instances of practical equivalence, ratio 1.05 or under, for the two hands in the RRL_3 group; one of equivalence for the RRR group; and 3 for the RLL group.

The long practice series carried out on one of each handedness type gave satisfactory evidence of the greater unidextrality of the RRR *S*. More than 100 trials with each hand were given to 4 *S*s with the following results:

S		*Ulna-Plus Difference*[38]	*Average Ratio*
R	RRR—Right-Eyed—Left Thumb	Pronounced	1.34
D	RRL_3—Left-Eyed—Right Thumb	Medium	1.15
C	RRL_1—Either-Eyed—Left Thumb	Vanishing	1.06
B	RLL—Either-Eyed—Left Thumb	Vanishing	1.17

The average difference in number of pegs placed by the right and left hand was as follows: R, 19.54; D, 9.24; C, 3.26; B, 12.03.

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[38] See Section 9.

TABLE XII

	Pegging						Dynamometer					
Formula	No. *Ss*	Mean 3 trials Right hand	Mean 3 trials Left hand	Excess Right hand	Mean ratio* R/L	M.V. on mean ratio	No. *Ss*	Mean 10 trials Right hand	Mean 10 trials Left hand	Excess Right hand	Mean ratio* R/L	M.V. on mean ratio
RRR	29	47.50	35.14	12.36	1.36	±.106	18	63.34	54.32	9.02	1.19	±.100
RRL$_1$	24	46.55	35.99	10.56	1.29	±.090	19	60.17	54.47	5.70	1.12	±.102
RRL$_3$	32	45.57	37.52	8.05	1.21	±.117	22	56.33	54.75	1.58	1.11	±.103
RLL	9†	44.60	37.50	7.10	1.18	±.099	9†	64.84	56.41	8.43	1.15	±.100

*Mean ratios were calculated from the ratios for individual *Ss*.
†Possibly one *S* in this group is wrongly classified since by all tests she appeared to be strongly right-handed. See footnote 43.

TABLE XIII

	Dexterity 1						Dexterity 2					
Formula	No. *Ss*	Mean no. taps Right hand	Mean no. taps Left hand	Excess Right hand	Mean ratio R/L	M.V. on mean ratio	No. *Ss*	Mean no. taps Right hand	Mean no. taps Left hand	Excess Right hand	Mean ratio R/L	M.V. on mean ratio
RRR	17	354.0	290.0	64.0	1.22	±.075	17	523.6	405.7	117.9	1.30	±.093
RRL$_1$	14	330.7	288.6	32.1	1.15	±.084	14	514.1	413.2	100.9	1.24	±.076
RRL$_3$	14	336.8	297.1	39.7	1.14	±.062	14	533.6	435.0	97.7	1.23	±.123
RLL	8†	362.1	290.75	71.4	1.22	±.108	8†	501.4	413.2	88.2	1.22	±.135

*Mean ratios were calculated from the ratios for individual *Ss*.
†Possibly one *S* in this group is wrongly classified since by all tests she appeared to be strongly right-handed. See footnote 43.

(*b*) *Dynamometer Test.* For measuring the strength of hand grip with the two hands a dynamometer was used. Twenty readings under standardized conditions were taken, 10 for each hand, alternately, with proper rest intervals. The mean record was calculated for each hand and a ratio obtained to show the degree of superiority or inferiority of right over left hand (see Table XII).

Although the relative strength of the two hands has been considered by many investigators a very dubious test of handedness, the present classification into handedness groups indicates a superiority for the RRR type in the dynamometer test as well as in the pegging and dexterity tests. The mean ratios in the dynamometer test are, however, considerably lower than in the other tests.

Reversals (ratios of less than 1.00 indicating superiority of the left hand) are, moreover, more frequent in this test than in the two other tests. Nine occurred for 69 *S*s distributed as follows: RRR 1 (18 *S*s); RRL_1 3 (19 *S*s); RRL_3 4 (22 *S*s); RLL 1 (9 *S*s).

Practical equivalence (ratio under 1.05) occurred once each for the RRR and the RRL_1 group and 4 times for the RRL_3 group.

(*c*) *Dexterity Tests.* For the dexterity test Scott's three-hole tapping apparatus was used. There was a 1-min. preliminary tapping test for each hand in which only one hole was used. This test is labelled 'Dexterity 1.' This was followed by 4 tests each 2 min. long, in which the 3 holes were utilized. The hands were tested in the following order; right, left, left, right. This test is labelled 'Dexterity 2.' The total number of contacts registered for each hand was used as the basis on which the ratios were calculated. The outcome is summarized in Table XIII.

As before, the results are, with one exception, (RLL, Dexterity 1) in favor of the RRR group for both Dexterity 1 and Dexterity 2. The *S*s who are classified under the formula RRR and who are also right-eyed and right-thumbed give a ratio of 1.41 for Dexterity 2. It is possible that the eye habits affect the outcome of this test.

Reversals of handedness (ratio under 1.00) occurred twice in Dexterity 1, in both instances for *S*s of the RRL formula. A practical equivalence of scores for the two hands (ratio under 1.05) occurred twice for RRL_3 *S*s and once for RRL_1 *S*s. There were no reversals of handedness in Dexterity 2 and a practical equivalence for only one RRL_3 *S*.

Conclusion. There is certainly some evidence that handedness formulae and degree of unidextrality are related but the experimental investigation needs to be supplemented by use of larger and more rigidly selected groups.

(9) *Structural differences and handedness types.* The question concerning the origin of dextrality types in native or acquired tendencies may possibly be settled by the discovery of significant structural differences between them.

(*a*) *'Ulna plus.'* The structural difference that has been suggested most frequently as a possible test of right or left handedness is that of relative bone measurements for the two halves of the body. Jones suggested measurements of the humerus and ulna, plus the distance to the middle knuckle of the little finger, and of the circumference of palm and wrist. He devised a special measuring instrument, the brachiometer, to get data on the humerus and the 'ulna plus.'

Beeley, who used the Jones' brachiometer in a test with young children, drew the following conclusion:[39] "The experimental application of the Jones' Brachiometer Test to 123 young children proves its unreliability for extensive use and also the invalidity of the theory upon which it is based, viz., that native handedness can be detected at any age by bone measurements."

Watson also reached a negative conclusion with reference to the value of measurements of right and left anatomical structures, such as width of right and left wrist, palm, length of forearm, etc.[40] He reports that measurements have been made upon several hundred children with specially devised instruments, but with results that show there is no significant difference in the right and left measurements. The average error of the measurement is reported to be greater than the observed difference.

Schwerz reports some interesting material on this point and cites bone measurements on adults from a number of investigators, who dealt each with a small *racial* group, a fact possibly of considerable significance.[41] Measurements were taken not only of the humerus, radius, and ulna but also of the femur and tibia. He deplores the inadequacy of the material and general lack of interest by anatomists in the comparison at stake, since if significant differences in bone-measurements should be discovered and associated with right- or left-handedness, a valuable method is at hand for determining racial differences in handedness, and, as he suggests, old graveyards could be made to yield profitable information. His general conclusions from study of the data at hand are as follows: there is a greater percentage of cases in which the left side measurements of radius and ulna exceed the right side measurements than is true for the measurement of the humerus, that is, the measurement of the humerus is more significant than the measurement of the ulna and radius. When all hand and arm measurements are in harmony, the presumption of handedness is greater than when the measures are inconsistent. The figures quoted by Schwerz show curious discrepancy of results for arm and leg. In the majority of cases the left femur and tibia were longer than the right tibia and femur. Schwerz remarks that the left leg is longer than the right and that our knowledge of the innervation of right and left side is very inadequate.

The problem of comparative bone-measurements is a very big one and obviously one to be handled by trained anatomists and anthropologists. My own material on this point is presented with some hesitation since I am thoroughly aware of its inadequacy. Yet the important point, as I see it, is the recognition of degrees of dextrality, which may be tied up with degrees of difference in bone-measurements. Massed records may easily obscure the point at issue. If possible, groups that show a big difference in bone-measurements on either side should be compared for handedness habits with groups that show a vanishing difference. That it is possible to identify such groups, at least among adults, I am convinced from my experience. In many cases a pronounced difference in ulna measurements, quite outside of range of error in measuring, is evident.

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[39] Quoted by Parson, *op. cit.*, 77.

[40] *Op. cit.*, 319.

[41] F. Schwerz, Die Rechtshandigkeit des Menschen, *Arch. f. Rassen- u. Gesellschbiol.* 11, 1914, 299-314. See also authorities quoted by Clarence Quinan, *Arch. Int. Med.*, 27, 1911, 256-261.

As part of the routine procedure in connection with getting the material reported in Section 8 of this paper three measurements each of the 'ulna plus' were obtained for right and left arms by means of Jones' brachiometer and an average taken. The instrument is not a delicately contrived one and, in addition, errors in manipulation are pronounced, so that inaccuracies in measurements are numerous, a vital matter when dealing with what is at best only a slight difference in lengths. One would like to see such measurements handled by X-raying of bones.

Altogether we have measures of the 'ulna plus' for 152 women, some of whom had been measured in a previous study. The median difference between the two arms was found to be about ⅛ in. A distribution of the measurements reported by Jones for women over 16 years of age gave the same median measurement.[42] The mean of our measurements was slightly less than 1/7 in. with a mean variation of about 1/12 in.

My assistant, Miss Bowman, and myself measured independently the same group of 26 college girls, taking three measurements each for right and left 'ulna plus.' We found that the mean difference in our 52 measurements was slightly less than 1/12 in., and that our mean difference in calculation of the 'ulna plus difference' was about the same, namely, 1/12 in.

TABLE XIV

Formula	Ulna Plus Difference			
	Vanishing		Pronounced	
	No.	%	No.	%
RRR	3	13.6	11	42.3
RRL	13	59.0	14	53.8
RLL	6	27.3	1	3.8
Total	22	99.9	26	99.9

From study of these measurements it appeared that if we were to select individuals who have a difference of approximately ¼ in. or more between the right and left 'ulna plus' and those who have a difference of 1/16 in. or less we would have groups whose difference in at least one bone-measurement would lie outside the range of error. Roughly, these groups correspond to the highest and lowest quartile as obtained in the distribution of the 152 girls.

Selection was accordingly made of all women with a difference between right and left 'ulna plus' of ¼ in. or more, and of 1/16 in. or less. Of this number I found that I had handedness formulae for some 48—for 22 who gave a vanishing difference between the two ulnas, and for 26 who gave a pronounced difference in ulna-lengths. I give above a tabulation of the handedness formulae.

The results of the tabulation seems to me highly promising, particularly for the RLL group. I discovered from study of my records that I had measurements of the 'ulna plus' of only 7 girls classified as RLL, and here I found six of them with a vanishing difference in length between the right and left ulna![43]

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[42] W. F. Jones, A study of handedness, *Univ. of South Dakota Pub.*, 1918, 1-80.

The contrast between these two groups is also maintained in the records taken for pegging, dynamometer, and dexterity tests, with one exception, namely Dexterity 2, where I believe the dominant eye complicates results.

To repeat, the results are distinctly promising. The experiment should be carried further. Bone measurements other than those of the 'ulna plus' should be used and an effort should be made to get a large group of RLL individuals. To obtain such, a group men subjects should be utilized.

TABLE XV

Test	Ulna Plus Difference			
	Vanishing		Pronounced	
	No.	Mean Ratio	No.	Mean Ratio
Pegging	23	1.25	22	1.33
Dynamometer	13	1.08	15	1.17
Dexterity 1	13	1.15	11	1.22
Dexterity 2	13	1.29	11*	1.27

*4 are left-eyed.

Skepticism concerning the value of bone-measurements as a test of native handedness has also been stimulated by reported discrepancies. Right-handed individuals may, for example, show an excess on the left side and left-handed individuals on the right. In all reports of this type the children measured are classified merely as right- or left-handed with no attention given to the type of dextrality. It is, therefore, of interest to canvas our material and to study discrepancies in the light of other records. It should, however, be stated that a summation of measures for hand and arm would be more satisfactory material than our single measure, namely, length of the 'ulna plus.'

Of 141 presumably right-handed women we found 14 (9.9%) with the left 'ulna plus' longer than the right. On 9 of these 14 cases we had data obtained in the present study. *Seven* of the 9 were classified under the group of 'Vanishing Difference' for right and left ulna; in every case the difference was less than 1/16 in. and so within the range of error. Five of the 7 were either left-eyed or either-eyed; two belonged to the formula RLL. There was only one RRR and right-eyed individual in the group which evidently is not a strongly right-handed one. Pegging and dynamometer ratios were also usually low for members of it.

Two of the 9 discrepant cases gave a pronounced difference in favor of the measurement of the left side. It amounted to ¼ in. for one girl. This *S* is, however, a cripple on the right side due to infantile paralysis at four years of age, and should not be cited adversely. It is, however, interesting to note that she is extremely right-handed in spite of her handicap. The second case, with about ⅛ in. difference in favor of the left side, is probably a naturally left-handed individual. Her pegging ratio was .89 (in favor of the left hand); her dynamometer ratio .96 (in favor of the left);

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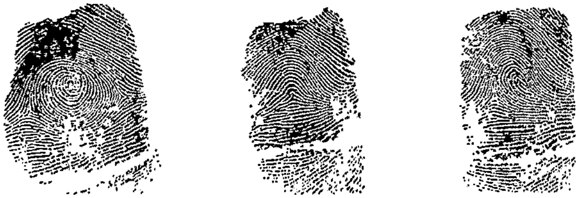
[43] I suspect the one individual of the RLL group who has a large difference between right and left ulna is wrongly classified as to handedness formula. The results of pegging, dynamometer, and dexterity tests suggest a strongly right-handed person. I have had no chance to recheck her reports on her batting and broom habits. See footnotes to Tables XII and XIII.

and her dexterity ratios 1.00 and 1.01. She reports certain left-handed tendencies but she appears to have been under no pressure in developing right hand writing. She is either-eyed and left-thumbed, RRL_1.

The above summary emphasizes the inadequacy of classifying individuals into two groups only if justice is to be done to the bone-measurement theory. The fact of importance is the degree of unidextrality.

Of 17 presumably left-handed women we found 3 (17.6%) with the right 'ulna plus' longer than the left. Of these three cases of discrepancy among left-handed individuals records were available for one only. This individual, who appears to be strongly left-handed but has been forced to use the right hand, writes a rapid and beautiful right-hand script. She stutters very badly, a fact usually attributed to the fact that she is a shiftover. Her formula is LLR, left eye, right thumb. This case deserves further study.



WHORL ARCH LOOP

PLATE 1. Finger Print Patterns

(*b*) *Finger Prints.* As a second attempt to determine whether possibly structural difference are found between right and left-handed or between handedness types I made use of finger-print material, availing myself of Bonnevie's tabular summaries of the patterns characteristic of each of the 10 fingers.[44]

Since, as Bonnevie has shown, characteristic differences are found in right and left hands, it occurred to me to ask whether any of these differences would be reversed in a left-handed group. Also it seemed possible that finger-print material might be used to determine whether contrasting types such as LLL and LRR, or RRR and RLL would exhibit differences with respect to degree of symmetry or asymmetry of pattern.

Bonnevie's table for percentages of total frequency of patterns and for frequencies of each pattern for each individual finger is taken from a total of 245,180 fingers. The sexes are treated together and the right and left-handed included without distinction. The classification into patterns is the generally recognized one of whorls, radial and ulnar loops, and arches. (See Plate 1.) As regards the general distribution of finger patterns Bonnevie remarks that they are very unlike with regard to their numerical appearance within the whole material. The whorls appear in a percentage of 25.65, loops in a percentage of 66.95, arches in a percentage of 7.4. With reference to a comparison of the right and left hands Bonnevie remarks, "the distribution of pattern-types between right and left hands also proves to be peculiar, the whorls being considerably more numerous on right hands

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[44] Kristine Bonnevie, Main results of a statistical investigation of finger-prints from 24, 518 individuals, *Eug., Genetics & Family*, 1, 1923 200 ff.; Studies on papillary patterns of human fingers, *J. Genetics*, 15, 1924, 1-109.

(29.38 per cent. against 21.92 per cent. on left hands), while arches (6.92 per cent. to 7.88 per cent.) and especially also loops (63.70 per cent. to 70.20 per cent.) are more richly represented on left hands. This difference is, within the loops, due to ulnar loops only (57.76 per cent. to 64.52 per cent.) while the occurrence of radial loops (5.94 per cent. to 5.68 per cent.) is practically the same on both sides."[45]

The paragraph quoted above refers to percentage of pattern-types for all fingers taken together. Some consideration should, however, be given to a comparison of the separate fingers. Comparing corresponding fingers on right and left hands, Bonnevie remarks the high percentage of whorls for digits I and IV, especially upon right hands. Loops and arches yield a higher percentage on left hands. Digit II gives an approximate statistical correspondence between right and left hands for all patterns, although radial loops and arches are slightly more numerous on the right hand. Digit III shows nothing of importance so far as comparison of the two hands is concerned. Digit V gives a very considerably surplus of whorls upon right hands as compared with left. Also radial loops are unequally distributed, being more frequent on the left than on the right. The above summary refers to total percentages for the different papillary patterns.[46]

Table XVI
Finger-Print Patterns
96 Left-handers
(Table prepared by Richard S. Uhrbrock)

		Percent of Fingers				
Fingers	Hands	Whorls	Loops			Arches
			Rad.	Uln.	Total	
All	Both	26.67	5.63	62.81	68.44	4.89
	R	30.00	6.04	59.58	65.62	4.38
	L	23.33	5.21	66.04	71.25	5.42
I	Both	36.46	0	60.94	60.94	2.60
	R	40.62	0	56.25	56.25	3.13
	L	32.29	0	65.63	65.63	2.08
II	Both	25.00	23.44	38.02	61.46	13.54
	R	25.00	26.04	36.46	62.50	12.50
	L	25.00	20.83	39.58	60.41	14.58
III	Both	15.10	2.61	77.08	79.69	5.21
	R	17.71	1.04	77.08	78.12	4.16
	L	12.50	4.17	77.08	81.25	6.25
IV	Both	45.83	1.04	52.09	53.13	1.04
	R	54.17	1.04	44.79	45.83	0
	L	37.50	1.04	59.38	60.42	2.08
V	Both	10.94	1.04	85.94	86.98	2.08
	R	12.50	2.08	83.34	85.42	2.08
	L	9.38	0	88.54	88.54	2.08

Table XVI gives a similar summary for 96 left-handed *S*s. The table was prepared by Dr. R. S. Uhrbrock who was well trained in handling finger-print technique. It will be noticed that in general the above comparison between right and left digits held true, but in three instances, there were reversals of percentages on right and left fingers as follows: (a) Digit I: arches; (b) Digit II: arches; (c) Digit V: radial loops.

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[45] *Op. cit.*, *Eug. Genetics & Family*, 1, 1923, 200.

[46] For discussion of another sort of bimanual distinction, see Harold Cummins and Charles Midlo, Palmar and plantar epidermal ridge configurations (dermatoglyphics) in European-Americans, *J. Amer. Phys. Anthrop.*, 9, 1926, 471-572.

Besides these rather striking reversals some changes in percentage of patterns occurred that seem worth noting. Whorls give about the same percentages, as Bonnevie's tables show, for all fingers of both hands and there are no reversals for right and left hands but there is an increase of about 4% of whorls on left Digit I (thumb), and an increase in percentage of whorls for the sum total of both hands on Digit IV, with a slight decrease for Digit II.

Radial loops show one reversal with respect to right and left on Digit V and also a striking increase in the percentage on left Digit III.

Ulnar loops show an increase of approximately 6 or 7% for the sum total of both fingers on Digits II, and III and slight decrease on IV.

Arches gave a decreased proportion on the whole for all fingers. Two reversals of percentages for right and left occurred, namely on Digits I and II.

Table XVII gives the distribution of patterns upon the different fingers in percentage of the type. Bonnevie's corresponding percentages are given in parentheses for totals in order to show that there is, in general, close agreement in spite of the great difference in number of fingers utilized in the two investigations, that is, totals for right- and left-handed yield similar results but with a few differences which may or may not be significant.[47]

To specify, on Digit I, whorls, ulnar loops and arches give approximately the same percentages in the two investigations, but radial loops run slightly higher in Bonnevie's tabulation. On Digit II whorls are reduced in percentage and ulnar loops and arches are increased for the left-handed. On Digit III the percentage of arches is decreased. On Digit IV the percentage of whorls and radial loops is increased, and the percentages of ulnar loops and arches are decreased. On Digit V the percentage of radial loops is decreased and of arches increased.

In specific comparison of correspondences for right and left fingers, the following reversals of percentages occurred: (a) Digit I: arches; (b) Digit II: loops (total) and arches; (c) Digit V: radial loops.

In commenting upon her statistical material Bonnevie asks the reason for the great frequency of whorls upon Digits I and IV, as well as the striking surplus of their percentage upon the right hands, a feature which is visible upon all fingers but especially upon Digit I. She also calls attention to the strange difference between right and left hands with regard to radial loops on Digit V, their value on the left side being more than 18 times that on the right. My own tabulation shows a slight increase of percentage of whorls on the left hand as compared with the right and also in the case of Digit V, a reversal in the percentage of radial loops on right and left hands, which is most interesting in the light of Bonnevie's finding.

In general, it will be seen that there is sufficient evidence of shifts in relative percentages for the right and the left hand to warrant a further and more comprehensive investigation. It is necessary to have a much greater number of left-handed subjects (pathological cases being sifted out if possible), to separate the sexes (since differences in handedness occur), and to keep constant the dominant eye. Possibly, also, tabulations should be made keeping constant the dextrality type and even the

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[47] *Op. cit.*, *J. Genetics*, 15, 1924, 12.

TABLE XVII

Finger Print Patterns
96 Left-handers
(Table prepared by Richard S. Uhrbrock)

Pattern Type		All Fingers		Digits									
				I		II		III		IV		V	
		R	L	R	L	R	L	R	L	R	L	R	L
Whorls	No.	144	112	39	31	24	24	17	12	52	36	12	8
	%	56.25	43.75	15.23	12.11	9.38	9.38	6.65	4.69	20.31	14.06	4.69	3.51
	*%	100		27.34†	(27.32)	18.76	(22.53)	11.33	(12.64)	34.37	(28.93)	8.20	(8.58)
Loops (Radial)	No.	29	25	0	0	25	20	1	4	1	1	2	0
	%	53.70	46.30	0.0	0.0	46.30	37.04	1.85	7.41	1.85	1.85	3.70	0.0
	*%	100		0.0	(1.15)	83.34	(82.57)	9.26	(7.96)	3.70	(2.70)	3.70	(5.62)
Loops (Ulnar)	No.	286	317	54	63	35	38	74	74	43	57	80	85
	%	47.43	52.57	8.96	10.45	5.80	6.30	12.27	12.27	7.18	9.45	13.27	14.10
	*%	100		19.41	(19.86)	12.10	(10.03)	24.54	(23.05)	16.58	(19.20)	27.37	(27.86)
Loops (Total)	No.	315	342	54	63	60	58	75	78	44	58	82	85
	%	47.95	52.05	8.22	9.59	9.13	8.83	11.42	11.87	6.70	8.83	12.48	12.93
	*%	100		17.81	(18.24)	17.96	(16.33)	23.29	(21.73)	15.53	(17.77)	25.51	(25.93)
Arches	No.	21	26	3	2	12	14	4	6	0	2	2	2
	%	44.68	55.32	6.38	4.26	25.53	29.78	8.51	12.76	0	4.26	4.26	4.26
	*%	100		10.64	(10.58)	55.31	(44.50)	21.27	(29.81)	4.26	(9.23)	8.53	(5.88)

*Percentages both right and left combined.
†Bonnevie 's corresponding percentages are given in parentheses.

dominant thumb. If significant reversals of percentages should be obtained from the use of large and definitely selected groups the results would indicate that handedness is linked up with structural items in such a way as to warrant the conclusion that it has its basis in inherited rather than acquired characters.

One other approach will be illustrated in this study but merely as an example of the way in which finger-print material might be used. My groups are too small to cite in any conclusive way. It was thought that it might be possible to obtain an index of degree of symmetry for the right and left hands for different dextrality types. It seemed possible that the degree of symmetry in RLL and LRR cases might be greater than that found in RRR and LLL cases. Bonnevie has a table summarizing for 245,180 fingers the percentages of correspondence between fingers of the right and left-hands both for totals and for specific patterns.[48] I have attempted to make comparisons for my material for the totals for each finger. See Table XVIII. In the first column I give Bonnevie's total percentages and then in succession percentages (1) for my totals (1890 fingers), (2) for the right-handed (830 fingers), (3) for the left-handed (1040 fingers), (4) for combined RRR (Right Eyed) and LLL (Left Eyed) (420 fingers), (5) for combined RLL and LRR (680 fingers). Both sexes were used in getting the data for this table.

TABLE XVIII

Percentage Correspondence Between Fingers of Right and Left Hands

Digits	% Bonnevie	Total 1890 Fingers	Right-Handed 830 Fingers	Left-Handed 1040 Fingers	RRR and LLL 420	RLL and LRR 680
I	74.05	76.71	77.10	75.96	71.42	76.47
II	51.28	55.55	54.21	56.73	57.14	55.88
III	73.83	75.13	72.28	76.92	69.04	70.58
IV	71.91	78.30	78.31	77.88	76.19	82.35
V	87.61	84.65	85.54	83.65	76.19	85.29

The results show that for Digits I, IV, and V there is a greater degree of correspondence for RLL and LRR, or symmetrical, types than for RRR and LLL, or unilateralized types, but that on Digit II, there is a slight reversal of percentage, and on Digit III no evidence of difference. As compared with Bonnevie's figures, Digit II shows a greater degree of correspondence than she obtained. On the whole, there is some evidence in confirmation of one's expectations as regards varying degree of symmetry in contrasting types.

One further attempt was made to compare contrasting types RRR, LLL with RLL and LRR. Ridge counts were obtained for all patterns on all 10 fingers of each member of the two groups. In order to use together loops and whorls, the double ridge count was made for the whorl and the sum divided by 2; arches were counted as zero. This is the method of summation suggested by Bonnevie.[49] A difference between the ridge count on corresponding right and left fingers was then obtained, the sum of the differences totaled for the two groups, and a mean obtained by dividing by the number of fingers in each group. The mean differences for the RRR and the LLL groups was 2.579, and for the RLL and LRR groups, 1.781. It will be noticed that this difference in symmetry is again in favor of the RLL and LRR group and again confirms one's expectations.

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[48] *Op. cit.*, *J. Genetics*, 15, 1924, 14.

[49] She says, "As a basis of classification has been used the method of ridge-counting, previously applied to loops only. This method has here been developed for application to other patterns in the way now to be described." "The quantitative value of any pattern is expressed in two figures based upon the number of ridges between each of the two triradii and the center of the pattern (the centre of a typical whorl, the core of a loop, the two cores of a double loop). If there exists only one tri-radius (loops) the figure of the other side is 0; if no tri-radius exists (simple arch) the figures of the pattern are 0—0" (*ibid.*, 47).

It occurred to me that it might be interesting to test out the possibility of the dominant thumb, or the thumb outside when the hands are clasped with the fingers interlacing, being related to finger-patterns. Accordingly I obtained the percentages of whorls and of loops and arches for the right thumbs of those individuals who placed the right thumb out when the hands are clasped, and also the percentages for the left thumbs in this same group. I obtained also the percentages of whorls and loops and arches for the left thumbs of those individuals who placed the left thumb out when clasping the hands and the percentages for the right thumbs of this left-thumbed group. For results see Table XIX.

TABLE XIX

	Right Thumb out (126 cases)						Left Thumb out (127 cases)					
	Whorls		Loops		Arches		Whorls		Loops		Arches	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
R Thumb	56	44.4	69	54.7	1	00.8	48	37.8	77	60.6	2	1.6
L Thumb	39	30.9	86	68.2	1	00.8	38	29.9	82	64.6	7	5.5

I should be unwilling to attempt to draw any conclusions from this tabulation since the groups are small, since more than a third of the *S*s were left-handed, and since no differentiation was made on the basis of sex. The figures suggest, however, the sort of observation that should be made on more satisfactory material in the hopes of finding significant differences. For example, Bonnevie finds 41.60% of whorls on right thumbs (245,180 fingers), whereas the above tabulation gives 44.4%, the right thumb being outside when the fingers are clasped, and 37.8% when the left thumb is out, a shift which may be worth following up. Nothing similar appears in the percentages for right and left thumbs for left-thumbed people. Bonnevie gives 28.42% of whorls on the left thumb. The present investigation gives 29.9% when the left thumb is outside and 30.9% when the right thumb is outside. In the case of the left-handed one gets an increased percentage of whorls on the left thumb as was previously mentioned. The fact that so many of the group used in the above tabulation were left-handed makes an interpretation of results difficult.

The tabulation suggests that a study should be made of the percentages of arches on right and left thumbs of right- and left-thumbed individuals respectively. The present tabulation suggests an increase in percentage of arches on the left thumb of left-thumbed people.

(10) *Handedness types and intelligence.* The foregoing discussions have given us some reason to believe that handedness types, *i.e.* variations in unidextrality called types for convenience, do exist and have possibly a structural foundation. The next question concerns their further significance. Is the degree of unilaterality of function indicative only of greater motor speed and skill on the part of the more highly unilateralized individual or has it far-reaching implications with reference to the general

constitution of the individual? It is quite conceivable that the latter is the case. Symmetry or asymmetry of structure and function may be related to certain as yet obscure but fundamental factors in development.

It is possible that we must look to such investigations as Newman's on the biology and physiology of twinning for an ultimate explanation of handedness.[50] Such studies imply that bilaterality of structure is a form of twinning. Newman suggests that the prevalent belief that one twin is physically superior to the other may have some foundation in fact. As a consequence of the twinning relation one twin has a tendency to gain a physiological ascendency over the other. Unilaterality of function might arise similarly in man. The preferred half is the 'superior twin.' One would anticipate, however, all degrees of difference between the two body-halves, just as one finds all degrees of difference between completely developed twins.

Since the goal of the biological processes appears to be the development of a bilaterally symmetrical individual, it is natural to conceive *a priori* the probability of the RLL or LRR individual being the physiological type in which the developmental process has been carried through most adequately. RRR and LLL individuals suggest a high degree of lateralization of function which if too extreme might result in psychic imbalance.[51] RRL and LLR suggest a mixed functioning which might be tied up with nervous instability.

It is quite possible that the degree of unilateralization of function may exert a supreme influence not only on the patterning of behavior but also in the patterning of psychic reactions.

At the present stage of the investigation we are limited largely to speculation with some curious differences of opinion concerning the relative value of ambidextrality and unidextrality. Many believe that the ambidextrous are less mentally gifted than the unidextrous, and that lateralization of brain function is essential to higher mental development. On the other hand, the advocates of ambidextral training believe that increased cerebral capacity accompanies training of the two hands. Kerr described dextrality as "the specialized aptitude associated with the psychical phenomena of speech and reasoning which is so marked in the left brain," and recognized three classes as follows. (1) The highest development of both hands in true ambidexterity, accompanied by high intellectual and expressive attainments. (2) One hand well developed in function, the other mainly accessory, the condition of the immense majority with whom right- or left-handedness is an accident and not expected to affect mentality. (3) Defective functioning of both hands, really an ambisinistrality which includes many of the so-called left-handed.[52] Many individuals who have not developed properly mentally and who show many degenerative stigmata such as speech defects are said to be 'ambisinistral.' Ioteyko urges attainment of ambidextrality not only to establish a complete equality of the two sides of the body but to correct such inequalities as grow out of a predominately unilateral training, since the asymmetry extends to the psychic centers.[53]

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[50] H. H. Newman, *The Biology of Twins*, 1917, 1-179; *The Physiology of Twinning*, 1923. 1-219.

[51] In this connection A. Gesell's Hemihypertrophy and mental defect (*Arch. Neur. & Psychiat.*, 6, 1921, 400-421) is most interesting. Gesell says, "the facts clearly indicate that hemihypertrophy should be added to the list of developmental anomalies which bear some lawful relation to the incidence of mental deficiency."

[52] J. Kerr, Left-handedness and mirrored writing, *School Hygiene*, 11, 1920, 39-44, 74-82

[53] J. Ioteyko, L'usage de la main gauche chez les mutilés, *Rev. scient.*, 1916, 494-499.

Whatever difference of opinion may exist about ambidextrality it is a common belief that left-handedness is a sign of mental deviation of some sort. This belief is based largely on the very high percentage of left-handers found among the delinquent and feebleminded. The following figures are taken from Gordon's extensive and careful investigation:[54]

% in ordinary schools 7.3 (3,298 children from 4-14 years).
% in mental defective schools 18.2 (4,620 children from 8-16 years).

The percentage of left handedness among idiots and epileptics is estimated to be from 16-30%.

Although in ordinary schools the percentage of left-handed boys is greater than the percentage of left-handed girls, Gordon found that in schools for the mental defectives the percentage of left-handed girls was about 25% higher than that of boys.

Gordon also reports that while among normal children the left-handed are frequently the most efficient and capable, among mental defectives it is exactly the reverse, left-handedness being often associated with marked deficiency. He adds, "spontaneous change of left-handedness to right-handedness in writing among mental defectives is often found to be associated with progress in school work and in intelligence."

On the basis of the above facts Gordon framed the following tentative hypothesis. "There seems therefore to be good reason to believe that there are certainly two distinct types of left-handedness. (1) Those who are naturally left-handed, *i.e.* those who have a dominant right hemisphere. (2) Those who are naturally right-handed, but who have been driven to the use of the left hand in some of their activities, owing to some defect of the nervous or muscular system."

The left-handedness prevalent among mental defectives is according to Gordon's hypothesis largely of the pathological type. The left hemisphere is affected and this disturbance not only interferes with the proper functioning of the right hand but also with the functioning of many of the higher intellectual centers.

Gordon's percentages are based upon unimanual activities. The present investigation has insisted upon the need of study of the bimanual performances also. It has, however, presented evidence that indicates that the type of left-handedness most prevalent among feeble-minded males (LLR) is not the same as that found among American psychologists (LRR). An extensive survey should be made of delinquent and feeble-minded groups to determine whether or not the indications reported in the present paper are confirmed. If so, the facts should then be reviewed in the light of Gordon's hypothesis.

Some experimental work has been done on the mentality of the left-handed. Ballard who was particularly interested in the lingual ability of dextro-sinistrals or those sinistral children who had been forced to use the right hand in writing, found that the results of his researches falsified his expectations that they would be below normal. He cites his results as indicating "that sinistrals, as a class, are in no way deficient in their general power of verbal expression; for although the pure sinistrals are better than the dextro-sinistrals, *both are better than the 'dextrals'* ".[55] Because of the small numbers involved Ballard does not press this inference, but he does conclude that "the phenomena of dextrality (in its broader sense) are manifested in the motor rather than in sensory bilateral asymmetry."

Gordon in an investigation of 1,019 dependent children concluded that the left-handed were not at all inferior to the right-handed in mental level.[56]

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[54] Hugh Gordon, Left-handedness and mirror writing, especially among defective children, *Brain*, 43, 1920, 312-368.

[55] *Op. cit.*, 30, italics mine.

[56] Kate Gordon, Some notes on the mental status of the left-handed, *J. Delinq.*, 8, 1923, 154-157.

I have Thorndike scores on 43 left-handed college students. Nearly 25% of this group have a score in the 80's and 90's, an exceedingly fine showing. It may be of interest to note that only one of this group is LLR in type.

The problem of the relation of intelligence and handedness involves, however, much more than the question of the mental deviation of the left-handed. From my point of view it centers in the determination of the significance of the different dextrality types. One may group together the formulae RRR and LLL, RLL and LRR, RRL and LLR and ask whether there are any facts that seem to point to mental differences between these three groups. The problem is complicated by the undoubted existence of sex differences in handedness. The material to be presented must be studied in the light of such differences.

TABLE XX

Handedness and Intelligence
115 Men
(Mean Thorndike 66.32—1919-25 Series)

Formula	Total	Above Mean		Below Mean	
	No.	No.	%	No.	%
RRR & LLL	37	26	70.27	11	29.73
RLL & LRR	35	22	62.86	13	37.14
RRL & LLR	43	22	51.16	21	48.84
Total	115	70	60.86	45	39.13

TABLE XXI

Handedness and Intelligence
201 Women
(Mean Thorndike 62.51—1919-25 Series)

Formula	Total	Above Mean		Below Mean	
	No.	No.	%	No.	%
RRR & LLL	69	34	49.27	35	50.72
RLL & LRR	20	13	65.00	7	35.00
RRL & LLR	112	68	60.71	44	39.28
Total	201	115	57.21	86	42.78

The statistical outcome of Section 3 indicated that superior and inferior groups of men give significant differences as regards the percentages falling under certain handedness types. Significant sex differences for certain formulae were also indicated. In order to carry this investigation further a tabulation was made of the handedness types of 201 college women and 115 college men in connection with their Thorndike scores. They were entered as above or below the mean Thorndike as calculated separately for 579 Freshmen women for the series 1919—1925, and for 571 men.

Tables XX and XXI give the tabulation. I refrain from drawing any conclusions from them since the figures show that some unknown factor of selection operated to give me more handedness records from individuals above the mean for intelligence than below it. The tables do suggest, however, the possibility of certain significant percentage differences existing and indicate the same trend of evidence as that exhibited in Section 3, namely, the existence of some relationship in males between superiority and either high unilaterality of function (RRR-LLL) or symmetry of function (RLL-LRR), a superiority not evident in the RRL (LLR) or mixed group.[57]

The case with the women is different, except for the RLL (LRR) formula where the results are comparable to those obtained from the men. The point of difference is this: that the RRL (LLR) women make a better showing on an intelligence test than the RRR (LLL) type. The interpretation of the sex difference revealed in the above tables remains obscure. No doubt the problem is one that the physiologist rather than the psychologist must solve.

To check further the relationship existing in the women between asymmetry and intelligence the mean Thorndike was calculated for two contrasting formulae, keeping the eye constant, with the following results:

RRR–Right-eyed = 67.57 (29 cases).
RRL$_3$–Left-eyed = 74.20 (22 cases).

Calculation was made for the same two formulae, keeping the thumb also constant, with the following results:

RRR–Right-eyed–Right-thumbed = 65.30 ± 9.39 (16 cases).
RRL$_3$–Left-eyed–Left-thumbed = 76.47 ± 11.81 (10 cases).

One would not, of course, care to draw conclusions from such small groups but since the trend of evidence is all in the same direction it remains for future workers to investigate further the possible relationship between intelligence, probably of the verbal and abstract variety, and asymmetry or mixed type of handedness.

It has long been my belief that differences in handedness are bound up with perceptual and ideational predispositions and that sex differences are also found within the same field. The particular correlation in mind is that of a possible relationship existing between extreme unidextrality and visual or spatial predispositions, and between mixed, or asymmetrical functioning and linguistic interests.[58] With reference to the latter point some further data were obtained. Four vocabulary tests (namely, Starch 1 and 2, Kirkpatrick, and Thurstone) were given to 100 girls. Percentile distributions of scores were made for each vocabulary test and then an average percentile rank obtained for each *S*. An average was then obtained for each of three handedness groups as follows:[59]

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[57] In this connection I may cite the handedness formulae of 18 psychologists who have been presidents of the American Psychological Association: RRR, 10; RLL or LRR, 5; RRL, 2 (one is possibly RLL); RRR or RLL, 1. The group is highly lateralized in function or ambidextral in the sense of an opposed unimanual and bimanual preference.

[58] I may say that while the author was working on the above hypothesis, a letter was received from Professor Yerkes which contained the following paragraph: "In connection with your studies of structural and functional asymmetry, I wonder if you have thought of trying to measure, for purposes of correlation, linguistic ability and related memory functions. I have long suspected that facility in learning a language is intimately related to asymmetry of motor functions. There certainly is amazing differences among individuals in linguistic ability as well as in the traits which you have been studying by means of the Will-Temperament Tests and your tests of asymmetry."

RRR–Right-eyed = 48.70 (17 cases),
RRL–Right-eyed = 51.33 (28 cases),
RRL–Left-eyed = 59.14 (24 cases).

It will be noticed that the last group in which there is asymmetry between hand and eye as well as a mixed dextrality type is distinctly the superior group. In the next section it will be shown that this combination of right-handedness and left-eyedness is a great *disadvantage in an orientation test.*

TABLE XXII

High in Vocabulary Test

S	Thorndike	Formula	Rank in Vocabulary	Rank in Hand Test
S	99.9	RRL$_1$ L-E*	1	34
T	79.3	RRL$_3$ L-E	2	69.5
Sh	82.5	RRL$_3$ L-E	3	69.5
Co	111.0	RRL$_1$ R-E	4	24.5
Cr	71.6	RLL L-E	5	45
U	†	RRL$_3$ E-E	6	73
Sn	77.9	RRL$_1$ R-E	7	60
H	91.3	RRR R-E	8	60

Low in Vocabulary Test

S	Thorndike	Formula	Rank in Vocabulary	Rank in Hand Test
Hi	44.6.8	RRR L-E	70	34
Ho	71.8	RRR R-E	71	49
R	63.7	RRL$_1$ R-E	72	60
M	31.6	RRL$_1$ L-E	73	15
F	63.5	RRR R-E	74	66
V	44.8	RRL$_1$ R-E	75	15
Va	50.8	RRL$_1$ L-E	76	30.5
Ch	59.7	RRL$_3$ R-E	77	39.5

*L-E = Left-eyed; R-E = Right-eyed; E-E = Either-eyed.
†No record, but undoubtedly of superior intelligence.

Some further evidence was obtained through giving to the same group of 100 girls the psychological examination published by the American Council on Education and obtaining a percentile distribution for such tests as those entitled 'Artificial language,' 'Reading' and 'Opposites,' etc. The sub-groups are small but possibly the following facts are significant: In the test on 'Opposites' only 1 RRR–Right-eyed individual appears in the highest 10%, while there are 7 RRL–Left-eyed individuals. In the test on 'Artificial Language' there is only 1 RRR–Right-eyed individual in the highest 10%, and 6 RRL–Left-eyed. In the reading test there are 2 RRR–Right-eyed in the highest decile, and 5 RRL–Left-eyed.

It would be most interesting if one had for comparison with the records above, the records of the same groups of girls on a mechanical aptitude test, but I do not have such material.

But in contrast to the result on the verbal tests I may cite those obtained from Thurstone's 'Hand test' which is described as a spatial relations test and consists of 49 pictures of the right and the left hand in different positions. "The problem to be solved is whether the figure is that of the right or left hand." The rank correlation with the results of the four vocabulary tests is practically zero, actually slightly negative (−.12) for 77 college girls. A tabulation of the rank standing in the two tests for the 8 *S*s who were respectively highest and lowest in the vocabulary tests is instructive (see Table XXII), for it seems to indicate that those who rank highest in the vocabulary tests (all have high Thorndike scores) are at some special disadvantage in the 'Thurstone Hand-test' although those who rank low in the former test are not necessarily high in the latter.

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[59] The group RLL was too small to use.

The special ability tapped by the 'Thurstone Hand-test' is not clear to me. I could find no evidence that success in it was dependent upon degree of unidextrality. It can be manipulated in two different ways, visually or kinesthetically, but does not, apparently, lend itself to verbal or symbolic manipulation. In this latter respect it differs from the 'Orientation Test,' a second spatial relationship test which will be reported in the next section. A rank correlation for 31 *S*s for the hand and orientation test gave a coefficient of +.09—practically a zero relationship.

Formula	% Reporting Confusion	
	R-eyed	L-eyed.
RRR	28.9	51.9
RRL	35.5	58.3
RLL	23.3	50.0

(11) *Orientation and eyedness.* In tabulating the results of my questionary investigation of the handedness of psychologists relative to the percentage of each dextrality type reporting a tendency to confuse right and left, I discovered a significant difference in the amount of spatial confusion reported by right-handed respondents who were right-eyed and those who were left-eyed.

I quote below, from my article "Distinguishing Right from Left,"[60] a summary of the returns from 257 questionaries received from right-handed individuals—67 checked questionaries from students were added to the number sent in by psychologists.[61]

Right-handed respondents who were left-eyed reported a greater amount of confusion than those who were right-eyed. LLL respondents, on the other hand, reported more spatial confusion when right-eyed than when left-eyed, a result in harmony with that obtained from right-handed and left-eyed respondents. It appeared from the questionary returns that a lack of agreement between hand and eye served to mediate confusion in distinguishing between right and left.

This discovery appeared to me so significant that I endeavored to confirm it by the use of experimental material. In order to do this it was necessary to devise a group test for orientation.

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[60] J. E. Downey, *Ped. Sem.*, 33, 1926, 153-166.

[61] For totals on which this summary is based and application of the formula to determine significant differences see *ibid.*, 164.

Adopting and extending a suggestion made by Terman and Chamberlain[62] of a direction orientation test for children, I devised one containing 30 different items. Below is a sample sheet of directions for the test, which is given orally. The testees number their answers to correspond to the questions.

ORIENTATION TEST C

Suppose you are in a city where the streets run north and south, and east and west, then

(1) Suppose you are going north, then you turn to your right, what direction are you going now? (5 sec.)
(2) Going north, then L, then R. (6 sec.)
(3) Going west, then R, then R again? (6 sec.)
(4) Going south, then L, then R again? (6 sec.)
(5) Going north, then R, then R again, then L? (7 sec.)
(6) Going south, then L, then R, then L? (7 sec.)
(7) West, then R, then R again, then R again? (7 sec.)
(8) South, then L, then L again, then L again? (7 sec.)
(9) West, then R, then R again, then R again, then L? (9 sec.)
(10) South, then L, then L again, then R, then L? (9 sec.)
(11) East, then R, then L, then R, then L? (9 sec.)
(12) Which way would you have to face so your left hand would be toward the east? (5 sec.)
(13) Which way would you have to face so your left hand would be toward the north-east? (6 sec.)
(14) Which way would you have to face so your right hand would be toward the north-west? (6 sec.)
(15) Which way would you have to face so your left hand would be toward the south-west? (6 sec.)

Suppose you are in a city where the streets run north-east, south-west, and north-west, south-east, then

(16) Suppose you are going north-east, then you turn to your right, what direction are you going now? (5 sec.)
(17) Going south-east, then turn to the left? (5 sec.)
(18) Going north-west, then turn to the right? (5 sec.)
(19) Going south-west, then R, then R again? (6 sec.)
(20) Going north-east, then R, then R again, then L? (7 sec.)
(21) Going south-west, then L, then R, then L? (7 sec.)
(22) Going north-west, then R, then R again, then R again? (7 sec.)
(23) Going south-east, then L, then L again, then L again? (7 sec.)
(24) Going north-west, then R, then R again, then R again, then L? (9 sec.)
(25) Going south-west, then L, then L again, then R, then L? (9 sec.)
(26) Going south-west, then R, then R again, then R again, then R again? (9 sec.)
(27) Which way would you have to face so your left hand would be toward the south-west? (6 sec.)
(28) Which way would you have to face so your right hand would be toward the south-east? (6 sec.)
(29) Which way would you have to face so your left hand would be toward the north-west? (6 sec.)
(30) Which way would you have to face so your right hand would be toward the north-east? (6 sec.)

Write an account of how you carry out the directions.

Are you confused in distinguishing right from left?

Do you have difficulty in making this distinction in such situations as are involved in military drill, waiting on table, driving a car?

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[62] L. M. Terman and M. B. Chamberlain, Twenty-three serial tests of intelligence and their intercorrelations, *J. Appl. Psychol.*, 2, 1918, 341-354.

This test as given above was the outcome of considerable preliminary investigation since it was necessary to choose items in such a way as to get a fairly normal distribution of scores. A percentile distribution was made for the scores of 146 unselected women. The median score was 18.8; (Q_3, 24.9; Q_1, 13.16). Seventy-one men gave a much higher median, 24.21.[63]

In order to determine the reliability of the test it was given a second time to 89 women. The coefficient of correlation between the first and second giving of the test is +.78 (p.e. =.044). In order to determine the degree to which success in the orientation test was determined by intelligence the Thorndike scores of 47 women were correlated with the scores on the orientation test. The correlation is low, namely +.124 (p.e. = .096). Differences in general intelligence as measured at present do not greatly influence the scores on the test although no doubt the acquiring of a technique for handling it would serve in a practice series to raise scores for the more intelligent subjects. There are, however, individuals of superior intelligence who are completely baffled by the test and low grade subjects who handle it with perfect ease.

Introspective reports on the way in which the test is manipulated are very interesting and valuable but will not be included in this paper. There are also interesting indications of particular difficulties encountered in certain items of the test, but, again, this material will not be canvassed in this article.

Table XXIII gives a summary of the results for 157 girls so as to show how ease or difficulty in orientation is related to agreement or disagreement between dominant eye and hand. Percentages were calculated for the right-eyed, left-eyed, and either-eyed who are above and below the median and also for those above the 75 percentile rank (Q_3) or below the 25 percentile (Q_1) as determined by the distribution of the scores of the 146 unselected cases mentioned above. Some special effort was made to increase the number of left-eyed individuals in the experimental group. The table confirms the conclusion drawn from the questionary returns received from a totally different group that there is more apt to be confusion between right and left when the dominant hand and eye are not on the same side than when they are.

It is possible that the results might be even more striking if the test was carried out on a younger group since shifts in the originally sighting eye appear to occur frequently in adults on account of visual difficulties. A group of 31 right-handed high school girls (included in the above tabulation) gave the following records in the orientation test.

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[63] The test in its present form is not sufficiently difficult for college men. It may be men have acquired through experience a technique for handling spatial relations that most women lack, or that more men than women are symmetrical in type as, in fact, my data suggest.

	Right-eyed		Left-eyed	
	No.	%	No.	%
Above Median	18	81.81	3*	33.33
Below Median	4	18.18	6	66.66

*One *S* had defective vision in the right eye.

TABLE XXIII

Orientation Test and Eyedness
157 Girls
Median Score of unselected group = 18.8
$Q_3 = 24.9$; $Q_1 = 13.16$

	Right Eyed		Left Eyed		Either Eyed	
	No.	%	No.	%	No.	%
Above Median	54*	62.79	20	37.74	6	33.33
Below Median	32	37.21	33†	62.26	12	66.67
Total	86	100.00	53	100.00	18	100.00
Above Q_3	34*	61.82	11	33.33	4	40.00
Below Q_1	21	38.18	22	66.67	6	60.00
Total	55	100.00	33	100.00	10	100.00

*One left-eyed, left-handed *S* included.
†One right-eyed, left-handed *S* included.

Mirror-writing and mirror-reading are unquestionably tied up both with the general problem of spatial orientation and with the particular problems that center in types of dextrality. I have at hand some relevant material bearing on these two activities but must reserve it for a later report.[64]

(12) *Temperament and handedness.* As another lead I sought to determine possible relationships between temperament and handedness. In the original questionary on dextrality sent out to psychologists I asked for a self-classification into introvert and extrovert.[65] The concepts are not unambiguous and are besides open to other criticisms. Many respondents refused to classify themselves. Others describe themselves as balanced or mixed in type ('ambiverts'). There were 20 women and 55 men who classified themselves as extroverts, and 18 women and 76 men who classified themselves as introverts When these classifications were studied in the light of the dextrality records, it was found that there was some evidence of a relationship in certain respects.

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[64] S. T. Orton in a report on Word blindness in school children, (*Arch. Neur. & Psychiat.*, 14, 1925, 1-35), states that interference with the *visual* associative function results from a unilateral lesion and interprets this to mean that in the case of this function there is no fusion of right and left hemispheral activities. "For some children," he writes, "with the first efforts at learning to read the external visual stimuli irradiate equally into the associative cortices of both hemispheres, and are there recorded in both dextrad and sinistrad orientation." To such a cause he attributes disability in differentiating 'p' from 'q' and 'b' from 'd.' Again, "those children who are neither dominantly right-handed or left-handed, or in whom clear dominance has not been well established before they begin to learn to read, probably have more trouble with reversals of letters (30). Mirror-writing and mirror-reading—to be discussed in a separate report—are tied up with the general problem of spatial orientation and dextrality types.

[65] J. E. Downey, How the psychologist reacts to the distinction 'Extrovert-Introvert,' *J. Abn. Psychol. & Soc. Psychol.*, 20, 1926, 407-415.

The following summary is in point. There were 54 cases in which there was a lack of harmony between preferred hand and eye. Of these 54 cases, 46.3% describe themselves as introverts, 18.5% as extroverts, 20.4% as ambiverts, and 14.8% who refused to classify themselves. The results suggested a connection between eye-hand discrepancy and an introverted make-up. The classification into dextrality types, while it furnished no conclusive evidence, did give some indication that the introvert was more asymmetrical than the extrovert.

In my further study of introversion and extroversion I gave some 100 of the Colgate Mental Hygiene Tests, Personal Inventory C 2,[66] for introversion, to students whose handedness habits I had studied. Laird, who devised this test, furnishes a percentile ranking on the basis of which individual results may be scored. His percentile ranking, however, could not be used with my students since both men and women marked many more introvert characteristics than did his. I have been unable to find the number and the selection of the group on which Laird established his norms. He cites the reliability of the test form as +.85 but does not estimate its validity. My group was composed of students in psychology who had survived the first course in this subject and who were possibly more highly introverted than an unselected group.

Since I was unable to use Laird's norms I ranked my *S*s, men and women separately, in terms of the number of symptoms of introversion checked by each, and I obtained a mean score and the mean variation for contrasting groups. Several of these groups were too small to use, for example, the left-handed and RLL groups, and the men of all formulae. For the women there are strong indications that asymmetry characterizes the introvert. For example, the mean score of 16 RRR women who were also right-eyed was 16.81 (m.v. 3.36) and for 14 RRL women who were left-eyed 21.68 (m.v. 4.44). There were, however, RRL women who were left-eyed who scored low for introversion and a *few* RRR-Right-eyed ones who scored very high. Such *S*s should be carefully studied in order to determine the possibility of acquired eye defects or of a tampering with the native manual habits, but such a study I was unable to carry out. I am tabulating below the formulae of the 11 women who were found by the test to be the most introverted. All of them scored above Laird's ninetieth percentile.

8 RRL_{1-3}-Left-eyed
1 RRL_3-Right-eyed
1 RRR-Left-eyed
1 RRR-Right-eyed

The amount of asymmetry in this group is certainly striking, only 2 are right-eyed, 9 are left-eyed. With 8 of these *S*s I am well acquainted; 7 of them are certainly outstanding introverts, but I should hesitate classifying one (the RRR-Left-eyed *S*) as introverted. I am not acquainted with the one pure dextral *S*.

In contrast to this group are those 11 women with the lowest scores for introversion (below Laird's twenty-second percentile) grouped as follows: 4 RRR-Right-eyed; 1 RRR-Left-eyed; 5 RRL_3-Right-eyed; 1 RRL_3-Left-eyed.

There is less asymmetry in this group than in the former one, only 2 of the women are left-eyed, 9 are right-eyed.

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[66] It is interesting to compare these symptoms of introversion with the characteristics cited by the psychologists.

In an earlier section evidence was given that the college women most proficient in the verbal tests were asymmetrical in type, although the proposition must not be reversed. A similar outcome in the test for symptoms for introversion suggests the need of connecting the latter with the former.

In a recent article Bingham has sought to do this.[67] He writes, "it has often proved useful to differentiate people with reference to their relative interest in manipulation, motivating people, and dealing with symbols and ideas." Following Freyd, he finds an excess of introvert personalities among the mechanically inclined and of extroverts among the socially inclined.[68] He also believes that scholars "whose vocation it is to deal primarily with concepts exhibit tendencies toward introversion more frequently than toward extroversion." In discussing the reason for such connection between mechanical or intellectual interest and tendencies toward introversion of personality, Bingham advances the theory "that early introversion of personality leads to the development *through disproportional exercise* of one's native interests in mechanism or ideas, at the expense of interest and proficiency in social contacts."

In the light of the present study I find some difficulty in grouping together as introverts on the same level those interested in dealing with symbols and ideas and those interested in manipulating mechanisms, at least so far as the latter are naturally gifted. I myself should expect those who are mechanically gifted by nature to show a high degree of unilateralization of function in contrast to the asymmetry of the verbally gifted since skill in handling spatial relationships would appear to be a part factor in mechanical achievements. Highly unilateralized individuals are apparently less 'temperamental' than asymmetrical individuals. Quinan found more evidence of asymmetry in musicians than in machinists.[69]

The relationship of introversion to nervous and emotional instability is by no means clear. In a further attempt to clear up my findings I used Woodworth's Personal Data Sheet on one group of 54 women, and Laird's revision of this test, known as the Colgate Mental Hygiene Test, Personal Inventory B 3, with another group of 60 women and studied the outcome in relationship to the handedness formulae. I found, however, very little evidence that asymmetry of function was related to high scores in these tests.

(13) *Handedness and speech defects.* That sinistral tendencies point to the existence of psychopathic trends is as common a belief as that they are indicative of inferior mentality. In fact the two beliefs are different slants on the same problem. Quinan, in particular, has argued that sinistrality is indicative of congenital inferiority of the central nervous system. He uses the word "sinistrality" to designate "partial as opposed to established left-sidedness" and cities as the most significant symptom of this condition "the dominance of the left eye in binocular vision and a limited use of the left hand as the leading hand."[70]

I hoped to be able to make some contribution to this problem in the present study. I can, however, present only a few relevant facts and suggestions for further work. Such a study of individuals as is outlined in this paper should be extended to two groups of *S*s: (1) psychotic and psychopathic *S*s, and (2) speech cases. There is little material, so far as I am aware, on the dextrality habits of the first group. There is much material as regards the second. But dextrality has been too narrowly conceived in the discussion of speech disturbances and needs to be extended to cover eye habits as well as hand habits.

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[67] W. V. Bingham, Personality and vocation, *Brit. J. Psychol.*, 16, 1926, 354-362.

[68] The personalities of the socially and mechanically inclined, *Psychol. Monog.*, 33, 1924, no. 151.

[69] A study of asymmetry and muscle coördination in musicians, iron-workers, and others, *Arch. Neur. & Psychiat.*, 7, 1922, 360.

[70] See two articles previously cited.

In the questionary I sent out I sought information concerning speech difficulties and delay in learning to talk. I also obtained the handedness formulae of such speech cases as were available. The margin of error in the questionary returns is undoubtedly very great since obviously certain individuals list as speech disturbances hesitancy and mild emotional blockings which other respondents would overlook. The standard is a fluctuating one. I have, however, included in my tabulation only the serious cases that were reported, including stammering and stuttering, and I have made no use of the reports on delayed speech although this is a fruitful field of work. The reports are tabulated separately for the right and for the left-handed.

Questionary returns from 409 *S*s were utilized for the right-handed—208 from male psychologists, 93 from delinquent girls whose speech defects were reported by their teachers, 96 from college girls, and 12 from a miscellaneous group of speech cases. Altogether 57 cases of speech disturbances were available for distribution under the handedness formulae of right-handed persons as follows:

TABLE XXIV

Speech Disturbances and Handedness Types

Formula	Right-eyed	Left-eyed	Total	%
RRR	5	10	15	26.32
RRL	16	10	26	45.61
RLL	5	2	7	12.28
R R R L L	4	5	9	15.79
Total	30	27	57	100.00

From the tabulation it is evident that sinistral tendencies, either dominance of the left eye or a total or partial dominance of the left hand in bimanual operations characterizes all but a small percentage of the cases. Since, however, the majority of human beings have sinistral tendencies of one sort or another one cannot conclude that the latter predispose to speech disturbances but only that the pure dextrals are relatively immune. Only 5 RRR–Righted-eyed individuals report speech trouble.

It is interesting to note that 1 of the 5, although RRR in type, claims that he is not strongly right-handed and that 4 of the 5 are left-thumbed.

The percentages for each formula must be reviewed in the light of the percentage of unselected individuals classified under each formula. It appears that the number of speech cases is especially high for the RLL formula and for those individuals of unstabilized manual habits entered as $\begin{matrix} R\ R \\ R \\ L\ L \end{matrix}$.

In determining the percentage of speech disturbances for the right-handed it is necessary to use the unselected group of 390, who gave 45 cases. The calculation runs slightly over 11%.[71]

Let us turn to the speech cases of the left-handed. The questionary returns for them were probably much less accurate than for the right-handed since they were unchecked and received from a miscellaneous group. They err, I believe, in reporting too few cases. The total percentage of speech difficulties was 35 (20.5%) for 171 left-handers distributed as follows:

TABLE XXV

Speech Disturbances and Handedness Types

Formula	Left-eyed	Right-eyed	Total	%
LLL	15	3	18	51.43
LLR	6	5	11	31.43
LRR	1	5	6	17.14
Total	22	13	35	100.00

It appears from the above tabulation that the number of speech cases, 51.43%, contributed to the total by the LLL group, particularly the left-eyed members of this group, is large as compared with the percentage of LLL individuals in the total group of left-handers, about 28%. See Table II. The percentages of cases recorded for LLR and LRR individuals are low.

Such figures need to be confirmed by a more accurate procedure in which speech-diagnoses are made by specialists, and by a more extensive survey. Cases of pathological left-handedness should also be excluded. Taken at their face value they may be compared with the tabulation of the speech difficulties of the right-handed. The striking point of contrast is that while pure dextrals are relatively immune so far as speech disturbances are concerned, pure sinistrals are disproportionally subject to such troubles. Moreover, the RLL individual appears to suffer more frequently from a speech impairment than does the LRR individual.

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[71] Smiley Blanton (Speech defects in school children, *Ment. Hygiene*, 5, 1921, 820-827) reports that 18.13% of 1,400 University of Wisconsin Freshmen "were found to be unable to meet the necessities of English speech."

My own anticipation, confirmed by the tabulation for right-handed but not by that for the left, was that a high degree of unidextrality (pure dextrality and pure sinistrality) would be unfavorable to the development of speech defects, which would appear largely in the mixed types. The outcome is, however, in line with the belief that the greater the tendency to sinistrality the more likely there is to be speech trouble.

It is a common notion with reference to speech and handedness that tampering with the native manual preference induces speech trouble.[72] It is possible that such tampering may have a more disastrous effect on the LLL child than on the LRR or LLR one and that this would explain the high percentage of LLL speech cases in the tabulation just given. Further investigation should emphasize this possibility for it is a matter of considerable importance pedagogically if it should be found that children of the LLR or of the LRR formula may be trained to use the right hand with relative impunity.

I have been impressed in my study of individual cases by the ease with which many LRR individuals shift to the right hand in writing. Recently a teacher of drawing who uses both hands in her work told me that she had shifted from left to right-handed penmanship in the eighth grade without the slightest difficulty. She did this on the suggestion of an older brother who was also left-handed and who had shifted in the same way late in his school career. Her formula was LRR-Right-eyed. The young lady reported that her teacher of psychology informed her that sooner or later she would develop a speech disturbance, a prophecy which was never realized. The young lady reported also that she learns skilled movements with the left hand and then transfers them to the right.

Lack of stabilization of manual function appears to be a bad symptom, but the LRR or RLL individual should not be thought of as unstabilized. The formula for the latter is $\left\{\begin{matrix} \mathrm{L\ R} \\ \mathrm{R} \\ \mathrm{R\ L} \end{matrix}\right\}$. A common casual factor for both impaired speech and for unstabilized manual habits may be found in instability in the functioning of the nervous system. There is need at present of comprehensive studies of the development of linguistic and manual habits in young children.

A tabulation of the handwriting habits of the majority of my left-handed *S*s is given in Table XXVI. I wished to determine whether certain left-handed types yield to pressure more readily with regard to writing than others do. Of course the question can be answered only in a very general way since one has no measure of the amount of pressure exerted in any particular case. It is, moreover, probable that the pressure upon the younger members of the group was less than that put upon the older members since it is widely believed to-day that it is a mistake to insist that a left-handed child write with the right. One curious difference in the amount of shifting was found in comparison of the superior and inferior groups. Only three of the latter individuals, or 20%, wrote right-handed. But of the superior groups more than 42% wrote with the right hand. Two interpretations of this are possible. Either more effort is made by parents and teachers to change the superior child to writing with the right hand or else these individuals discipline themselves. Table XXVI gives the writing habit for 124 men and 86 women and also the total for the two sexes. There is no evidence of a significant difference with reference to the percentage of shift-overs for the different handedness formulae although possibly with larger numbers a significant difference might be found between the LLR and LRR groups since nearly 50% of the latter use the right hand in writing, and only 33.3% of the LLR group. It is possibly significant that the formula LLR gives a low percentage for impaired speech although it is, apparently, the one that shows least frequency in shifts to writing with the right hand.

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[72] L. M. Terman, *Hygiene of the School Child*, 1914, 1-418, chap XIX; also Ballard, *op. cit.*, 299 f. It is interesting to note that investigators of bilingual children report that they have not as unerring a sense of dextrality as the monoglots, which is supposed to be caused by the conflict initiated in learning a second language before they have acquired adequate power of expression in their native language. See D. J. Saer, The effect of bilingualism on intelligence, *Brit. J. Psychol.* 14, 1923, 25-38.

TABLE XXVI
Dextrality Types and Writing Hand

Formula	No.	Writes Right No.	Writes Right %	Writes Left No.	Writes Left %
	Women				
LLL	32	12	37.5	20	62.5
LLR	31	10	32.2	21	67.7
LRR	23	11	47.8	12	51.3
Total	86	33	38.4	53	61.6
	Men				
LLL	31	16	51.6	15	48.3
LLR	29	10	34.4	19	65.5
LRR	64	31	48.4	33	51.6
Total	124	57	45.9	67	54.0
	Combined Men and Women				
LLL	63	28	44.4	35	55.5
LLR	60	20	33.3	40	66.6
LRR	87	42	48.1	45	51.7
Total	210	90	42.8	120	57.1

SUMMARY AND CONCLUSIONS

The data reported in this paper, gathered from a survey of about 1500 individuals, show impressively the value of considering both unimanual and bimanual performances in the study of dextrality.

Specifically, the three types, RRR, RRL, and RLL, with the corresponding types for the left-handed, LLL, LLR, and LRR, have proved to be most illuminating descriptions of manual habits. The percentage distribution of these types has shown some significant percentage differences existing between superior and inferior men and between men and women.

Contrasting types are characterized by all manner of transitional forms, so that in experimental work a very large number of subjects would be necessary if one were to sift out particular sub-groups for adequate investigation of particular problems. The term 'type' is used as a convenient mode of description. Whether it would be possible to obtain a single curve of distribution running from the pure dextral at one end, through the mixed types, to the pure sinistral at the other, I do not know. Certainly I have no reason to think that the pure sinistral would in number equal the pure dextral. In no investigation should the possibility of sex differences be overlooked, nor the probable existence of pathological as opposed to a natural left-handedness.

The matter of a fixating eye was shown to be important, but it seemed from the present data impossible to believe that as a usual thing the dominant eye determined handedness, or even the reverse. The fact that one thumb rather than the other, is outside when the hands are clasped, with fingers interlocking, seemed to have some significance, since a dominant left thumb appeared to indicate latent sinistrality. Rife's theory that the spade foot is determined by the bimanual dextrality, was confirmed.

Evidence was presented that suggests the possibility of characterizing dextrality groups by structural differences. Differences in bone-measurements and in finger-print patterns for the two hands deserve thorough consideration.

It is also possible that certain motor and psychic differences are bound up with differences in degree of unidextrality.

For *women*, there is some evidence that the following motor and psychic characteristics are correlated with handedness types; the pure dextral appears to be motorly more expert than the mixed types, less subject to speech disturbance and less introverted. The mixed type, and especially the type with asymmetry of eye and hand, give a higher Thorndike score and a higher score on verbal tests, shows some special disability on the Thurstone and orientation tests, and is often highly introverted.

Comparable records have not been obtained for men, except in the case of the Thorndike scores, where in contrast to the results for women, the unilateralized and ambidextral male appeared to be superior to the mixed type.

SUGGESTED PROGRAM FOR FURTHER INVESTIGATION

The above conclusions are to be understood as tentative only. The main purpose of the present paper was exploratory, to suggest fields for more intensive experimental work, following the technique herein described. Many problems suggest themselves in a program for work. I am listing a few below, as I hope that the field now opened up may be worked extensively.

(1) The tests described or similar ones should be tried out on a large group of men. It is particularly desirable that the RLL group should be tested. On account of the limited number of women falling in this group, men must be used.

(2) Surveys of handedness types and the amount of asymmetry between eye and hand should be made for large groups of mental defectives and for psychopathic subjects. Dextrality types should also be determined for the congenitally blind.

(3) Studies of different racial groups should be made. I expect to publish a brief report on the handedness of small groups of North American Indians.

(4) Genetic studies are badly needed. Certain problems relative to the relationship existing between hand and eye might be solved by a determination as to which habit, the optical or the manual, is first stabilized. I am giving elsewhere a brief report on observations carried out on 49 preschool children. It is my impression that the manual habits are stabilized earlier than eye habits. The problem of determining the age for stabilization of each may be suggested to Directors of Nursery Schools as a valuable one to work on. The motor and temperamental traits of right-handed but left-eyed children should be determined and also the intelligence of the RLL (or LRR) child. I believe the latter will run high.

(5) Studies in the heredity of handedness need to be reviewed in the light of the handedness types described in this paper.

(6) Vocational applications of the present study remain to be worked out. I have been impressed by the number of skilled typists in my LRR group. I have failed to make records on this point for the RLL individual. I wish to cite in this connection the following recent statement by Book.

> "It also is evident from our results that an unusual amount of voluntary motor control in the left hand and arm is needed to attain the highest degree of success in typing. This is indicated by the fact that such superior control in the left hand and arm is present in all individuals that have achieved the greatest success in typing and in learning to typewrite, and by the fact that the mean variation for the left hand movements is less for all these contestants than it is for the right."[73]

A determination of the handedness types of champion typists would be of extreme value in the light of the facts revealed by Book's investigation. A similar study should be made on successful telegraphers. Again, I believe, RLL and LRR men may have an advantage in such work.

The many problems growing out of such a survey as the one reported in the present paper suggest the need of coöperation among investigators.

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[73] W. F. Book, Voluntary motor ability of the world's champion typists, *J. Appl. Psychol.*, 8, 1924, 283-308.

PSYCHOPHYSICAL ANALYSIS

By L. L. THURSTONE, University of Chicago

The purpose of this paper is to present a new point of view in psychophysics and to trace some of its implications. In the determination of a difference limen, the psychophysical judgment, no matter which of the classical methods is followed, is traditionally considered to be a function of *two* factors, namely, (1) the separation or difference between the two physical stimulus magnitudes, and (2) a discriminatory power measured in terms of sense-distances or just noticeable differences. The psychological continuum, no matter what it may be called, is supposedly determined by these just noticeable differences or equal appearing intervals, which are by definition assumed to be equal. The stimulus magnitudes are laid out on this continuum as landmarks, and the psychological separation between them is stated in terms of just noticeable differences or equal appearing intervals.

It will lead to a rather more flexible and illuminating analysis if we start out a little differently. I shall suppose that every psychophysical judgment is mainly conditioned by four factors, namely the two stimulus magnitudes or the separation between them, the dispersion or variability of the process which identifies the standard stimulus, and the dispersion or variability of the process which identifies the variable stimulus. The present analysis will concern these variables and finally the experimental procedures by which they may be isolated.

At the outset it may be well to make clear some things that will *not* be assumed. I shall not assume that the process by which an organism differentiates between two stimuli is either psychic or physiological. I suppose it must really be either, or perhaps both, but it is indifferent for the present argument whether the processes by which we identify or discriminate grays and loudnesses and handwriting specimens are mental or physiological. Hence this analysis has nothing really to do with any psychological system. I shall try not to disturb the main argument with systematic irrelevances or with my personal notions regarding the psychic or physiological nature of the psychophysical judgment.

Further, I shall not assume that sensations, or whatever the identifying and discriminating functions may be called, are magnitudes. It is is not even necessary for the present argument to assume that sensations have intensity. They may be as qualitative as you like, without intensity or magnitude, but I shall assume that sensations differ. In other words, the identifying process for red is assumed to be different from that by which we identify or discriminate blue.

A term is needed for that process by which the organism identifies, distinguishes, discriminates, or reacts to stimuli, a a term which is innocuous and as non-committal as possible, because we are not now interested in the nature of the process. Sensations, or more generally, subjective conditions would be good terms but physiological states or intraorganic conditions would also be satisfactory. In order to avoid any implications I shall call the psychological values of psychophysics *discriminal processes. The psychophysical problem concerns, then, the association between a stimulus series and the discriminal processes with which the organism differentiates the stimuli.*

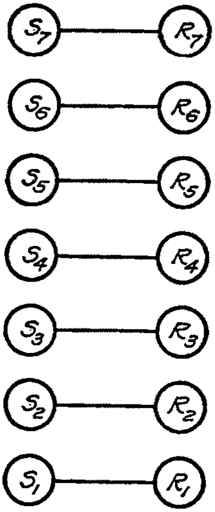

FIG. 1

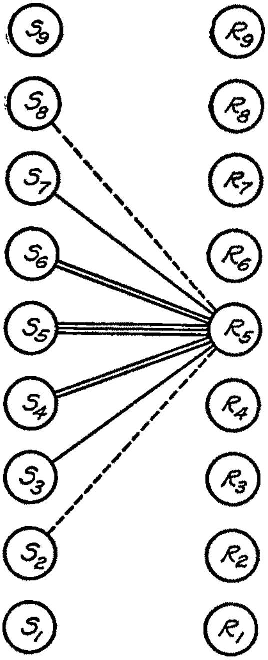

FIG. 2

In Fig. 1 let the circles R_1, R_2, R_3, R_n represent a series of stimuli which constitutes a continuum with regard to any prescribed stimulus attribute. It is not necessary to limit psychophysical analysis to stimuli which have intensity or magnitude as their principal attribute. For example, a series of handwriting specimens may be arranged in a continuum on the basis of general excellence. They would of course arrange themselves in a different continuum if some other attribute were specified such as size of letters, legibility, coarseness of pen, or what not. Similarly a series of spectral colors may be arranged in a continuum for discrimination of brightness, chroma, saturation, apparent remoteness from red, or what not. Psychologically some of these attributes can be measured, while physically the measurement may even be impossible. We are assuming, then, that a series of stimuli have been arranged in a continuum according to any attribute about which one can say 'more' or 'less' and that psychophysics need not be limited to stimuli which have magnitude or size, such as lifted weights and the brightnesses of grays.

Referring again to Fig. 1, suppose that each stimulus in the series has a discriminal process which is a psychic or physiological function of the organism. Thus the stimulus R_5 has a discriminal process S_5 with which the discrimination of the stimulus takes place. These discriminal processes, whatever be their nature, can be labeled only in terms of their corresponding stimuli so that the discriminal process S_5 is labeled by the stimulus R_5 with which it is associated. In the same manner the other discriminal processes in the series may be labeled by the stimuli which produce them. Naturally the discriminal processes would arrange themselves in a totally different order by changing the attribute of the stimuli by which they are arranged in a continuum. We have then two continua, one for the stimuli and one for the discriminal processes of these stimuli. The stimulus continuum must of course be defined in terms of some definite stimulus attribute. The discriminal continuum is a qualitative one which does not necessarily have either magnitude or intensity.

There is of course no possibility of recording experimentally in any direct way these discriminal processes that correspond to a series of stimuli. It is possible, however, to make some interesting inferences about the psychological continuum indirectly. The stimuli may be used to designate locations in the psychological scale just as though the stimuli, or their names, were used as tags or landmarks in a continuum which has otherwise no identifying marks or mile posts. It is the relative separations between these landmarks on the qualitative psychological continuum which it is the central problem of psychophysics to survey. In the figure there is no attempt to indicate quantitatively the relative separations between the stimuli, or between their psychological correlates. The diagram indicates only that for each of the stimuli in the stimulus continuum one may postulate a discriminal correlate and that these psychological correlates also form a continuum of some kind. Nothing more is known, for the purposes of measurement, about the psychological continuum except that a discrete series of discriminal processes of unknown nature can be used as landmarks along its course and that these processes or landmarks are experimentally controlable or identifiable only in terms of the physical stimuli that produce them.

So far the argument has proceeded as though there were a fixed one-to-one relation between the stimuli and their respective psychological correlates. It may be assumed that this relation is not so fixed as might be indicated by Fig. 1. It undoubtedly happens that stimulus R_5, for example, does not always produce the same discriminal process S_5. The present method of psychophysical analysis rests on the assumption that constant and repeated stimuli are not always associated with exactly the same discriminal process but that there is some qualitative fluctuation from one occasion to the next in this process for a given stimulus. This raises an interesting possibility. It might happen for example, that stimulus R_5 has ordinarily S_5 as its discriminal process but that sometimes the qualitative fluctuations would spread to S_4 or to S_6. It might even happen, although rather seldom, that the stimulus R_5 would have as its process S_3 or S_7. It should be recalled that each of these processes or qualities is identified by that stimulus which most frequently produces it so that S_4, for example, is habitually associated with R_4 and so on. This is the fundamental idea of the psychophysical analysis of the present paper.

The variability of this connection between the stimulus and its discriminal process works both ways. A given process S_5 would be associated most frequently with R_5 but occasionally also with adjacent and closely similar stimuli in the stimulus continuum such as R_3, R_4, R_6, R_7. Similarly, the stimulus R_5 can be thought of as most frequently associated with the process or quality S_5 but occasionally with the adjacent qualities such as S_3, S_4, S_6, S_7. Since the discrimination between stimuli is made in the processes of the psychological continuum we shall be concerned with the latter of these two regressions, namely the qualitative fluctuations in the discriminal processes that are associated with a constant and repeated stimulus.

The psychophysical relations may be summarized, so far, in the following propositions.

(1) A series of stimuli R_1, R_2, R_3 . . . R_n can be arranged in a continuum, with reference to any prescribed quantitative or qualitative stimulus attribute.

(2) These stimuli are differentiated by processes of the organism of unknown nature and they are designated S_1, S_2, S_3 . . . S_n respectively. Every stimulus R_k is identified by the organism with the process S_k. These processes may

be either psychic or physiological or both. In this discussion they are referred to as the discriminal processes or qualities.

(3) When the discriminal processes $S_1 \ . \ . \ . \ S_n$ are considered in the same serial order as the corresponding stimulus series they constitute what may be called the discriminal continuum or the psychological continuum. This continuum is the correlate of the already postulated stimulus continuum.

(4) It is assumed that the correspondence $R_n — S_n$ is subject to noticeable fluctuation so that R_n does not always produce the exact process S_n but sometimes nearly similar processes S_{n+1} or S_{n-1} and sometimes even S_{n+2} or S_{n-2}. It goes without saying that the numerical subscripts are here used to denote qualitative similarity and that no quantitative attributes are thereby necessarily injected into the discriminal processes. This fluctuation among the discriminal processes for a uniform repeated stimulus will be designated the *discriminal dispersion.*

In Fig. 2 are represented the two continua, one for the stimulus series and one for the corresponding discriminal processes. Let R_5 be one of the stimuli in the stimulus series. It is asssumed that some discriminal process S_5 occurs more frequently with this stimulus than any of the other processes. Hence it is designated the *modal discriminal process* for that stimulus. In this sense S_5 is the modal discriminal process for the stimulus R_5, and so on.

The relative frequencies of the different processes are represented for stimulus R_5 in a rough diagrammatic way. Thus there are three lines connecting R_5 with S_5 to indicate the relation between the stimulus and its modal discriminal process. There are only two lines connecting the adjacent processes with the same stimulus R_5 and this represents the relatively lower frequency of this association. The processes S_3 and S_7 are connected with the same stimulus with only one line to represent relatively infrequent association. Finally the dotted lines represent in the same manner very infrequent association between the processes so marked and the stimulus R_5. The extreme processes without connection with R_5 represent, then, those processes which are so different from the modal process for R_5 that they never occur in association with the given stimulus or that such association would take place only under unusual conditions as affected by practice, fatigue, sensory adaptation, successive or simultaneous contrast, and so on.

The simplest and perhaps the most obvious plan for scaling would be to assign linear values to the discriminal processes, with reference to a given stimulus, inversely proportional to the frequencies with which these processes occur with the given stimulus. With R_5 as the given stimulus in Fig. 2 the reckoning would start with the corresponding modal process S_5 as an origin or datum. For this stimulus the other processes could be assigned distance-values from S_5 inversely proportional to their frequencies of occurrence with the given stimulus. Any plan that might be adopted is subject to experimental test in that the separations between the processes can be scaled with reference to each of the various stimuli. Naturally these scale distances between the processes should remain practically constant, no matter what the stimulus may be, in order to have a valid measuring method. Experimental test shows that the plan just suggested of assigning distance values on the psychological continuum breaks down. It is found that the separations between the processes do not retain stable values when they are determined for different stimuli. Therefore some other plan must be adopted.

The normal probability curve has been so generally abused in psychological and educational measurement that one has reason to be fearful of criticism from the very start in even mentioning it. The only valid justification for bringing in the probability curve in this connection is that its presence can be experimentally tested. The writer has found experimentally that the normal probability curve was not applicable for certain stimuli. In most of the experiments the distributions are reasonably close to normal.

Since the assumption of a normal distribution for the discriminal dispersion can be experimentally verified and limited to those stimulus series where its reality can be tested, it will be reasonable to make this assumption subject to verification in every case. The hypothesis can be stated as follows. *The discriminal dispersion which any given repeated stimulus produces on the psychological continuum is usually normal. The frequencies with which the discriminal processes occur for a given stimulus ordinarily describe a normal distribution when plotted on the psychological continuum as a base.* In experimental practice the procedure is the reverse of this hypothesis because the frequencies are known first experimentally and from these frequencies we *construct* the psychological continuum. The writer has found in several studies that the separation between any pair of processes remains practically constant no matter which of the neighboring stimuli is used as a base for the calculation. Such is not the case, however, when the separation of any pair of processes is assigned values directly or inversely proportional to their frequency of occurrence.

In Fig. 2 where R_5 is chosen as the stimulus we should therefore, according to this hypothesis, assign scale values to the various processes as distances from S_5 as an origin. These distances would be assigned in terms of the standard deviation of the distribution of process-frequencies. There is of course no further unit in terms of which this standard deviation can be expressed. It is itself a unit of measurement because all that we can do with the psychological continuum is to lay off linear separations between the processes proportional to their true value since, so far as we know, there is in the nature of the case no further absolute unit of measurement for the psychological continuum. But we shall see that it is possible to compare the discriminal dispersions for two stimuli and to determine experimentally the ratio of any two of these dispersions. *Psychological measurement depends, then, on the adoption of one of these dispersions as a base, and the use of its standard deviation as a unit of measurement for the psychological continuum under investigation.*

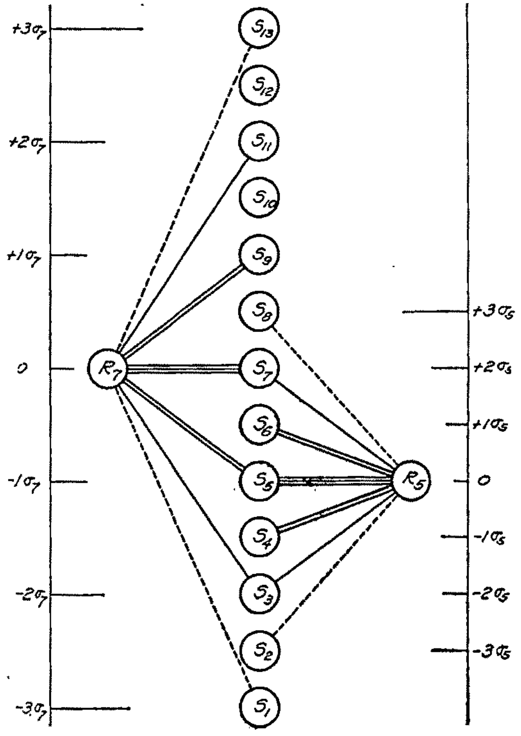

FIG. 3

In Fig. 3 let the column of thirteen circles represent so many discriminal processes, each of them being a modal discriminal process for a stimulus with the same numerical designation. Two of these thirteen stimuli are indicated in the figure, namely R_5 and R_7. Suppose that these stimuli are arranged in a continuum according to any prescribed stimulus attribute and let R_7 be more ambiguous, or less sharply defined, than R_5. An example would be two specimens of handwriting, one of which would be a beautiful but unusual handwriting, or perhaps it might be written in a foreign language, or it might be in German script which would possibly call forth judgments influenced by prejudice from factors other than those of the handwriting characteristics. If the experiment involves the comparison of loudnesses, a variation of the certainty or ambiguity of judgment for a particular stimulus might be caused by variations in timbre or pitch. Ordinarily psychophysical experiments are so set up as to avoid, as completely as possible, the introduction of extraneous factors to influence the ambiguity of judgment and the stimuli are made into as homogeneous a series as may be experimentally possible.

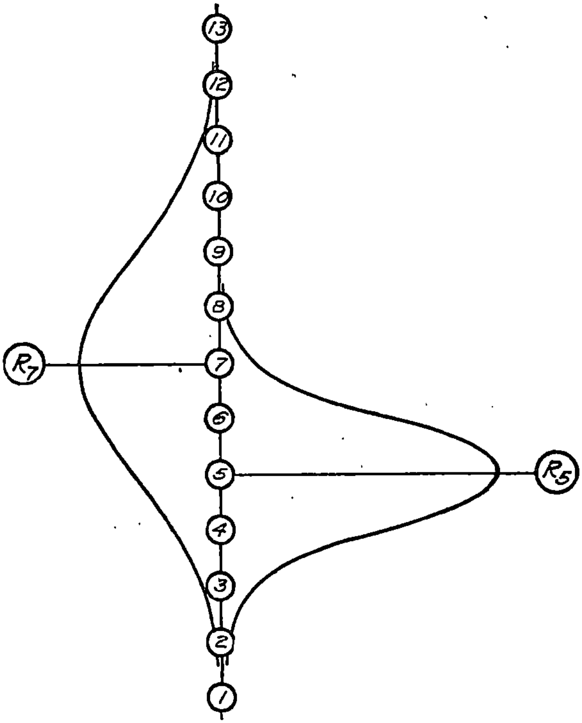

FIG. 4

In Fig. 3, the two stimuli are represented as differing in the certainty with which they can be judged as to the prescribed attribute for the stimulus continuum and R_7 is indicated as the more variable or uncertain of the two. The modal discriminal process for R_7 is S_7 as before, and the discriminal processes S_5, S_3, S_1 might be assigned deviation values of 1σ,

2σ, 3σ respectively from S_7 as a datum. These deviation values would be assigned on the basis of the frequency with which each of these processes occur with R_7 as a stimulus. With the same diagrammatic representation let the other processes be assigned their deviation values from S_7 as a base and let the same processes be assigned frequency-deviation values from S_5 as a base for stimulus R_5. In Fig. 3 these hypothetical deviations are given numerical values. Note from the figure that the discriminal process S_5 which is modal for stimulus R_5 has a deviation value of $-1\sigma_7$ for stimulus R_7. Similarly the discriminal process S_4 has a deviation value of $-1.5\sigma_7$ for stimulus R_7 while it has a deviation value of $-1\sigma_5$ for stimulus R_5. If this analysis is correct it should happen not infrequently that the stimuli which constitute a continuum according to any prescribed stimulus attribute are subject to varying degrees of dispersion when they are perceived or judged. Some stimuli are probably placed with reference to the prescribed attribute more accurately and consequently with a smaller discriminal or subjective dispersion than other stimuli. It is probably true that this variability of the discriminal dispersion on the psychological continuum is of relatively less serious importance in dealing with strictly homogeneous stimulus series but it becomes a serious factor in dealing with less conspicuous attributes or with less homogeneous stimulus series such as handwriting specimens, English compositions, sewing samples, Oriental rugs. In measurements of the type known as judgment scales the discriminal dispersion on the psychological continuum becomes one of the unknowns to be determined as well as the scale value of the specimen. Every specimen in such a series presents two unknown values to be determined: namely, the scale value of its modal discriminal process on the psychological continuum, and its discriminal dispersion.

Instead of the diagrammatic representation of Fig. 3 two normal probability curves may be substituted, subject of course to subsequent experimental verification. This has been done in Fig. 4. Here the psychological continuum has been constructed on the hypothesis that the discriminal processes describe a normal distribution when plotted on that continuum. When R_7 and R_5 are presented for a comparative judgment, each of the stimuli produces a discriminal process of some kind and the certainty of the discrimination may be assumed to be mainly a function of the difference between these two processes. If R_7 happens to be associated with one of the processes at the upper range of its discriminal dispersion and if R_5 happens to be associated with one of the processes at the lower end of its discriminal dispersion, then the discrimination is made with ease and the judgment is correct. If these conditions are reversed so that R_7 has a process slightly below its modal process while R_5 happens to have a process slightly above its modal process, then the two stimuli may even have the same discriminal process and there would be no possibility of a confident discrimination. Finally, if on some occasion R_7 happens to have a process unusually low in its scale, while R_5 has a process higher in the psychological scale, then the judgment would be made, perhaps even with confidence, that R_5 is greater than R_7 and the judgment would be recorded as incorrect. The discrimination is considered, then, as a function of the *discriminal difference* between the two processes that happen to be associated in the same judgment. By the discriminal difference is meant the linear separation on the psychological continuum between the two processes involved in any particular judgment. It may be designated $S_{7.5}$ or more generally S_{ka}. The discriminal difference is the same as the *sense distance* if we allow that the sense distance for two stimuli fluctuates from one occasion to the next.

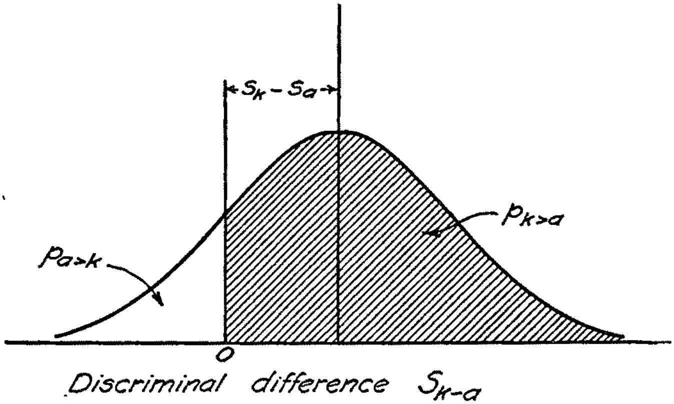

FIG. 5

If in a long series of experimental judgments it were possible to isolate the two discriminal processes for every judgment and if the separation between these two processes for every judgment were recorded, one could tabulate them in the form of a frequency table of discriminal differences. These differences would of course be expressed in terms of some unit of measurement on the psychological continuum. Let the standard stimulus be A and the variable stimulus K. The mean of the distribution of discriminal differences would be the mean or true difference ($S_k - S_a$) and its standard deviation would be

$$\sigma_{ka} = \sqrt{\sigma^2_k + \sigma^2_a}$$

on the assumption that deviations from the modal processes for the two stimuli are not correlated. This distribution is represented in Fig. 5. The base line of this distribution represents discriminal differences in terms of any desired unit of measurement on the psychological continuum. The mean is ($S_k - S_a$) because that is the difference between the two modal processes. The origin represents a difference of zero. This would occur when the two stimuli happen to be associated with the same discriminal process in which case there is no discrimination possible. The points to the right of the origin on the base line represent positive values for the differences S_{k-a} in which R_k has a process higher in the scale than R_a. Similarly the points to the left of the origin represent negative values for the discriminal difference S_{k-a} in which R_k happens to have a process lower in the psychological scale than R_a. It should be recalled that S_{k-a} or S_{ka} represents the sense distance between two stimuli on any particular occasion whereas $S_k - S_a$ represents the mean sense distance for several hundred judgments and it is in scale construction called the 'true' sense distance or scale distance between the two stimuli.

For the present it will simplify analysis to assume that any discriminal difference, no matter how small, is directly reflected in the judgment. A correction may be inserted for this approximation by which a *discriminal difference limen* can be calculated but this correction will not seriously alter the results. It may be assumed for the present that all positive discriminal differences, S_{k-a}, result in the judgment "R_k greater than R_a" and that all negative discriminal differences result in the judgment "R_a greater than R_k." If the two paired stimuli are presented N times there will of course be observed N discriminal differences and their expected distribution is represented in Fig. 5. The shaded portion of that figure represents the expected proportion of judgments "R_k greater than R_a" and these judgments would be correct if K is higher than A. The unshaded portion of the surface represents the expected proportion of judgments "R_a greater than R_k." The proportion of correct judgments will of course increase if the two stimuli are chosen farther apart. Also, the proportion of correct judgments will increase if stimuli are chosen with smaller discriminal dispersions. If the shaded area is greater than $N/2$ it represents correct judgments. If it is less than $N/2$ it represents the proportion of incorrect judgments.

A discriminal difference, S_{k-a}, is not necessarily a magnitude. It is a pair of processes, a pair of qualities. The only way in which numerical value is assigned to it is by placing each of these processes on a measured continuum by means of the frequency with which each of them is associated with the same stimulus. The difference between these two assigned linear values is the discriminal difference, S_{k-a}. The scale distance, $S_k - S_a$, can be defined as the most common discriminal difference, S_{k-a}.

At this point we have arrived at a measure which can be experimentally verified. By the method of constant stimuli it is readily possible to ascertain the actual proportion of judgments "R_7 greater than R_5" for the two stimuli. This proportion is a function of four variables namely S_7, S_5, σ_7, σ_5. If there are n stimuli in the stimulus series there will be $2n$ unknowns to be evaluated, namely n scale values for the modal discriminal processes, and n scale values for the discriminal dispersions. If every stimulus is used in turn as a standard the number of possible pairs of stimuli will be

$$T = \frac{n(n-1)}{2}. \qquad [1]$$

Since there is an experimental proportion 'a greater than b' for every possible pair of stimuli, it follows that there will be $n(n-1)/2$ observation equations and $2n$ unknowns. One of the modal discriminal processes can be chosen as a datum or origin for the psychological scale, and one of the discriminal dispersions can be chosen as a unit of measurement for the construction of a psychological scale. This reduces the number of unknowns to $(2n-2)$ or $2(n-1)$.

TABLE I

Number of stimuli in the series, n	Total number of unknowns, $2(n-1)$	Number of observation equations, $T = n(n-1)/2$
1	0	0
2	2	1
3	4	3
4	6	6
5	8	10
6	10	15
7	12	21
8	14	28
9	16	36
10	18	45

Table I shows for stimulus series of varying length from 1 to 10, the number of available observation equations and the total number of unknowns. When the stimulus series has less than four stimuli, the number of unknowns is greater than the number of observations equations and the problem therefore cannot be solved. When there are four stimuli in the series the number of observation equations exactly equals the total number of unknowns and the problem can then be solved by simultaneous equations. When the stimulus series has more than four stimuli there are more observation equations than there are unknowns and the problem must then be solved by the method of least squares or by some other method of balancing errors of observation.

The fundamental psychophysical equation can then be stated in the following form.

$$S_k - S_a = X_{ka}\sqrt{\sigma_k^2 + \sigma_a^2} \qquad [2]$$

in which S_k and S_a are the two modal scale values on the psychological continuum for the two stimuli R_k and R_a.

X_{ka} is the sigma value for the experimentally observed proportion of judgments "R_k greater than R_a." When these proportions are greater than .50 the stimulus R_k is higher in the psychological continuum than R_a.

σ_k = the discriminal dispersion of R_k on the psychological continuum.

σ_a = the discriminal dispersion of R_a on the psychological continuum.

The assumptions underlying this psychological equation are as follows:

(1) That every stimulus in the stimulus-series is associated with a modal discriminal process with which the organism identifies the stimulus for a prescribed attribute.

(2) That the modal discriminal process for any given stimulus retains at least some of its identity even when the stimulus is combined with other stimuli into a single perceptual judgment.

(3) That the modal processes may be arranged in a linear psychological continuum in the same serial or rank order as the corresponding stimulus series.

(4) In addition to arranging the discriminal processes in rank or serial order, *linear separations* between them are assigned on the assumption that the discriminal dispersion for any stimulus is normal on the psychological continuum. This assumption is subject to experimental verification.

(5) That the discriminal deviations for the different stimuli are uncorrelated. This a fairly safe assumption but if they are correlated, the psychophysical equation [2] becomes

$$S_k - S_a = X_{ka}\sqrt{\sigma^2_k + \sigma^2_a - 2 \cdot r_{ka} \cdot \sigma_k \cdot \sigma_a}$$

in which case the numerical solution becomes unwieldy.

(6) That all positive discriminal differences S_{k-a} give the judgment '$k > a$,' that all negative discriminal differences S_{k-a} give the judgment '$k < a$,' and that discriminal differences of zero, $S_{k-a} = O$, are equally distributed between 'higher' and 'lower' if only two judgments are allowed. This is a close approximation to truth but a correction can be introduced in terms of a discriminal difference limen for judgments 'equal' and 'doubtful.' This correction is left for a separate paper.

Experimental Procedure for Verifying Assumed Normality of Discriminal Dispersion

Assumption (4), that the discriminal dispersion for any stimulus is normal on the psychological continuum, may be experimentally tested by ascertaining whether the separation between any two modal processes (sense distance) remains constant no matter which of the stimuli is used as a base. Consider R_k as the base or standard for equation [2]. Then the proportion of judgments $k > a$ will be controlled by the relation

$$S_k - S_a = X_{ka}\sqrt{\sigma^2_k + \sigma^2_a} \qquad [2]$$

Similarly for the proportion of judgments $k > b$,

$$S_k - S_b = X_{kb}\sqrt{\sigma^2_k + \sigma^2_b}$$

Subtracting,

$$S_b - S_a = X_{ka}\sqrt{\sigma^2_k + \sigma^2_a} - X_{kb}\sqrt{\sigma^2_k + \sigma^2_b} \qquad [3]$$

If the same equation is written with R_l, R_m, R_n, as standards, we have

$$\begin{aligned} S_b - S_a &= X_{la}\sqrt{\sigma_l^2 + \sigma_a^2} - X_{lb}\sqrt{\sigma_l^2 + \sigma_b^2} \\ &= X_{ma}\sqrt{\sigma_m^2 + \sigma_a^2} - X_{mb}\sqrt{\sigma_m^2 + \sigma_b^2} \\ &= X_{na}\sqrt{\sigma_n^2 + \sigma_a^2} - X_{nb}\sqrt{\sigma_n^2 + \sigma_b^2} \end{aligned}$$

If every separation such as $S_b - S_a$ remains constant when determined by different stimuli such as R_k, R_l, R_m, R_n, as standards, then internal consistency for the measurements has been demonstrated and the validity of assumption (4) is thereby established. Such internal consistency depends on the nature of the assumed distribution of discriminal processes by which the psychological continuum is constructed.

The point of view that I am describing has many implications bearing on well known psychophysical principles. One of the conclusions to which the present analysis leads is that Fechner's law and Weber's law are really independent and that it is consequently incorrect to speak of these two laws jointly as 'the Weber-Fechner law.' Another important conclusion relates to the well known hypothesis that equally often noticed differences are equal. The present analysis shows that hypothesis is incorrect because it is possible for two differences to *seem unequal* on the average and yet be equally often discriminated. Other implications concern the limitations of the phi-gamma hypothesis in psychophysical experimentation and the distribution of judgments of equality. A few applications of the concept of discriminal dispersion are described below.

FECHNER'S LAW

Fechner's law is usually phrased as follows:

$$S = K \log R$$

in which S represents sensation intensity which we have here called scale value. The notation R refers to stimulus intensity or magnitude. It will be noticed that in writing our psychophysical equation nothing has been said about stimulus magnitudes or intensities because of the fact that many stimulus series that are subjected to psychological measurement are not capable of quantitative measurement on their objective side. For example, the relative excellence of a series of handwriting specimens may be measured on a psychological continuum but the corresponding physical 'magnitudes' probably do not exist as a single variable. The physical handwriting specimens cannot be readily measured as to the stimulus variable 'excellence.'

Fechner's law can be applicable only to those stimulus series in which the attribute which is being judged can also be physically isolated. Then, if the discriminal separations of the psychological continuum are plotted against the physical stimulus attribute and if this plot is logarithmic, Fechner's law is verified.

In many cases there is no possibiltiy of making sure that the physical variable really corresponds to the psychological one. For example, a series of circles can be arranged in a stimulus series in accordance with their diameters. The discriminal experiments may then be carried out with instructions to indicate which of two exposed circles is the larger without specifying further what is meant by larger. The circles would no doubt arrange themselves in the same serial order in the psychological continuum as in the stimulus continuum so that the two series would have exactly the same rank orders. Now, if we want to verify Fechner's law, we should plot the separations between the modal processes for the circles along the psychological continuum against the corresponding physical stimulus variable. Shall we plot diameters on the base line or shall we plot areas? These two plans would arrange the stimulus series in the same rank order, but the relation between diameter and area is not linear. Both diameter and area would be physical variables covariant with the apparent bigness of the circles. Now, if Fechner's law is verified for one of these physical variables, it could not possibly be verified for the other because of the non-linear relation of diameter and area. If we should find experimentally that Fechner's law is satisfied by plotting the psychological continuum against the diameters, for example, that would not justify the conclusion that Fechner's law applies. We could artificially force Fechner's law everywhere by merely selecting that particular stimulus variable which does give a logarithmic relation with the psychological continuum. Fortunately the law has been shown to hold true for many stimulus series in which there is hardly any possibility of an ambiguous stimulus variable and its universality therefore commands our respect.

WEBER'S LAW AS INDEPENDENT OF FECHNER'S LAW

In the present discussion Weber's law is interpreted broadly for the frequently observed relation between the stimulus magnitudes and the scale distances on the psychological continuum. I am not here limiting myself to those particular applications of the law by which it is restricted to sensory intensities. The law is not always verified for sensation intensities, but, on the other hand, I have found it applicable to some other stimulus series that are not sensory intensity magnitudes. The present discussion of Weber's law concerns the functional relation between stimulus magnitudes and psychological scale distances without implying that the law is limited to sensory stimulus intensities.

Weber's law and Fechner's law are often described together and they are frequently called jointly 'the Weber-Fechner law.' The two laws are independent so that either one of them may be applicable without the other being verified for a particular set of data. The two laws must be separately verified for any given set of data.

Weber's law is usually stated as follows: The just noticeable increase of a stimulus is a constant fraction of the stimulus. The term 'just noticeable' is ambiguous so that it is necessary to specify how often a stimulus increase must be correctly noted in order for the stimulus increase to be called 'noticeable.' This frequency is often placed arbitrarily at 75% of the judgments when two judgments are allowed. Restating Weber's law with this provision so as to remove the ambiguity of the term 'just noticeable' we have the following statement of the law: The stimulus increase which is correctly discriminated in 75% of the attempts, when only two judgments 'higher' and 'lower,' or their equivalents, are allowed, is a constant fraction of the stimulus magnitude. With reference to Fechner's law there are two cases under which Weber's law may be verified. In case 1 Fechner's law is postulated, and in case 2 it is not postulated.

Case 1. Let the stimulus magnitude be designated R_a and let it be increased to the magnitude R_b at which separation the two stimuli are correctly discriminated in 75% of the attempts by the constant method and with two judgments allowed. At this separation between the two stimuli our psychophysical equation [2] takes the following form:

$$S_b - S_a = X_{ab}\sqrt{\sigma_a^2 + \sigma_b^2}$$

which, when stated explicitly for the required proportion of 75% judgments "*b* greater than *a*," becomes

$$\frac{S_b - S_a}{\sqrt{\sigma_b^2 + \sigma_a^2}} = X_{ab} = 0.674. \qquad [4]$$

Weber's law states that any pair of stimuli, R_a and its increased magnitude R_b, corresponding to the two modal processes S_a and S_b in the above equation, are such that the fraction R_b/R_a remains a constant no matter what the absolute magnitudes of the stimuli may be. It is clear from the above equation that the separation between the two stimuli which gives a result of 75% correct judgments is a function not only of the two stimulus magnitudes and their corresponding modal processes but also of the discriminal dispersions for the two stimuli. Weber's law may be verified under *Case 1* if an additional condition is satisfied, namely, that the discriminal dispersions are the same for all the stimuli. If the discriminal dispersions are not constant, then it is possible for Fechner's law to be applicable when Weber's is not. If the discriminal dispersions are equal for all the stimuli, then equation [4] may be written as follows:

$$\frac{S_b - S_a}{\sigma\sqrt{2}} = X_{ab} = 0.674 \qquad [5]$$

and since the discriminal dispersion which is here assumed to be constant may be taken as a unit of measurement on the psychological continuum, we have

$$\frac{S_b - S_a}{\sqrt{2}} = 0.674$$

$$\text{or } S_b - S_a = \sqrt{2}\cdot 0.674. \qquad [6]$$

This relation is obtained by condition (2) above. But Weber's law states a constant relation in terms of the stimuli. This transformatinon can be made by Fechner's law as follows:

$$S_a = K \log R_a$$
$$S_b = K \log R_b$$
$$S_b - S_a = K [\log R_b - \log R_a]$$
$$S_b - S_a = K \log \frac{R_b}{R_a} \qquad [7]$$

From equations [6] and [7] we have

$$S_b - S_a = \sqrt{2} \cdot 0.674 = \mathrm{K} \log \frac{R_b}{R_a}$$

or simply

$$\log \frac{R_b}{R_a} = \text{constant}$$

and hence

$$\frac{R_b}{R_a} = \text{constant}$$

thus verifying Weber's law. But in order to verify Weber's law under *Case 1* it was necessary to make two assumptions, namely that Fechner's law applies and also that the discriminal dispersions are constant. If stimuli were used of varying degrees of homogeneity or ambiguity the discriminal dispersions would not be constant and it would then be possible to discover that Fechner's law is applicable when Weber's law is not.

Case 2. It is possible for Weber's law to be applicable when Fechner's law is not verified and when the discriminal dispersions are not all equal. This is best illustrated by a short list of stimuli with hypothetical discriminal dispersions. For the purpose of this illustration we can assume any relation between S and R except the logarithmic relation of Fechner's law. Let us tabulate some paired values for S and R such that $S = R^2$. This is clearly, then, a case in which Fechner's law does not apply. In Table II the first column identifies the six stimuli in the hypothetical series. Column R designates the stimulus magnitudes. Column S shows the scale values of the cor-

TABLE II

Stim. Series	R	$S = R^2$	σ	$P_{(R+1)>R}$
1	10.00	100.	20.00	.75
2	11.00	121.	23.94	.75
3	12.10	146.	29.16	.75
4	13.31	177.	35.40	.75
5	14.64	215.	43.40	.75
6	16.11	259.	51.90	.75

responding modal processes (sensation intensities). Column σ shows a hypothetical series of discriminal dispersions. By means of the fundamental psychophysical equation [2] it can then be shown that the stimuli 1 and 2 are correctly discriminated in 75% of the judgments, that stimuli 2 and 3 are correctly discriminated in 75% of the judgments, and so on. Since the ratio of each stimulus magnitude to the next lower stimulus magnitude is always 1.10 in this table and since these successive pairs of stimuli are correctly differentiated in 75% of the observations, we conclude that Weber's law has been verified by these hypothetical data. The only new factor that we have introduced is the plausible assumption that the discriminal dispersion may not be constant throughout the whole stimulus range. With an assumed variation in the discriminal dispersion we find that it is logically possible to have a set of data in which Weber's law is verified but in which Fechner's law is not verified. All that is necessary for the discriminal dispersion to vary from one stimulus to another is that the stimuli be unequal in the ambiguity or difficulty with which they are judged and this surely must happen much more often than we suspect when the stimuli consist in such qualitative values as handwriting specimens or specimens of English composition. It is quite probable that the variation in discriminal dispersion is rather slight and perhaps negligible when the stimulus series is rather homogeneous. A good example of a homogeneous stimulus series is a set of cylinders for the lifted weight experiment in which size, color, texture, shape, and even temperature are ruled out of the experiment by keeping them constant. In such experiments it is probable that the discriminal dispersion stays constant.

Finally, if the discriminal dispersions can be assumed to be equal throughout the whole stimulus range, then Fechner's law and Weber's law become identical. The frequent association of these two laws as though they were always identical depends on the constancy of the discriminal dispersion. It may be expected in psychophysical experiments with stimuli that are not experimentally kept constant in all but one stimulus variable, that one or two stimuli in the series are more difficult to judge than the rest. In such a case these one or two stimuli will have larger discriminal dispersions than the other stimuli and the consistency of the psychological continuum is thereby disturbed if these variations are not accounted for in the derivation of the scale values.

EQUALLY OFTEN NOTICED DIFFERENCES ARE NOT NECESSARILY EQUAL

It is usually assumed that equally often noticed differences are equal on the psychological continuum. They are rarely assumed to be equal on the stimulus continuum. It is however incorrect to assume that pairs of stimuli are equally distant on the psychological scale even though all the pairs are equally often discriminated. It is not even correct to say that stimulus differences *seem* equal, or that they are subjectively equal, just because the differences are equally often noticed. Two pairs of stimuli may be equally often discriminated while one of the separations may on the average actually *seem* greater than the other.

Referring again to the psychophysical equation [2] the psychological or *apparent* separation between two stimuli R_a and R_b is expressed by the difference $(S_b - S_a)$, measured on the psychological scale which is a scale of appearances or impressions. The frequency with which the two stimuli can be discriminated is, however, a function of their respective discriminal dispersions as well as their modal discriminal processes. The separation between the modal processes can also be called the mean sense distance. Here again, if we can assume that the discriminal dispersions are constant, then it is correct to say that equally often noticed differences are psychologically equal but that assumption should be tested before constructing a psychological continuun or scale by means of this assumption.

A POSSIBLE EFFECT OF PRACTICE

It is probable that practice has the effect of reducing the the discriminal dispersions and that this may account for the shifts in the proportions of correct judgments in psychophysical experiments. If two stimuli are presented to an unpracticed subject for whom these stimuli have relatively large discriminal dispersions, the denominator of equation [4] will be relatively large while the numerator remains constant. Graphically the situation can be represented in Fig. 5 by increasing the standard deviation of that probability curve while the separation $(S_k - S_a)$ remains constant. This produces a low proportion of correct judgments. With practice, the subject reduces the discriminal dispersions and this might be represented in Fig. 5 by reducing the standard deviation of that curve while the separation between the two modal processes remains constant. The effect is to increase the proportion of correct judgments. Naturally, stable results for the construction of a psychological scale depend on reaching such a practice level that the discriminal dispersions will remain practically constant throughout the experiments. The interpretation of the psychophysical equation in connection with the effect of practice would be that two lights, for example, seem just about as bright to the practiced laboratory subject as to an unpracticed subject. Practice in psychophysical experimentation does not make one of the lights seem brighter or the other one weaker. The two lights retain their same general level of brightness except for sensory adaptation and contrast which are momentary effects. But there is a practice effect in the capacity to discriminate between the two lights. This is determined by the discriminal dispersion or subjective observational error. Here again, equally often noticed differences are not necessarily equal subjectively or psychologically.

EXPERIMENTAL TEST

The simplest experimental procedure for verifying the assumption that the discriminal dispersions are constant for any particular stimulus series is probably to arrange a table showing the proportion of judgments, $P_{a>b}$, for all the possible pairs of stimuli. If there are N stimuli, such a table will contain $N(N-1)$ entries if identical stimuli are not experimentally compared. From such a table the stimuli can readily be arranged in rank order. From the table of proportions of judgments, a corresponding table of sigma values can be prepared. One can then plot a graph for X_{ka} against X_{kb} in which a and b are standards. If the discriminal dispersions are equal through the stimulus series, the graph should give a linear plot with a slope of unity. This may be demonstrated as follows:

If in the psychophysical equation

$$S_k - S_a = X_{ka}\sqrt{\sigma_k^2 + \sigma_a^2} \qquad [2]$$

we assume that the discriminal dispersions are equal, the equation becomes

$$\begin{aligned} S_k - S_a &= X_{ka}\sqrt{2\sigma^2} \\ &= X_{ka}\cdot\sigma\cdot\sqrt{2} \qquad [5] \end{aligned}$$

and if we use the discriminal dispersion as a unit of measurement on the psychological scale, we have

$$S_k - S_a = X_{ka}\cdot\sqrt{2} \qquad [6]$$

By symmetry it follows that

$$S_k - S_b = X_{kb}\cdot\sqrt{2} \qquad [7]$$

Subtracting and transposing,

$$X_{ka} = X_{kb} + \frac{S_b - S_a}{\sqrt{2}} \qquad [8]$$

This equation is in linear form and if X_{ka} is plotted against X_{kb} we should have a linear plot. The slope should be unity and

$$Y\text{-intercept} = \frac{S_b - S_a}{\sqrt{2}} \qquad [9]$$

If the plot is linear, it proves that the assumed normal distribution of discriminal processes is correct. If the slope is unity, it proves that the discriminal dispersions are equal. It is left for a separate paper to apply this method to educational judgment scale data.

LINEAR INTERPOLATION *VS.* THE CONSTANT PROCESS

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Much has been done to simplify the application of the method of constant stimuli. Computation has been facilitated by the publication of Urban's Tables[1] and later of various checking tables,[2] by detailed accounts of procedure,[3] and by the suggestion of various minor aids.[4] Given the values of the stimuli and the observed frequencies, a threshold can be computed in about 30 minutes provided the first figures check and the computor is familiar with the task.

On the experimental side, certain technical variations in instruction, recording and apparatus have resulted in decreasing the time necessary for obtaining the subject's judgments. Furthermore, it has been shown for lifted weights at least, that the number of the comparison stimuli can be reduced from 7 to 5 without substantially modifying the constants of the psychometric functions and without necessitating a greater number of series.[5]

The requisite number of series is itself a problem and depends on a number of factors such as the training of the subject, his attitude and sensitivity, and the nature of the stimuli. There is experimental evidence that the value of the measure of precision and the approximation of observed frequencies to the phi-gamma hypothesis increase with practice, especially if the subject at the start is untrained in laboratory observation.[6] It is possible then that 200 series, say, may be necessary in one situation, while as few as 20 may be sufficient in another, though it is quite impossible at present to make accurate predictions in the matter. It has been pointed out, too, that the certainty required is dependent upon the nature of the problem.[7] Were a highly 'significant' difference between two limens required, for example, it might be necessary to employ a relatively large number of series to obtain the requisite reliability. The same is true for the commoner problem of securing the usual reliability for a small difference.

Another approach to the problem of simplifying the process of obtaining an accurate limen is to compare different psychophysical methods and results secured by using them. Certain comparisons have been made though not always with respect for this problem. Urban, for instance, found the phi-gamma hypothesis to be more satisfactory than the arctan.[8] Thomson and Fernberger each found it possible to obtain comparable results by the methods of limits and of constant stimuli.[9] Irrespective of theoretical difficulties, other methods have been presented which employ a simple average value based upon all the data.[10] Various methods of interpolation of the limen have been suggested, sometimes as complete methods and sometimes as accessory to more elaborate methods.[11]

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[1] F. M. Urban, *Arch. f. d. ges. Psych.*, 24, 1912, 240 f.

[2] G. J. Rich, this JOURNAL, 29, 1918, 120 f.
L. B. Hoisington, this JOURNAL, 33, 1922, 244-246.

[3] E. B. Titchener, *Quantitative Manual.*
E. G. Boring, this JOURNAL, 28, 1917, 280-293.
W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, 1921.

[4] For instance, suggestions concerning the use of calculating machines, the probable loci of errors in computation, and forms to guide the calculation. The writer has used a mimeographed form in computing a large number of limens. This was suggested by Boring, *op. cit.*, 284, and differs from that in that columns 9 to 15 are replaced by a single column headed 'Rich's Check' which simplifies the checking of the Urban's Tables entries. Furthermore, the formulae for L and h are clearly indicated in the form, minimizing errors in substituting the numerical values.

[5] S. W. Fernberger, this JOURNAL, 25, 1914, 121-130; and also *Psychol. Rev.*, 21, 1914, 335-355.

[6] L. B. Hoisington, this JOURNAL, 28, 1917, 588-596.
Brown and Thomson, *op. cit.*, 1921, 90 ff.
S. W. Fernberger, this JOURNAL, 34, 1923, 498-500.
E. Culler, *J. Exp. Psychol.*, 9, 1926, 169-190.

[7] E. G. Boring, this JOURNAL, 27, 1916, 315-319.
S. W. Fernberger, *Psychol. Bull.*, 14, 1917, 110.

Problem and Method

Simple linear interpolation, as far as the writer is aware, has never been extensively compared in its application with theoretically more precise methods of computation. It has the obvious advantage of a negligible amount of calculation, and it is clear that if it were found to give results as precise as those of the constant process its use would be widely indicated; or if the results were only approximately as accurate the requirements of many problems would be met. Preliminary study of a limited amount of data led the writer to believe the problem worthy of more extended investigation. The problem of present report is, then, to estimate the adequacy of several simple interpolation methods by trying them out on actual data and comparing results with those of the constant process.[12]

As Urban has recently pointed out, there are abundant lifted weight data in the literature which might well be utilized for problems of this character. The writer selected, from among the data of three lifted weight investigations, 100 psychometric functions believing them to constitute a fairly repsentative sample.[13] The data are of 15 *S*s who varied in respect of age, sex, practice, attitude or instruction, length of session, and other possibly less important matters. In regard to constants, it can be said that each set of frequencies resulted from 100 series using five comparison stimuli, 88, 92, 96, 100, 104 grm., and a standard of 100 grm. The observed frequencies, forming the basis for all our computations, are given in Table I, to which reference will again be made.

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[8] F. M. Urban, *Psychol. Rev.*, 17, 1910, 257-9 with reference there given.

[9] Brown and Thomson, *op. cit.*, 1921, Ch. 2.
G. H. Thomson, *Brit. J. Psychol.*, 5, 1912-13, 226 ff.
S. W. Fernberger, *Psychol. Monog.*, 14, 1912-13, No. 4.

[10] G. E. Müller, *Die Gesichtspunkte und Tatsachen der psychophysischen Methodik*, 1904.
C. Spearman, *Brit. J. Psychol.*, 2, 1906-8, 227-242; but cf. Brown and Thomson, *op. cit.*, 61 ff.
R. Pauli, *Psychologisches Praktikum*, 1923, 34-37.

[11] Brown and Thomson, *op. cit.*, 60 f.
E. B. Titchener, *Student's Manual—Quantitative*, 95 f.
A. Lehmann, *Arch. f. d. ges. Psychol.*, 8, 1905-6, 484 ff.
F. M. Urban, *Psychol. Rev.*, 17, 233 ff.

[12] I am greatly indebted to Professor Boring for a number of suggestions concerning both the investigation and the manuscript.

[13] F. M. Urban, Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, *Arch. f. d. ges. Psychol.*, 15, 1909, 268-271. Presented here are 102 sets of p-values, each set based on 50 judgments with each of seven comparison stimuli. Eliminating the extreme upper and lower p-values, I combined intermediate pairs, presumably obtained under comparable conditions, and so obtained sets based on 100 judgments at each of five stimulus values. Thirty-five such sets were selected for the present study, *i.e.* the first thirty-five in Table I.

S. W. Fernberger, On the elimination of the two extreme intensities of the comparison stimuli in the method of constant stimuli, *Psychol. Rev.*, 21, 1914, 342, 351. Presented here are 80 sets of p-values, each set based on 100 judgments with each comparison stimulus. Thirty-seven sets were selected from the "reduced series observed," *i.e.* series in which but five stimulus values were used experimentally.

E. S. Rudisill, Constancy of attitude in weight perception, this JOURNAL, 36, 1925, 576. Tables 1-4. Presented here are 144 sets of p-values each set based on 50 judgments with each of seven comparison stimuli. I proceeded in the same manner as with the Urban data to obtain sets based on 100 judgments at each of five stimulus values. Twenty-eight such sets were selected for use in the present investigation.

TABLE I
OBSERVED FREQUENCIES

The capital letters signify experimenters and the numbers following subjects, *e.g.* U3 = Urban's *S* no. 3. Cf. footnote 13. The ascending and descending frequencies represent, respectively, judgments of 'heavier' and of 'lighter.'

Series	*E* & *S*	Stimuli				
		88	92	96	100	104
1	U1	4	10	27	34	84
2		1	11	19	32	88
3		3	8	19	55	93
4		72	41	28	10	1
5		93	49	44	13	1
6	U2	3	18	34	49	77
7		4	7	31	48	95
8		86	67	46	25	15
9		84	71	40	29	1
10		86	63	40	16	2
11	U3	5	14	38	44	87
12		5	10	24	57	88
13		1	3	20	57	89
14		89	72	47	25	4
15		92	80	66	35	5
16	U4	2	8	39	58	90
17		3	21	36	66	88
18		90	79	41	25	6
19		90	65	39	19	4
20		97	86	52	19	5
21	U5	1	14	34	46	70
22		2	17	44	68	93
23		4	17	76	90	98
24		88	71	38	18	6
25		98	78	39	31	3
26	U6	6	13	31	51	82
27		1	2	17	32	60
28		81	62	48	21	4
29		87	82	50	16	4
30		91	71	49	25	12
31	U7	1	8	9	41	65
32		1	4	19	35	79
33		90	70	46	28	10
34		88	83	52	36	6
35		91	69	39	16	2
36	F1	3	26	39	51	85
37		1	23	45	60	79
38		3	5	36	49	67
39		4	12	51	61	77
40		94	58	53	36	10
41		94	69	47	32	12
42		97	84	38	25	15
43		87	81	36	29	10
44		87	75	30	24	11
45	F2	20	32	51	80	79
46		8	27	43	58	68
47		2	14	41	56	79
48		2	16	40	52	74
49		5	27	55	71	88
50		60	34	24	9	9
51		69	32	18	6	3
52		52	25	7	2	2
53		52	15	5	2	1
54	F3	7	30	42	78	83
55		6	31	55	64	77
56		2	7	28	53	74
57		1	3	25	39	61
58		91	55	37	11	13
59		90	55	34	30	14
60		92	80	43	16	7
61		92	75	44	9	5
62		90	81	52	16	4
63	F4	14	41	61	66	78
64		8	43	58	77	82
65		18	28	46	65	73
66		20	27	59	65	69
67		23	28	51	65	74
68		86	58	39	34	22
69		92	57	42	23	18
70		78	65	42	30	25
71		78	73	33	33	29
72		73	67	43	29	20
73	R1	15	33	41	65	62
74		13	31	33	58	77
75		5	22	39	49	69
76		67	61	35	27	14
77		65	44	33	13	8
78		65	52	25	15	13
79		72	52	32	15	7
80	R2	15	19	28	55	78

TABLE I (Continued)

Series	*E* & *S*	88	92	96	100	104	Series	*E* & *S*	88	92	96	100	104
81		15	13	46	60	74	91		68	53	37	22	18
82		12	22	39	56	74	92		75	54	33	16	11
83		14	24	50	62	77	93		75	52	31	20	4
84		69	39	18	8	6	94	R4	11	39	59	83	90
85		75	57	44	17	9	95		8	22	49	57	88
86		72	58	22	9	5	96		11	41	58	67	91
87	R3	14	22	42	44	61	97		13	26	44	72	91
88		13	22	42	52	65	98		83	65	35	18	9
89		16	24	35	59	67	99		90	88	55	16	8
90		62	33	24	18	9	100		84	75	54	5	4

The 100 constant process limens and measures of precisions (L and h) were tabulated to permit comparison with the corresponding limens obtained by interpolation methods.[14] Four interpolation methods were applied to each set of frequencies and so 400 interpolated limens were obtained. All these limens are not published because they have no very direct bearing on the problem.

Two rather familiar assumptions involved in the constant process are as follows: (1) when a liminal or near-liminal stimulus is presented a sufficiently large number of times the frequency of right cases may serve as a measure of the perceptibility of the stimulus; and, (2) the actual relation between percentage of right cases and magnitude of stimulus is a function of the normal curve. Two of our interpolation formulae, which are fundamentally the same, make these same assumptions, and are mathematically sound in the sense that they may be derived from the normal curve. The other two are also similar to each other but they differ from the first pair in that they assume a rectilinear relationship between frequency of correct cases and magnitude of stimulus.

The rectilinear interpolation is simple: given two frequencies of judgment, one less and one greater than frequency 0.5, and given the corresponding stimulus values and their difference, the liminal stimulus is readily obtained by interpolation. Referring to Fig. 1, of rectilinearly distributed frequencies, let:—

R_{50} = limen

R_l = a lower stimulus value, *i.e.* less than R_{50}

R_u = an upper stimulus value, *i.e.* greater than R_{50}

P_l = the difference in the frequencies corresponding to R_l and R_{50}

P_u = the difference in the frequencies corresponding to R_{50} and R_u

$$\text{Then } \frac{P_l}{P_l + P_u} :: \frac{R_{50} - R_l}{R_u - R_l}$$

for corresponding sides of similar triangles are proportional. The only unknown in this proportion is $R_{50} - R_l$ so that is easily obtained, and the value then added to R_l. This result is the liminal value,[15] for:—
$R_l + (R_{50} - R_l) = R_{50}$. The complete interpolation formula is, then:

$$L_{r2} = R_l + \frac{P_l}{P_l + P_u}(R_u - R_l). \qquad [1]$$

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[14] The values of the constant process limens of the Fernberger data were taken from his paper, *op. cit.*, 344, 352; but the values of h being unpublished were obtained, by permission of Professor Fernberger, directly from the original records. The other values of L and h, together with the interpolated limens, were computed by the writer or assistants.

[15] Cf. Brown and Thomson, *op. cit.*, 60 f., and Titchener, *op. cit.*, 95 f.

In the symbol 'L_{r2},' L stands for limen, r for the fact that it was obtained by rectilinear interpolation, and 2 indicates that two stimulus values with the corresponding observed frequencies served as the basis for interpolation. The bold-faced figures in Table I are the frequencies to which this formula was applied. In general, those figures were so selected as to include the limen but without further attempt to obtain values most favorable for interpolation.[16] The interpolated limen from the first set of frequencies in Table I is obtained as follows:

$$L_{r2} = 92 + \frac{40}{40 + 34}(104 - 92) = 98.49.$$

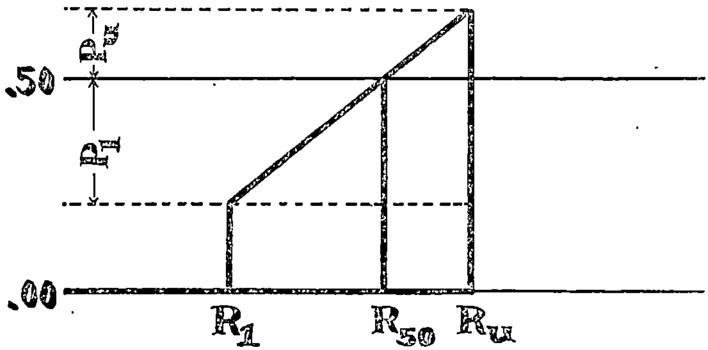

FIG. 1

The symbol "L_{r4}" is used to represent the other variety of rectilinear interpolation, and here the 4 means that four stimulus values with their frequencies were used, instead of two. The means of the two lower stimuli, of the two lower frequencies, of the two upper stimuli, and of the two upper frequencies formed the basis for interpolation:

$$L_{r4} = \frac{R_l + R'_l}{2} + \frac{\frac{P_l + P'_l}{2}}{\frac{P_l + P'_l}{2} + \frac{P_u + P'_u}{2}} \left(\frac{R_u + R'_u}{2} - \frac{R_l + R'_l}{2}\right) \quad [2]$$

The rule for selecting values for this interpolation was to select that set of two adjacent pairs of frequencies the average of whose averages would be nearest 50. The aim here was to obtain optimum values for interpolation whereas for formula [1] we aimed to obtain random values. Using this formula, the limen from the first set of frequencies is obtained as follows:

$$L_{r4} = 94 + \frac{31.5}{31.5 + 9}(102 - 94) = 100.22.$$

---

[16] The basis of selection of these figures was as follows: the p's corresponding to the stimulus values just above and just below the middle stimulus were used provided the upper p lay within the range + 1.5 sigma and the lower within − 1.5 sigma. In other words, those p-values were used when the lower was between 7 and 50 and the upper between 50 and 93. When these conditions were not fulfilled, one or both p's as the case might be, were selected from among the other p's in order to obtain the two p's within the selected ranges. In this way, then, the extremes of the distribution were avoided and p-values on either side of the limen were chosen; but there was no further attempt to select the most favorable figures for interpolation.

It will be observed that L_{r4} is based upon twice as many observations (400 in the present data) as L_{r2}. In other words, L_{r4} is based upon four-fifths of all the data and L_{r2} upon two-fifths.

The formulae for curvilinear interpolation differ from those just discussed only in that they assume normality of distribution.

$$L_{c2} = R_l + \frac{V_l}{V_l + V_u} (R_u - R_l) \qquad [3]$$

The symbol 'V_l' stands for the distance in terms of P.E. (or gamma or sigma) corresponding to the difference in the frequencies for R_l and R_{50}, or expressed more briefly, to P_l. The value of V_l is immediately found by entering a table of the normal probability integral with the value of P_l. Similarly V_u corresponds to P_u. The values of V_l and V_u may be obtained in terms of P.E., gamma, or other dispersion unit indifferently, for it is only the ratio $\frac{V_l}{V_l + V_u}$ which the formula requires. Except for these substitutions for P_l and P_u, this formula [3] is identical with [1]. Furthermore, formula [4] is analogous to [2]:

$$L_{c4} = \frac{R_l + R_l}{2} + \frac{V_l}{V_l + V_u}\left(\frac{(R_u + R'_u}{2} - \frac{R_l + R'_l}{2}\right) \qquad [4]$$

The 'L_{c4}' limen from the first set of frequencies:

$$L_{c4} = 94 + \frac{.8965}{.8965 + .2275} (102 - 94) = 100.38$$

It was found necessary to extrapolate eight of the 400 limens for in those cases the liminal values fell outside the selected frequencies. This extrapolation was performed with appropriate modifications of the formulae given above.

Several points concerning these formulae may now be summarized: (1) the first two make the assumption of rectilinearity; (2) none of them employs all the data; (3) none of them takes account of the Müller-Urban weights; (4) all of them are very easy to apply; (5) the accuracy of any of them, as compared with the constant process, depends in some measure upon the symmetry of placement of the selected frequencies; (6) the accuracy of the last two also depends upon the normality of the data; (7) none of them can be expected to give results consistently identical with those of the constant process. Our specific problem, to repeat, is to discover the degree to which these interpolated values do approximate the constant process values, in data chosen from the literature.

RESULTS

In Table II are the Pearsonian coefficients of correlation between the constant process and the several interpolated limens; they are all positive and very high with P. E.'s very low. The left pair of coefficients in the table, moreover, are almost identical and somewhat lower than the right pair, which are identical. Thus it comes about that the probable correctness of the direction of difference between either L_2 coefficient and either L_4 coefficient, is very nearly 1.000. On the basis of these results, then, the L_{r4} and L_{c4} interpolations appear significantly superior to the others; while at the same time all four appear closely to approximate the constant process. This, however, does not throw much light on the interesting problem of the reliability of the single interpolated limen. In the bottom row of the table are given the probable errors of estimation of constant process limens from interpolated limens. They indicate the probable deviation, in terms of grams, of an estimated or interpolated limen from the constant process value. It may be remarked that this deviation for the L_{r4} and L_{c4} interpolations is of the order of only 0.3 grm.

Another comparison was made between the constant process and interpolation methods by computing the P.E. of each constant process limen and then obtaining the departures, in terms of these P.E.'s, of the limens interpolated from the constant process. Boring's formula for $P.E._L$, not to be confused with that for $P.E._{av}$, was used.[17] The actual gram differences between the interpolated and constant process limens were first obtained and then converted into $D/P.E._L$. Fig. 2 exhibits the results graphically. There is a separate histogram of the departures of each of the four kinds of interpolated limens. The abscissa unit is 0.1 $P.E._L$ while the ordinates are frequencies of departures. It may be noted that the L_{r4} and L_{c4} limens depart least from the constant process. The L_{c4} distribution, furthermore, exhibits the greatest normality and the least variability, as well as equal numbers of positive and negative divergencies.

TABLE II

CORRELATIONS BETWEEN CONSTANT PROCESS AND INTERPOLATED LIMENS

	L_{r2}	L_{c2}	L_{r4}	L_{c4}
r	.962	.960	.987	.987
$P.E._r$	.0050	.0053	.0017	.0017
$P.E._{est.L}$	.4992	.5382	.3328	.3263

Frequencies of divergencies falling between various P.E. limits are shown in Table III. Since there are 100 of each variety of limen these figures are percentages as well as actual cases.

The values in Table IV below are also in terms of $P.E._L$ and allow of some comparison between the variability of the deviates and that of the constant process. Q has been taken as the measure of the variability of the deviates because, practically, we are interested in the variability of the single limen. As might be expected, the medians fall close to zero deviation from the constant process.

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[17] E. G. Boring, this JOURNAL, 27, 1916, 317.

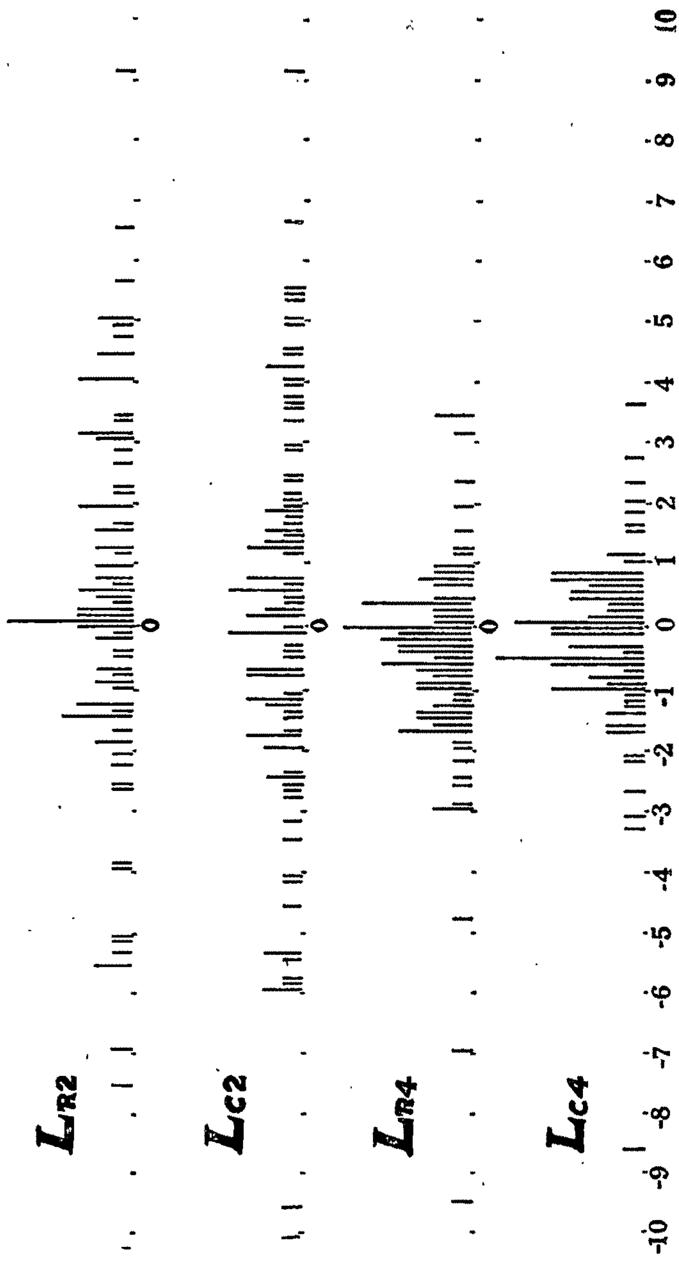

FIG. 2. DEPARTURES OF INTERPOLATED LIMENS FROM THE CONSTANT PROCESS IN TERMS OF P.E. OF THE LATTER.

TABLE III

FREQUENCIES OF VARIOUS DEVIATIONS FROM THE CONSTANT PROCESS

P.E.L	L_{r2}	L_{c2}	L_{r4}	L_{c4}
within ± 0.5	24	15	40	41
1.0	38	29	65	74
1.5	49	44	77	83
2.0	58	57	87	90
2.5	62	64	90	94
3.0	66	69	94	96
3.5	76	72	97	98
4.0	78	76		99
4.5	85	83		
5.0	87	85	98	
5.5	92	91		
6.0	95	96		
6.5				
7.0	97	97	99	
7.5				
8.0	98			
8.5				
9.0				100
9.5	99	98	100	
10.0		99		
10.5	100	100		

TABLE IV

THE QUARTILES AND SEMI-INTERQUARTILE RANGES OF DEVIATIONS FROM THE CONSTANT LIMEN.

	L_{r2}	L_{c2}	L_{r4}	L_{c4}
Q_1	−1.43	−1.70	−1.20	−0.73
Median	0.09	0.17	−0.28	0.00
Q_3	2.03	1.85	0.30	0.55
Q	1.73	1.78	0.75	0.64

Both Tables III and IV show interesting differences in the dispersion of the four kinds of deviates. As noted above, the distributions L_{r4} and L_{c4} exhibit least dispersion, L_{c4} being somewhat less dispersed and more symmetrical than L_{r4}.

It seemed desirable to get some reply to one other question which would have value as a fairly general check on the other results. How do the differences between the constant process limens of the same *S*, obtained at different times to be sure but otherwise under fairly comparable conditions, compare with the corresponding departures of the interpolated limens. We applied this question to one or two pairs of constant process limens of every one of the 15 *S*s. The comparison is shown in Table V by means of 38 ratios for each of the four interpolation methods, and aims to be fairly representative of all the data. Column 1 refers to the observed frequencies given in Table I. Column 2 gives the gram differences between the members of pairs of the constant process limens, each pair being of a particular *S* and both members being either upper or lower limens. The difference (L′ − L″) for the first pair of constant process limens, *e.g.* is 2.44 grm. The remaining columns contain the

TABLE V

DIFFERENCES BETWEEN CONSTANT PROCESS LIMENS OF THE SAME *S* COMPARED WITH CORRESPONDING DEPARTURES OF INTERPOLATED LIMENS

*O*s Freqs. cf. Table I	L′ − L″ in grams	D of L_{r2} L′ − L″	D of L_{c2} L′ − L″	D of L_{r4} L′ − L″	D of L_{c4} L′ − L″
4	2.44	.34	.13	.02	.07
5	“	.23	.42	.03	.03
6	.51	1.47	1.22	.14	.04
7	“	1.75	2.12	.06	.22
14	1.75	.18	.16	.03	.13
15	“	.09	.18	.36	.25
16	.67	.76	1.18	.01	.04
17	“	.57	.36	.01	.04
24	1.24	.33	.22	.15	.06
25	“	.62	.70	.21	.02
26	3.46	.22	.23	.00	.03
27	“	.10	.05	.11	.05
34	2.01	.49	.59	.10	.18
35	“	.12	.02	.04	.03
36	.21	7.72	7.81	.57	.57
37	“	.05	.52	1.14	1.29
38	2.16	.04	1.67	.07	.08
39	“	.50	.52	.28	.32
50	.91	1.17	.68	.09	.04
51	“	.86	.67	.11	.13
52	.41	1.32	1.10	1.12	1.24
53	“	1.98	1.88	4.40	.32
54	.36	3.25	3.50	.06	.39
55	“	.69	.61	2.28	2.47
56	1.91	.19	.13	.11	.00
57	“	.11	.06	.12	.12
69	.32	4.56	4.91	1.38	1.16
70	“	.00	.13	1.81	1.63
71	.89	.88	.94	1.15	1.04
72	“	.73	.69	.25	.20
73	.23	8.80	8.75	1.57	1.57
74	“	1.83	1.57	.04	.04
85	1.45	.12	.23	.02	.00
86	“	.81	.63	.20	.04
88	.47	.06	.15	.02	.09
89	“	2.19	1.96	.15	.04
98	2.03	.18	.09	.07	.07
99	“	.00	.06	.34	.20
Per cent less than L′ − L″		71	68	79	82

ratios of the gram deviation of the interpolated limen (D) to the gram difference between the two constant process limens (L′ − L″). The 0.34 at the top of column 3, *e.g.* means that the L_{r2} interpolated limen departs from the constant process limen (both based on the No. 4 set of observed frequencies) only .34 as much as the two constant process limens (based respectively on No. 4 and No. 5 sets of observed frequencies) depart from each other. The second figure in column 3, viz., 0.23, is to be similarly interpreted: it refers to the interpolational deviation from the constant process limen based on observed frequencies No. 5 *i.e.* on the second member of the first pair of observed frequencies. These ratios, then, show how much greater or less than L′ − L″ the deviations of the interpolated limens are.[18]

The columns of this table are not averaged partly because they are, and properly, not normal. The figures of the bottom row, however, are the relative frequencies with which the interpolational deviates are less in value than the difference between the two constant process limens of the given *S*. The first figure in the bottom row, for instance, means that 71% of the L_{r2} limens departed less from the constant process than did two constant process limens from each other, even though the latter were of the same *S* under similar environmental conditions. The mean of the entire bottom row is 75, which means that 75% of all the departures are less than L′ − L″, the difference between the constant process limens. The mean for the L_{r2} and L_{c2} values is 69.5 as compared with 80.5 for L_{r4} and L_{c4}.

The logic of Table V is this: L′ and L″ are supposed to be comparable for they are supposed to be representative for the subject and to differ because of minor 'chance' changes in conditions. The difference between them should not be significant. Since the difference L′ − L″ ought not to be significant, then those deviations of interpolated limens from constant process limens which are of the same order or less than L′ − L″, ought not to be significant. As mentioned in the preceding paragraph,

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[18] Culler has recently recommended a new formula for P.E.$_L$ (*Psychol. Monog.*, 35, No. 2, 1926, 87 f.) which had not been available to the writer during the course of this investigation. Quite aside from the question of the relative merit of different formulae, it may be remarked that, when the P.E.'s of the constant process limens are recalculated with this formula, the values are roughly 2.4 times as great as with the formula suggested by Boring in 1916. The ratio of the results of the two methods does, indeed, make some approach to uniformity. Thus, when the P.E. of the one method is divided by the P.E. of the other, the smallest quotient in the writer's 100 instances is approximately 2.0 and the largest 2.7, while the mean ratio is 2.39 with 0.12 as P.E. of the *single* ratio. Therefore, in the present data, the order of magnitude of the one P.E. usually can be obtained from the other by simply multiplying or dividing by the constant 2.4. Of course, as regards the present investigation, P.E.'s of such increased magnitude may substantially increase the conservatism of the principal conclusions. Referring to the L_{c4} distribution in Fig. II, for instance, about 94% of the interpolated limens would fall within ± 1 P.E. of Culler's as compared with the 74% indicated in the figure (or in Table III).

75% of all the interpolated limens were found to deviate from the constant process by less than the amount L′ − L″. This result seems remarkable, especially when one recalls that the interpolation was performed without any weighting.

There are certain objections which might be raised against the procedure underlying Table V. No account is taken of differences in variability; the actual gram differences only are considered. Then, too, it should be recalled that about 60% of the observed frequencies given in Table I were obtained by summing the results of two sets of 50 series each.[19] These sets of 50 were presumably obtained under comparable conditions, but, even so, summing them might increase the difference between constant process limens figured from them. That would result in a relative decrease in the departures of the interpolated limens. Since the other 40% of observed frequencies in Table I were not obtained in this way, however, they serve as a check upon this sort of error. The ratios in Table V corresponding to these latter 40%, *i.e.* to Nos. 36-72, do in fact run somewhat larger than the others, a condition which, while it tends to support the objection mentioned, might well be due nevertheless to sampling variations.

SUMMARY

(1) Limens (L_{r2}, L_{c2}) interpolated from two-fifths of data regarded adequate to the constant process, and when little attempt is made to secure those portions of the data which would yield the most accurate limen, often depart widely from the constant process.

(2) Limens (L_{r4}, L_{c4}) interpolated from four-fifths of the data, and when some attempt is made to secure the most favorable four-fifths, usually approximate quite closely the constant process.

(3) Interpolation (L_{c4}) assuming normality and utilizing four-fifths of the data appears the most accurate of all the tested methods; in other words the writer's results favor the use of formula [4].

(4) If these results are typical of data from lifted weight and other experiments, it would seem that the labor of computing constant process limens, reduced though it certainly has been, is quite unjustified, at least for many situations.

These provisional results suggest other problems which seem never to have been solved empirically. It seems theoretically possible, for instance, that the number of the stimuli might legitimately be still further reduced from five to four, three or even the traditional two. If such reduction is legitimate, the experimentation (as well as the computation) for a limen could be substantially reduced. The requisite number of series and the changed attitude of the subject with the reduced stimuli, would undoubtedly be important considerations. The present study was designed in part to give preliminary information on this question, *i.e.* to indicate whether an experimental investigation of it would be likely to prove worthwhile. Since the L_2 methods were applied to observed frequencies taken fairly at random within a wide range, we might expect that actual experimentation with only two comparison stimuli would yield comparable frequencies within that range. But considerable preliminary work as well as numerous extra trials during the experiment over a distinctly wider range than that of the two selected stimuli, might well be found necessary; the former to assure the selection of two suitable stimulus values, and the latter as *Vexierversuche* as well as to provide some extreme and hence easy judgments to give the subject confidence and orientation. Yet even assuming no difficulty along such lines the preliminary indication is negative; the use of two comparison stimuli with 100 series would probably be inadequate, for the interpolations based upon them were found inadequate when five comparison stimuli were actually employed experimentally. This does not imply, of course, that if those two frequencies most favorable for interpolation were selected from five observed, that the result would be unsatisfactory.

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[19] Cf. footnote 13.

The L_4 methods were applied to 'selected' data, but really there was not in these cases much opportunity for selection (four adjacent observed frequencies were selected out of the five), and also the data varied widely with respect to normality and precision. The writer feels, therefore, that the results obtained from the L_4 methods give a useful preliminary indication of what to expect from the experimental use of two comparison stimuli with 200 series; the indication is that the investigation would prove worthwhile, and the results interesting, in any event. The L_4 methods obviously give favorable indications with respect to the use of four comparison stimuli with 100 series. The whole question is one which demands actual experimentation with different numbers of comparison stimuli. The results, as do ours, would have a bearing upon the applicability of tables like Fullerton and Cattell's.[20]

A quite different problem is that of an adequate measure of variability of an interpolated limen.[21] Computing the P.E. on the basis of the data used in interpolating the limen, would be easy and possibly adequate.

There are a number of shorter methods for computing limens which might be compared with the constant process, for instance, Wirth's[22] and Spearman's[23] methods and the quartile method mentioned by Brown and Thomson.[24] Applying such methods to the observed frequencies after the latter had been simply weighted might increase the value of the results.

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[20] A. S. Fullerton and J. McK. Cattell, *On the Perception of Small Differences* (Publications of the U. of Penn., Phil. Series, No. 2), 1892, 16.

[21] Brown and Thomson, *op. cit.*, 60 f.

[22] R. Pauli, *loc. cit.*

[23] C. Spearman, *loc. cit.*

[24] Brown and Thomson, *op. cit.*, 60 f.

FOOD *VS.* SEX AS INCENTIVES FOR MALE RATS ON THE MAZE-LEARNING PROBLEM[1]

By CALVIN P. STONE and MARY STURMAN-HUBLE, Stanford University

No doubt every one who has handled rats on a maze problem has wished for an experimental technique that would effectively impell them to disregard irrelevant objects on the floor, the alley walls, or maze covering and to attend more strictly to the business of learning and running the maze. Hunger and the food-reward are serviceable activators of movement, yet one cannot overlook the fact that rats in a state of hunger are frequently distracted by irrelevant stimuli in the maze. Because of this the reliability of one's criteria of learning is considerably lowered.

In this preliminary report our interest centers chiefly in determining whether sex-hunger and an opportunity to copulate at the end of each run will cause the rat to attend more strictly to the business of learning and rapidly tredding the maze than a state of food-hunger and the opportunity to appease this appetite.

Animals. Albino rats were used because previous experiments[2] assured us that an adequate supply of receptive females could be made available for each day's work. This can be assured for the mouse and rabbit, as well, but for our purpose the rat is preferable because more is known of his maze performances than of either of the other animals. Food and sex groups consisted of 9 animals each.

The following brief report deals with animals approximately one year old. Preliminary experiments on males of other ages, however, have been run. Before being placed on the experiment each male was tested for copulatory ability with a receptive female, and at irregular intervals prior thereto had been given opportunities to copulate many times. Thus for sex as well as for food the animals demonstrated the presence of appetite and the ability to satisfy it when opportunity was afforded.

Diet. It is of paramount importance in an experiment of this kind to have a well balanced diet not only for the males of the experiment but also for the colony of females from which each day's quota of receptive females

McCOLLUM DIET

Whole wheat ground fine	67.5
Casein	15.0
Powdered whole milk	10.0
Calcium carbonate	1.5
Sodium chloride	1.0
Butter fat	5.0
	100.0

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[1] Financial support for this study was granted by the National Research Council through the Committee for Research on Sex Problems.

[2] C. P. Stone, The awakening of copulatory ability in the male albino rat, *Amer. J. Physiol.*, 68, 1924, 407-424; Delay in the awakening of copulatory ability in the male albino rat incurred by defective diets: I. Quantitative deficiency, *ibid.*, 69 , 1924, 195-224.

is selected in order that sexual aggressiveness and sexual receptivity, respectively, will be at high level. Our diet consisted of the McCollum mixture which has proven eminently satisfactory in other experiments on sexual behavior in our laboratory.[3]

Mazes. The animals were run on two types of mazes. (1) Maze A is a simple T-maze allowing only one turn from the starting pathway either to the right or to the left. The distance from start to exit is 48 in. (Measurements taken down the center of the alleys with right-angled turns.) The simplicity of this maze permits the male to reach the reward in 2 or 3 sec. after he has learned to go to the proper side. Since each trial involves a choice of right or left turn, however, several trials must be run before the male learns which turn to take. (2) Maze B is a close approximation of a maze described by Warden.[4] In this maze the distance from start to exit, as measured along the true pathway, is 319 in. The home box was used in our experiments only as a vestibule leading to a portable cage placed just outside of the maze. In the portable cage food or the receptive female was kept. This maze is one of only medium difficulty for rats aged 30 days or over and can be traversed by large animals in a minimum of 5 or 6 sec. In mazes of this type the errors in learning are by no means directly proportional to the number of blind alleys, as is more nearly the case in a multiple T-maze in which a definite choice is involved at each unit of the maze.

Trials. Every animal was given 1 trial per day on the simple maze until 15 trials had been completed. Immediately following this he was given 1 trial daily on the more complex maze until 15 trials had been run.

Rewards. Individuals of the food group were given an opportunity to eat for a period of 20 min. in their portable cages set aside at the end of each run. With Maze A the food thus consumed was the daily ration. Toward the middle of the experiment with Maze B, however, an addition of 2 grm. per animal was alloted in the home cages at the end of each day's work. This was found necessary to offset a small but steady drop in the weight of the food group after the early days of the experiment. Approximately 30 min. after the individuals of the food group were returned to the home cage, a receptive female was introduced into their cages to permit each male to copulate 3 or 4 times and thus, on the physiological side, balance the daily sexual experience of the sex group. This sex activity in the colony room, being separated in time by a half hour or more from the runs on the maze, we believed would not react on the learning process as an additional incentive for running the maze. In one other preliminary experiment with 8 animals this copulatory experience in the home cages was not permitted the food group. Their results do not appear to be significantly different from those of this experiment.

Every animal of the sex group had an excess of food in its home cage throughout the experiment and could eat *ad libitum* according to the dictates of appetite. After completing the run in the maze he was permitted to copulate from 3 to 5 times in the portable cage. He was then returned to the home cage, there to remain until the next day.

From the foregoing description it may be inferred that our experimental technique, so far as handling incentives is concerned, is more complicated than that used by Moss,[5] Simmons,[6] or Tsai.[7] Every male of the food and sex groups was required to demonstrate sexual aggressiveness by the daily copulatory test. Whatever deleterious influence, if any, might arise and be reflected in the activity of the sex group as a result of daily sex activity, would also affect the food group which had in its home cage an equal amount of sexual activity. Both groups were kept at a high nutritional level, as our weight records will show, to insure that there would not be a

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[3] C. P. Stone, *ibid.*, 68, 1924, 407-424.

[4] C. J. Warden, The distribution of practice in animal learning, *Comp. Psychol. Monog.*, 1, 1923, No. 3, 1-64.

[5] F. A. Moss, Study of animal drives, *J. Exper. Psychol.*, 7, 1924, 165-185.

[6] R. Simmons, Relative effectiveness of certain incentives in animal learning, *Comp. Psychol. Monog.*, 2, 1924, No. 7, 1-79.

[7] Chiao Tsai, The relative strength of sex and hunger motives in the albino rat, *J. Comp. Psychol.*, 5, 1925, 407-415.

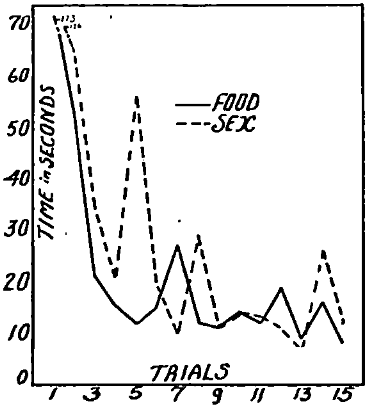

FIG. 1. Median time for daily runs by food and sex groups on T-maze.

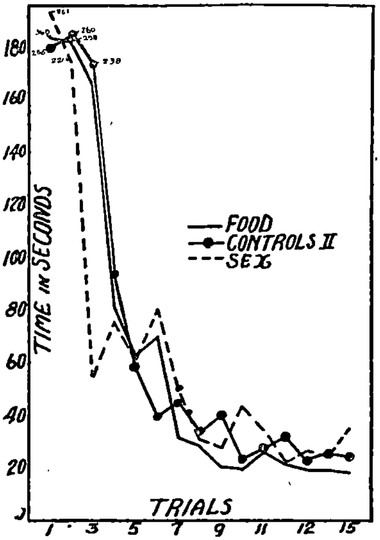

FIG. 3. Median time for daily runs on complex maze. Controls II is a second food group with weights reduced below the regular food group of the experiment.

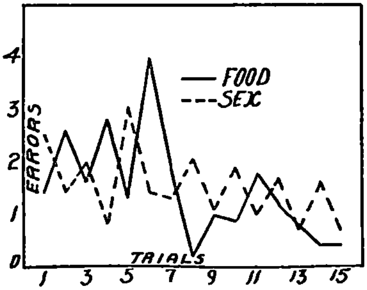

FIG. 2. Mean number of errors for daily runs on T-maze.

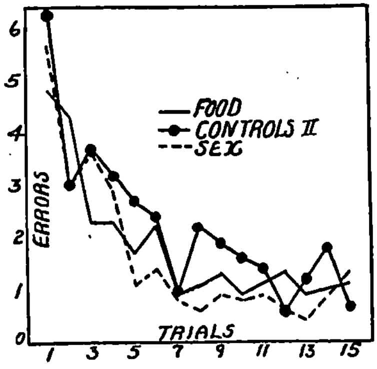

FIG. 4. Mean number of errors for daily runs on complex maze.

waning of the sexual drive as a result of chronic inanition. Only a small number of copulations per day was allowed each animal in order that we might be reasonably sure, in the light of previous experiments in our laboratory, that potency would not disappear toward the end of the experiment.

Criteria of Learning. The time records consist of the total time from the rat's initial start until his nose reached the doorway of the home-box. Although the records do not include delays in leaving the starting position or hesitation in entering the home box at the end of the runs, static time was not removed.

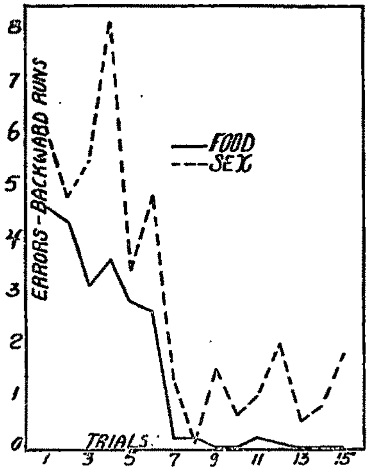

FIG. 5. Mean number of pathways retraced in daily runs on complex maze.

An error consists of any deviation from the true pathway, *i.e.* an entrance into a blind alley as the animal was going away from or back to the starting point. To commit an error the animal must progress beyond the entrance to the alley with two-thirds or more of his body length, tail excluded. Backward turns on the true pathway are included in the error records of Maze A, but not in Maze B. For the latter a separate record of the pathways traversed on backward runs was made.

RESULTS

Maze A (*T-Maze*). The data for the T-maze are summarized in Table I. Because certain animals of each group made erratic runs from day to day the means are less adequate representative of the average time per trial than the medians. The latter were selected for the learning curves of Fig. 1. As will be observed upon considering the graphs of Fig. 1, the general trend of the time curves for the food and sex group is quite similar. Owing to the small number of cases, however, it is impossible to go beyond rough comparisons of the general trends of these curves. Fig. 2 gives the mean error curves for daily runs. Here although the curves are quite similar, there appears to be a very slight superiority on the part of the food group in learning the correct route. The median number of errors for the food group is consistently lower than that of the sex group as may be seen from Table I. Whether the incentive is responsible for this slight superiority in learning by the food group or whether there is a slight intellectual superiority of this group over the sex group we cannot say. However, the probability of the latter being the case is somewhat increased from a consideration of the results of experiment 2, in which the food group became the sex group and the sex group the food group.

Maze B (*Complex Maze*). The summarized data of experiment 2 are given in Table II. One animal of the sex group was excessively slow throughout the 15 trials, and other animals of both groups made excessively slow runs from time to time, hence the mean time for daily runs is again less adequate than the medians for purposes of comparison. Fig. 3 graphically presents the trends in time of successive runs for the two groups. As will be seen the curve for the food group drops to a slightly lower level than that of the sex group. Individual animals of the latter, however, were equal to the best of the food group.

As to errors, the situation for the two groups is the reverse of what was found for time although the difference is very slight and probably insignificant (Fig. 4). In the previous experiment, it will be recalled, these same animals were slightly superior in the reduction of errors, although at that time food was the incentive.

The records for retracing show that there was very little difference between the two groups as a whole. One animal of the sex group was an inveterate retracer, however, and his record makes the mean number of alleys retraced by the sex group slightly higher than the mean of the food group (Fig. 5).

Weight Records. In Tables I and II mean weights for the two groups of this experiment are given for each day. Inspection of these data show that the sex group, which was allowed to feed in the home cages according to the dictates of appetite, varied little from day to day. The food group, however, continually lost a little weight due to the fact that they did not consume sufficient food in the 20-min. feeding period to hold them at the high weight level maintained under conditions of unrestricted feeding. Their physical condition, nevertheless, was excellent.

In our preliminary experiments on this problem we foresaw the desirability of inquiring into the relation of speed of running by very heavy animals as compared with similar animals whose weight was quickly dropped at the beginning of the experiment from 15 to 20 percent and thereafter maintained at this lower level. A preliminary experiment on this point was made. Ten one year old males starting with a mean weight of 275 grm. were dropped during the trials preliminary to Maze B to a level of 220 grm. where by careful dispensation of food to each animal according to his daily weight he was held practically at maintenance throughout the experiment. Their time and error records are graphically represented (Controls II) in Fig. 3 and 4. It was found that after the very erratic first trials very little difference in the time and error curves appeared. These slight differences are probably insignificant.

CONCLUSIONS

Considering all criteria together it would seem that one is justified in drawing the tentative conclusion that food and sex as used in this experiment are approximately equal as incentives to maze learning and running by one year old male rats. As everyone knows, however, mathematical comparison leading to more exact conclusions demand that a number greater than 18 individuals be submitted to the experiment. This we expect to do when a maze of *high reliability* with which we are now experimenting has been satisfactorily proven.

TABLE I
Summary of Data for Maze A (T-Maze)

Criteria of Comparison	Group	Trials														
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Mean Time per run	Food	121	101	56	55	86	42	15	31	33	58	66	8	25	17	23
	Sex	124	64	56	26	83	37	34	53	16	35	20	65	15	25	15
Median Time per run	Food	173	52	21	15	12	15	27	12	11	14	12	19	9	16	8
	Sex	126	64	34	21	56	20	10	29	11	14	13	11	7	26	12
Mean number Errors	Food	1.4	2.6	1.6	2.8	1.3	4.0	1.9	.2	1.0	.9	1.8	1.4	0	.4	.4
	Sex	2.5	1.4	2.0	.8	3.1	1.4	1.3	2.1	1.1	1.9	1.0	1.7	.7	1.6	.7
Median number errors	Food	2	1	0	0	1	0	1	0	0	0	0	0	0	0	0
	Sex	2.5	1	2	1	3	1	0	2	0	2	1	0	0	1	0
Mean weights	Food	300	287	289	285	282	281	282	275	277	277	278	275	275	267	270
	Sex	309	298	300	294	293	288	292	285	292	288	290	289	291	286	286

TABLE II
Summary of Data for Maze B (Complex Maze)

Criteria of Comparison	Group	Trials														
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Mean trial per run	Food	640	536	197	367	240	113	35	32	25	22	26	20	22	20	18
	Sex	819	80	498	411	122	169	54	35	63	86	59	132	58	64	119
Median time per run	Food	300	253	166	81	63	70	31	28	20	19	26	21	19	19	18
	Sex	801	175	54	75	61	80.5	50	30.5	27	43	34.5	21.5	26.5	24	34.5
Mean number errors per run	Food	4.8	4.3	2.3	2.3	1.7	2.2	.9	1.1	1.3	.9	1.1	1.3	.9	1.0	1.1
	Sex	5.7	3.0	3.6	2.9	1.1	1.4	.8	.6	.9	.8	.9	.6	.4	.9	1.3
Mean number alleys retraced on true pathway	Food	4.6	4.3	3.1	3.6	2.8	2.6	.2	.2	0	0	.2	.11	0	0	0
	Sex	6.1	4.8	5.5	8.1	3.4	4.8	1.3	.1	1.5	.6	1.0	2.0	.5	.8	1.8
Mean body weight	Food	290	280	278	275	270	264	258	258	260	251	253	250	252	251	250
	Sex	299	300	294	299	292	288	298	299	303	297	294	297	302	295	297

INTERESTS OF COLLEGE STUDENTS[1]

By M. O. Wilson, University of Oklahoma

The Problem

It is the main purpose of this study to analyze the interests of college students, as indicated by their selection of courses and their achievement in these courses, in order to characterize, if possible, certain types of students with respect to their chosen fields of intellectual activity. One of the most important problems of undergraduate education is to fulfill the needs of students with respect to their different abilities and interests and to turn out men and women who are prepared to meet the various needs of society. Therefore, such a study as the one here described should be of some practical value to the maker of the college curriculum, the college administrator, the vocational guidance expert, and the teacher, in so far as the interests of college students thus expressed reveal the needs of the students themselves and of society in general.

Previous Investigations

One of the earliest studies dealing with the phase of the problem outlined above was that of Strayer and Thorndike.[2] They made a statistical analysis of the courses actually taken for the A.B. degree by 391 men students graduated in 1909 at different American colleges where various degrees of freedom in selection of courses were permitted. Among other tendencies, their results show (1) that specialization toward a profession will occur when it is permitted, (2) that free election (but within non-professional courses) increases specialization outside of languages and literature, and (3) that in certain colleges specialization by the candidates for the A.B. degree is chiefly in languages and literature, a specialization artificially cultivated by the requirements for entrance and graduation.

In determining the relation between courses taken and achievement on the one hand and securing the *cum laude* degree in the professional schools on the other, Lowell in 1911 found a tendency for the future *cum laude* men to take more courses in the classics and mathematics than the "plain" men, while the latter showed a stronger tendency to select subjects reputed to be less difficult.[3]

In 1912, Thorndike selected some 300 college junior students and had them rate their own interests in certain school subjects, first at the elementary school level and again at the high school level.[4] He then correlated their rankings with their achievement and secured a coefficient of 0.91. From this result he concluded that a person's relative interests are quite permanent and that they are "extraordinarily accurate symptoms of his relative capacities." This study was duplicated by Bridges and

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[1] This article is part of a Ph.D. dissertation submitted by the writer to the Department of Psychology, the University of Chicago. The study was carried out under the direction of Professor L. L. Thurstone, to whom many obligations are gratefully acknowledged.

[2] G. D. Strayer and E. L. Thorndike, *Educational Administration*, Section 17, 1913.

[3] A. L. Lowell, College students and professional training, *Educ. Rev.*, 42, 1911, 217-223.

[4] E. L. Thorndike, Permanence of interests and their relatiom to abilities, *Pop. Sci. Monthly*, 81, 1912, 449-456.

Dollinger, in which they found the correlation to be much less.[5] They took issue with Thorndike as to the relation between capacities and interests. In 1917, Thorndike reported an extension of this study in which he confirmed the previous results.[6]

In 1917 Davis investigated the question as to what were the courses pursued by members of Phi Beta Kappa.[7] He found that at the time the study was made and for the group concerned, English, classics, and mathematics still constituted approximately two-thirds of the material pursued by high school pupils whose intellectual superiority was later evidenced by election to Phi Beta Kappa. Further, when these students reached college, there was a tendency to substitute modern languages for the classics, and the social sciences assumed leadership in the list of subjects taken.

In the same year King made a study similar to that of Thorndike, based on 140 college students.[8] The correlation between achievement in college and interest in college subjects, as indicated by the students' own rankings, was 0.73. But, when this correlation was computed for interest and achievement throughout the elementary, high school, and college courses, the coefficient dropped to 0.47.

In 1923, Thorndike had his advanced students in education, mainly teachers and administrators, rank the high school subjects according to their interests in them.[9] He found that English, literature, history, and science were the most liked, and Latin the least. The men favored history and science, and the women literature.

Stoy made a study in 1925 of the relation between preferences of freshman students and scores made on certain psychological entrance examinations.[10] He found that mathematics and exact science students excelled in the quantitative tests, such as arithmetic and estimating, and the exact science students did poorly in the artificial language tests. The foreign language students and literature students excelled in opposites and grammar. The foreign language group excelled also in artificial language and the literature group excelled in proverbs, but did poorly in arithmetic. The history group excelled in no particular tests but did poorly in arithmetic, artificial language, proverbs, grammar, and reasoning.

METHOD

The Criteria of Interest. For the purpose of this study two criteria of interest are set up, namely, selection of courses and relative achievement. It is assumed, according to the first of these, that when a student chooses a special field of intellectual activity in which to make an intensive study, this is a strong indication of his desires and needs as he senses them at the time. It is further assumed that on the whole the selection of courses in other fields for the purpose of getting some "acquaintance with the intellectual wealth of the world and with the main human problems of the present day" is also an expression of interest in so far as prevailing administrative regulations permit free election.

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[5] J. W. Bridges and Verona Dollinger, The correlation between interests and abilities in college courses, *Psychol. Rev.*, 27, 1920, 308-314.

[6] E. L. Thorndike, Early interests: their permanence and relation to abilities, *School & Soc.*, 5, 1917, 178-179.

[7] C. O. Davis, Courses pursued by members of Phi Beta Kappa, *School & Soc.*, 5, 1917, 686-690.

[8] I. King, Permanence of interests and their relation to ability, *School & Soc.*, 6, 1917, 359-360.

[9] E. L. Thorndike, The interests of teachers in studies during their high school course, *School & Soc.*, 17, 1923, 419-420.

[10] E. G. Stoy, The relationship of interests in high school subjects and ability in certain psychological tests. An unpublished M. A. Thesis. Typewritten copy in the University of Chicago Library, 1925.

The second criterion assumes that in general the student will put forth his best efforts in those subjects in which he is most interested. It is granted that the exceptions to this assumption are quite numerous, but it is believed to be true in a broad sense. It seems to be justified by most of the investigations referred to in the previous section.

In order to determine further the validity of this assumption an attempt was made to secure additional information concerning the "theory of two factors of intelligence" as supported by Spearman. If, as Spearman maintains, there is a common fundamental function or group of functions underlying the various intellectual activities of students, then, the validity of the achievement criterion is strengthened. But, if, as Thorndike maintains, intelligence is made up of specific abilities, then, this must be allowed for in the use of this criterion. Two methods of attacking this problem were used.

(1) For the first method, the coefficients of correlation between achievement of the students in the various departments were obtained. These coefficients were then treated statistically according to a procedure outlined by Spearman and Holzinger, which is somewhat as follows.[11] The criterion for determining the extent to which a set of data conform to the general ability theory is expressed by the relation

$$\frac{r_{13}}{r_{14}} = \frac{r_{23}}{r_{24}} \qquad [1]$$

where the subscripts represent any four of the abilities in question and the r's the coefficients of correlation between them. Then, for the true correlation or those derived from some 'entire population' the relation may be expressed thus:

$$F = r_{13}r_{24} = r_{23}r_{14} = O. \qquad [2]$$

Now, if the 'entire population' is replaced by a sample and several abilities are correlated, we have

$$\text{Mean } F = O\ (\pm \text{ the error of sampling}) \qquad [3]$$

The formula for finding the error of sampling is:

$$\sigma_F = 2\sqrt{\frac{r^2(1-r)^2}{N} + \frac{S^2}{N}} \qquad [4]$$

where S = standard deviation of all r's, r = mean of all r's, and N = population. Applying these formulae to the correlational coefficients of this study the following results were obtained as compared with the theoretical values needed to bear out the hypothesis:

Status of Value	Mean of all F's	Standard Deviation of all F's	Percentage of F's falling within ± 1 Theoretical S. D.
Theoretical	0.0000	0.0327	68.26
Observed	0.0043	0.0266	70.00

Considering the closeness with which these three values—the mean, the standard deviation, and the percentage of F's falling within the theoretical measure of dispersion — approximate those demanded by the normal probability curve, it is believed that the error is reasonably well within the error of sampling. Therefore, it may be concluded that the data obtained by intercorrelating the achievement in the different departments tend to support the concept of general ability as proposed by Spearman.

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[11] C. Spearman and Karl Holzinger, Note on the samplimg error of tetrad differences, *Brit. J. Psychol.*, 16, 1925, 86-88.

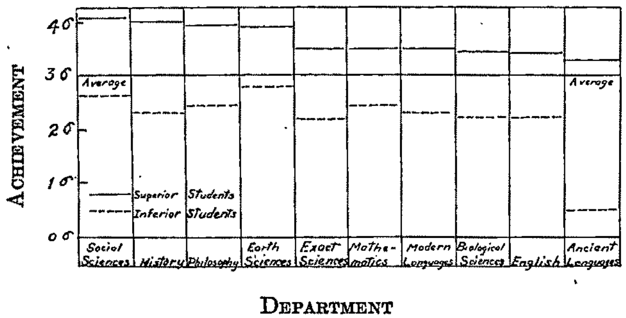

FIG. 1. RELATIVE ACHIEVEMENT OF EXCEPTIONAL EXACT SCIENCE STUDENTS

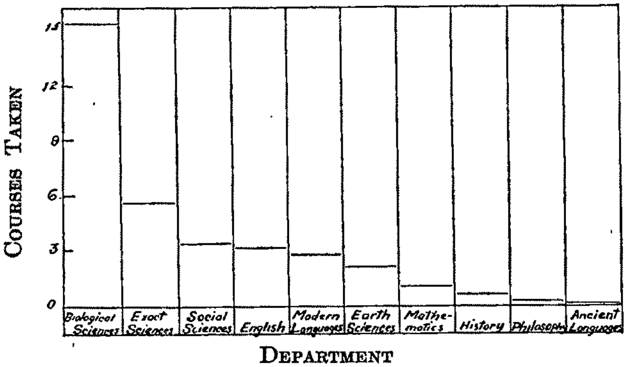

FIG. 2. AVERAGE NUMBER OF COURSES TAKEN BY BIOLOGICAL SCIENCE STUDENTS

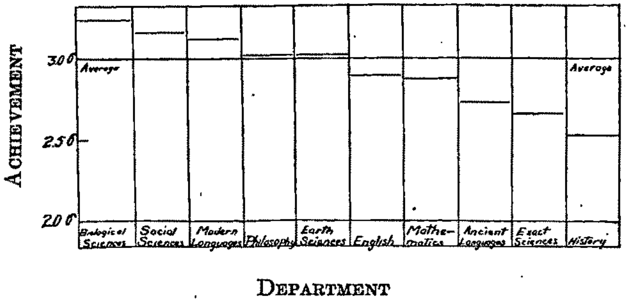

FIG. 3. RELATIVE ACHIEVEMENT OF BIOLOGICAL SCIENCE STUDENTS

(2) For the second method an analysis was made of the achievement of exceptional students. In a given departmental group the students were ranked with respect to achievement in their major department. Then all students in the upper fifth and all those in the lower fifth were selected. These two groups were compared with respect to their achievement in each department. Fig. 1 shows the relative achievement of exceptional exact science students. It shows that the student who is poor in scholarship in his own department is inferior in every other department and likewise the student who is superior in his own department is superior in every other department.

The same procedure was used for each of the other departmental groups and this tendency was found to be true for each group. The results agree with those obtained by Blackhurst,[12] who found in a study of school grading that, after leaving out such subjects as music and physical education, the bright pupil in high school or college will stand high in all subjects, the dull low in all subjects, and the intermediate around a corresponding point in all subjects.

It may be concluded that in so far as academic courses in college call forth the best efforts of the student and to the extent that teachers' marks are discriminative as to ability, there exists a general ability or disability on the part of students for meeting the intellectual demands of the various types of subject matter. It may be assumed on the basis of this conclusion that differential scholarship in the various departments means differential interests and that scholarship varies roughly in direct relation to interest.

The Data. The data for this study were provided by the records of 758 students in the College of Arts, Literature, and Science, of the University of Chicago. Only students who had completed the baccalaureate degree and only those who had done all of the four years' work in residence were included.

The various departments of instruction were grouped into fairly homogeneous groupings for convenience as follows: mathematics, exact sciences, biological sciences, earth sciences, ancient languages, modern languages, English, history, social sciences, and philosophy.

In order to secure comparable measures of achievement in the various departments all teachers' marks were transmuted to 'standard scores,' that is, each mark was expressed in terms of the standard deviation of the distribution of marks in the department in which it was given.

Results

The results of the study are somewhat conflicting. There is a moderate degree of agreement between the two criteria of interest, as is shown in Figs. 2 and 3, which are representative of tendencies in selection of courses and scholarship gained. The average coefficient of correlation between achievement in the different departments and the courses taken is 0.34. It is impossible to give as clearcut and concise a characterization of any departmental group as was desired. On the other hand, there are revealed many interesting and illuminating facts, which it is hoped will be of some interest to the reader. They will now be considered, first taking up those concerned with selection of courses and lastly those dealing with relative achievement.

(1) *Selection of Courses.* Table 1 shows the extent to which students choose the various intellectual fields for intensive study. This table shows the strong tendency of students to select one of the social sciences as a major subject notwithstanding the fact that this study does not include those students who were enrolled in the School of Commerce and Administration and in the School of Social Service Administration.

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[12] J. H. Blackhurst, The normal curve as applied to high school and college grading, *School & Soc.*, 13, 1921, 447-450.

TABLE I

Distribution of students with respect to the departments in which they take their principal sequence

Department	Frequency	Percentage
Social Sciences	176	23.21
English	154	20.32
History	103	13.59
Exact Sciences	85	11.21
Modern Languages	70	9.23
Earth Sciences	66	8.71
Biological Sciences	36	4.74
Mathematics	33	4.35
Ancient Languages	27	3.56
Philosophy	8	1.06
Total	758	99.99

If all the students are divided roughly into 'humanitarians' on the one hand and 'scientists' on the other, it is found that the scientists seek a wider range of experiences than do the humanitarians. These results are represented graphically in Fig. 4. Whether conditions shown here represent the most desirable balance between 'constants' and 'electives' is problematical. Administrators for many years have been concerned with the question as to the extent to which students should 'concentrate' and 'distribute' their efforts during the college course. But, if the writer were to venture a suggestion it would be to the effect that the humanitarians should secure more training in the sciences. If this could be done without changing the requirements but rather by increasing the drawing power of the sciences, then so much the better. As Rigg has pointed out, the sciences certainly do not lack in cultural value.[13]

With the exception of the ancient language group, few of the students select to any extent courses in the ancient languages. The maximum average number of courses selected by any departmental group, excluding the classics students, is less than one-half for each student. All students tend to take more courses in English and the social sciences than are required for graduation.

Fig. 2 is a graphical representation of the way in which biological science students select their courses and is quite typical of the students in general. All curves showing the distribution of courses taken in the various departments by the different departmental groups are high and steep in the vicinity of the major and closely related departments. There is a strong tendency toward specialization despite the fact that the students were doing their work in an institution which allowed a great deal of freedom in the selection of courses.

(2) *Relative Achievement.* When the various departments are arranged in a hierarchy with respect to scholarship gained in each by a given departmental group it is found that the department in which the group are taking the major sequence always stands at the top or near the top, for example, see the biology group in Fig. 2. One condition which prevents the order of courses from being what would be expected in connection with any departmental group is the almost universally high position held by the social science department. This is accounted for largely by the fact that scholarship of the social science students is quite low as compared with that of other groups. The achievement of these students is below the average in every department including their own. This group

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[13] G. B. Rigg, Science and liberal education, *School & Soc.*, 17, 1923, 477-485.

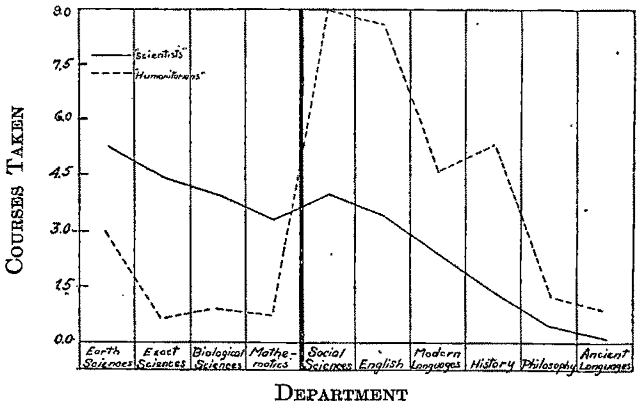

FIG. 4. COMPARISON BETWEEN 'SCIENTISTS' AND 'HUMANITARIANS' IN COURSES TAKEN

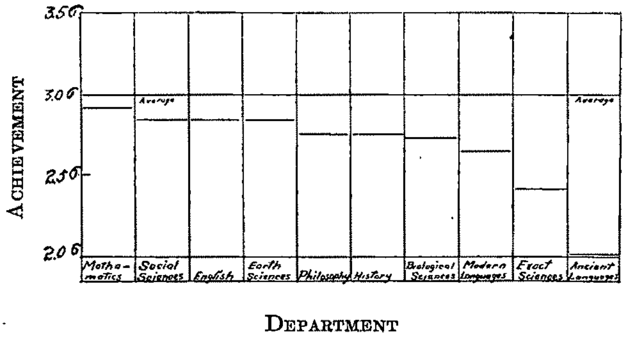

FIG. 5. RELATIVE ACHIEVEMENT OF SOCIAL SCIENCE STUDENTS

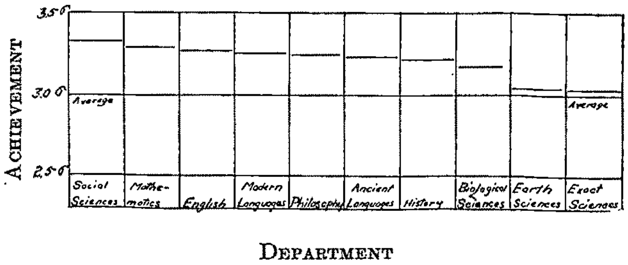

FIG. 6. RELATIVE ACHIEVEMENT OF ANCIENT LANGUAGE STUDENTS

is quite large, therefore, the marks received by other students in social sciences are relatively higher than they would be if the social science students had given stronger competition in securing higher marks in their own department. This low level of scholarship is brought out in Fig. 5.

On the basis of achievement criterion the greatest interest of mathematics students lies in the field of mathematics and its closely related field exact sciences. But apparently there is much less appeal to these students in the realms of history, biological sciences, modern languages, and philosophy. The exact science students gain highest scholarship in exact sciences, philosophy, and social sciences. Their lowest achievement is in ancient languages and earth sciences. The biological science students do their best work in the biological sciences and modern languages while their lowest grades are made in ancient languages, exact sciences and history, as shown in Fig. 3.

Regarding the earth science students there are two interesting facts. First, their absolute level of achievement is very low. It is slightly above the average in their own department but considerably below the average in every other department. Secondly, their achievement in most departments is far below that in earth sciences. It may be concluded from these results that earth science students are inferior in scholarship to the average arts and science student and that their interests are centered largely within their own field.

The ancient language students are the antitheses of the earth science students. They are approximately equal to the average student in two departments and excel the average in every other department, as is shown in Fig. 6. They apparently do not find that the exact sciences and the earth sciences challenge them to do their best, but they gain as high scholarship in these courses as does the average student who takes courses in these departments. Apparently these students are superior students and their interests are quite broad, as indicated by the fact that their achievement is almost equally high in every department.

Much has been said for and against the disciplinary value of the classics. The question is not yet settled, but we are at least justified in assuming that the classics afford a very effective selective agency. Only students with strong intellect would likely survive both the high school and the college course. Davis analyzed the courses taken by 244 Phi Beta Kappa students and concluded that the results did not show that "the greater attention to the study of the classical languages is the sole, or even the chiefest factor in producing superior students." These students were "brilliant and honor students not because they studied more Latin; they may, however, have studied more Latin because they were brilliant."[14] Bolton states in effect that men with strong minds take to the study of languages;[15] they find them interesting. The same type of mind will take to any other studies and success will be achieved in these quite as well. One may conclude from these results that ancient language students are quite superior in scholarship to the average student, and that their intellectual interests extend into practically every division of the arts and science curriculum.

Modern language students make their best grades in modern languages, mathematics, and the social sciences. There seems to be considerably less appeal for these students in all other subjects, although their achievement in general is quite high. They rank second only to the ancient language students in general average in scholarship. English students gain highest scholarship in English, philosophy, and the social sciences. In all other departments their achievement is significantly below that of the average student. One is struck by their low scholarship in the other languages, their achievement in the ancient languages being at the bottom of the list. History students gain highest scholarship in history, philosophy, and the social sciences, but their grades are significantly low in all other departments.

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[14] Davis, *op. cit.*

[15] T. L. Bolton, What is the disciplinary value of the classics? *School & Soc.*, 14, 1921, 205-208.

The data for the social science group present some interesting facts, as shown in Fig. 5. In the first place their achievement is below the average in every department including their own. Secondly, their achievement is rather uniform in all departments with the exception of the foreign languages and exact sciences, where it drops to a very low level. From these results it may be concluded that the social science students are inferior in scholarship to the average student, and that their interests are quite diversified.[16]

The philosophy group was too small to be included in this study, there being only eight students taking a principal sequence in this department.

SUMMARY AND CONCLUSIONS

(1) Students tend highly to specialize, apparently toward a profession, in an institution in which is given considerable freedom in the selection of courses.

(2) The evidence is quite clear that any given student may gratify the same general intellectual desire equally well in several different departments of instruction. Hence, it is impossible to define, except in a general way, any types of students.

(3) Students who are pursuing a major sequence in one of the sciences tend to seek a wider range of experiences than do those who are interested in the humanities.

(4) There is moderately high positive correlation (0.34) between the number of courses taken and the scholarship gained in a given department, indicating that the student does his best work generally in those courses which he chooses more or less freely.

(5) On the basis of scholarship the foreign language students, especially the ancient language students, are not only superior to all other students but exhibit a wider range of interests.

(6) The social science students show a diversity of interests almost equal to that of the classics students but their scholarship in all departments including their own is considerably below the average.

(7) The earth science students are inferior in scholarship in all departments except the earth sciences and their interests are apparently concentrated largely in their own department.

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[16] By way of information it may be stated that the rank order of departmental groups from high to low in general average of scholarship is: Ancient languages, modern languages, biological sciences, mathematics, exact sciences, history, English, social sciences and earth sciences.

THE EXPERIENCE OF HEAT

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The experiment here reported is a continuation of the preliminary study of heat made in the Cornell laboratory by Cutolo.[1] At the time of Cutolo's study two views regarding the experience of heat were current: (1) the view of Alrutz[2] that heat is a unique and further unanalyzable quality conditioned upon the simultaneous stimulation of warm and cold spots; and (2) the view of Thunberg[3] and Kiesow[4] that it is a fusion of cutaneous qualities, particular importance being ascribed by Kiesow to the quality of pain.

Cutolo's stimuli were punctiform and areal. In the punctiform procedure he stimulated adjacent warm and cold spots; the warm by temperatures of 44°-45°C, and the cold by temperatures of 8°-14°C. In the areal procedure he had the ventral surface of the forearm placed on a temperature grill, constructed of 8 glass capillary tubes; he ran warm water, 43°-45°C, through the even-numbered tubes, and cold water, 5°-9°C, through the odd-numbered tubes. His results in both procedures confirmed Alrutz's findings. Heat, he found, is conditioned physiologically upon the stimulation of the warm and cold spots, and is introspectively a psychological unique. It is a simple quality that lies in the qualitative continuum between pressure and pain.

Cutolo's warm stimuli were close to the critical temperatures for pain and paradoxical cold (43°-45°C); he is therefore extremely cautious in his conclusions. "We do not," he says at the close of his paper, "regard the matter as settled. Kiesow's contention that heat can be obtained only with stimuli near, at, or above the pain limen, can be fully met only by reproducing the experiences reported in this paper with stimuli that are beyond the shadow of a doubt inadequate to pain; *i.e.* with a warm stimulus that is inadequate to paradoxical cold, and with a cold stimulus that proves experimentally to be inadequate to ache in the region of the cutaneous surface chosen for experimentation." The next logical step is thus indicated.[5] The temperatures of the cold and warm stimuli must be systematically varied.

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[1] F. Cutolo, Jr., A preliminary study of the psychology of heat, this JOURNAL, 29, 1918, 442-448.

[2] S. Alrutz, Om förnimmelsen 'hett,' *Upsala Läkaref. förh.*, 2, 1897, 340-359; The sensation 'hot,' *Mind*, 7, 1898, 141-144; Studien auf dem Gebiete der Temperatursinne, II. Die Hitzeempfindung, *Skan. Arch. f. Physiol.*, 10, 1900, 340-352.

[3] Torsten Thunberg, Förnimmelserna vid till samma ställe lokaliserad, samtidigt pagaende köld-öch värmeretning, *Upsala Läkaref. förh.*, 1, 1896, 489 ff. Cf. also W. A. Nagel's *Handbuch der Physiologie des Menschen*, 3, 1905, 707.

[4] F. Kiesow, Zur Analyse der Temperaturempfindugen, *Zsch. f. Psychol.*, 26, 1901 231-246.

[5] Since Cutolo stimulated only adjacent warm and cold spots, another problem remained: the degree of adjacency necessary for the arousal of heat. J. H. Alston (The spatial condition of the fusion of warmth and cold in heat, this JOURNAL, 31, 1920, 303-312) found that under adequate stimulation relatively widely separated spots (10-15 cm.) still give the simple quality heat. His *Os*' descriptions agree with those reported by Cutolo, and further corroborate the finding of Alrutz that heat is a unique and ananlyzable experience.

APPARATUS AND PROCEDURE

Since Cutolo obtained identical results in his two procedures—the punctiform and the areal—we chose the areal as being simpler and more suited to our requirements.

Apparatus. We constructed a temperature grill of small copper tubing (1/16 in. bore), making the cold (A) and warm (B) systems separately. Each system consisted of a piece of tubing bent, without kinks or buckles, into 10 parallel sections every one 14 cm. in length; the water flowed freely and unobstructedly through each system. The turns of system A were bend down so that the two systems, when interplaced and fastened to the asbestos-covered cleats on the arm rest, lay in the same plane. At the cleats the systems were isolated from each other by asbestos, and between the cleats, which were 8 mm. high, by air spaces of approximately 1 mm. The stimulating surface was the area of the grill between the cleats, 8 × 8 cm.

The water, forced through the systems by the pressure of the University mains, was led to the grill systems through copper coils, kept in constant temperature cold and warm water baths. We regulated the flow of water from the mains, and, by means of electric heaters and fans, we maintained the temperature of the room at 25°C ± 0.5°. As a result the temperature of the grill systems remained constant for any given setting to within 1/10°C. During the preliminary set-up we took the temperature of the water both before it entered the grill systems and after it had passed through. After the differential between the temperatures at the intake and exit had been established, we took the temperature of the water only at the exit. We adopted this procedure so the thermometers at the intakes—at the forward end of the apparatus—could be dispensed with as they would have interferred with the *O*s' freedom of action. The thermometers at the exits were suspended so that the streams of water, squirting from the grill systems, bathed the mercury bulbs.

Procedure. We had 4 temperatures for each system: 2°, 10°, 20°, and 30° for the cold system A; and 34°, 37°, 40°, and 43° for the warm system B. We used every one of the cold temperatures 5 times with every one of the warm temperatures.

When *E* had brought the systems to the proper temperatures, *O*, seated at a table and to the left of the grill, placed his left hand on a rest; at a signal he brought down the volar surface of his left forearm and rested it, with the pressure of the arm's weight, upon the grill. After 15 sec. he removed his arm and wrote his report. Meanwhile, *E* adjusted the temperatures for the next experiment.

Observers. Our *O*s were Dr. Elizabeth F. Möller (M), honorary fellow in psychology, Dr. Harry Helson (H), instructor in psychology, and the senior author (D). H and M worked without knowledge of the problem. All the *O*s were acquainted with Titchener's touch pyramid.[6] How far they may have been influenced by this classification we do not know with certainty; their reports, however, show that they were affected little if at all, and that they approached the observations without bias.

Instructions. At the beginning of every experimental hour *E* read the following instructions to the *O*s: "At the signal 'now' place the volar surface of your left forearm upon the apparatus. Keep your arm steady. Remove it at the second signal 'now,' and write your report. Describe the course and the quality of your cutaneous experience during the experimental interval."

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[6] E. B. Titchener, Models for the demonstration of sensory qualities, this JOURNAL 31, 1920, 213.

RESULTS

The number of times that every *O* reported heat at the various temperature combinations is shown in Table I under the captions 'Total.' All the *O*s report heat at warm temperatures far below those adequate to pain and paradoxical cold, and at cold temperatures far above those adequate to ache. For example, they all report heat at 34° given in combination with 2°, and at 30° with 40° and 43°. At the intermediate temperature-combinations, in which neither of the extremes of warm and cold appear, all again report heat. When for example, system A is at 10°, H reports heat at all 4 temperatures of system B; D and M at 37°, 40°, and 43°. Again, when system A is at 20°, H reports heat at all the temperatures of system B; D at 37°, 40°, and 43°; and M at 43°.

TABLE I

Showing the number of reports of intense heat and mild heat and the total number for every *O* at the various stimulus-temperatures.

Stimulus Temperature		OBSERVERS								
		D			H			M		
A	B	Intense	Mild	Total	Intense	Mild	Total	Intense	Mild	Total
30°	34°	0	0	0	0	0	0	0	0	0
	37°	0	0	0	0	2	2	0	0	0
	40°	0	1	1	0	5	5	0	1	1
	43°	0	4	4	5	0	5	0	2	2
20°	34°	0	0	0	0	4	4	0	0	0
	37°	0	2	2	0	4	4	0	0	0
	40°	0	4	4	0	5	5	0	0	0
	43°	4	1	5	5	0	5	3	0	3
10°	34°	0	0	0	2	3	5	0	1	1
	37°	4	1	5	3	2	5	0	3	3
	40°	3	2	5	3	2	5	3	1	4
	43°	5	0	5	5	0	5	4	0	4
2°	34°	1	3	4	5	0	5	0	2	2
	37°	2	3	5	4	1	5	2	1	3
	40°	3	2	5	5	0	5	2	2	4
	43°	5	0	5	5	0	5	5	0	5
Total		27	23	50	42	28	70	19	13	32
43°	43°	0	0	0	2	3	5	0	0	0

Reports of heat at temperatures that are totally inadequate to ache, pain, and paradoxical cold clearly indicate that these qualities are not the *sine qua non* of the experience. Kiesow's contentions are fully met; Alrutz's findings are again confirmed.

Quality. The introspections, again, bear out the view that heat is a unique quality. We give below excerpts from the *O*s' reports at the various temperature combinations.

D, (temperatures 30°-43°) "Cool, of momentary duration; then sudden and abrupt change to warm heat, that is to a warm that is slightly pricky and puckery or drawy. This quality is there in addition to the warm. It soon disappeared, leaving the smooth comfortable warm behind, and this in turn gradually faded in intensity."

(20°-43°) "Cool momentarily, and then hot. This rapidly grew in intensity, and then toward the end of the experiment declined. During this experiment I gave my entire attention to the quality of heat. The words that I have hitherto used to describe it do not fully convey the character of the experience. 'Stingy' is too gross; hot does not have a gross character. It seems intimate, 'Drawy' is too mild. The quality isn't coarse, but neither is it lamb-like. It is pressury without a doubt. It is an astringent smooth pressure, to use a new combination of adjectives."

(10°-43°) "Cold of short duration, then a short stage of indifferent pressure; then intense heat, pricky and drawy in quality, which persisted throughout the rest of the period."

(10°-37°) "Intense cold, followed by a blush of warmth, and then by a puckery, drawy heat. It was intense, and seemed ever on the point of burning. A stingy, pressury quality."

(2°-37°) "Cool for an instance, then a warm heat. This change was abrupt. Then the warm heat became less and less warm, until the heat stood alone. This change was gradual. The heat became more and more intense, until at the end of the experiment it was very intense. The quality of the heat was at first pricky, drawy, tingling. These qualities became more and more intense as the intensity of the heat advanced. At the end of the experiment a new quality of smart was added to the heat."

(2°-34°) "At first cold; this very soon disappeared. It was followed by a period of indifferent pressure, and then by a blush of warm which lasted but a brief interval. This was followed by a mild heat which was of a pricky, drawy, tingling quality. Cold and warmth were both absent; only this heat was present."

H, (temperatures 30°-40°) "A mild hot: tingle, penetrating components and bright pressures constitute the body of the experience."

(20°-37°) "Immediate experience of heat. Tingling, bright prick and sting components—all very surfacy."

(20°-34°) "First cold, then hot. The hot took a moment to come to its full intensity, and then was a typical hot—a pricky, tingling, surfacy quality."

(10°-43°) "A clear unmistakable hot—surfacy, bright, and penetrating, with sting and prick components."

(10°-34°) "A suffused, intense, penetrating heat—pricky, burny, and stingy."

(2°-43°) "Intense heat—burn, prick, sting and bright pressures. The experience is a piercing one, penetrating from without in—a sort of blanket of heat which bright pressures, and pricks, and stings obtrude themselves."

(2°-34°) "Intense heat with smart, tingle, prick, and bright pressure components. Penetrates and bites in; the more intense the heat, the deeper the bite."

(2°-37°) "Intense, stinging, biting heat with prick, tickle and bright pressure components. The first shock was the worst, eating into the arm and then subsiding into a constant sting and heat. The experience maintained a constant quality after the first intense experience. This intense heat was penetrating, from without in, with bright quivery components strongly in evidence. The bright pressures almost verge on pricks, being like little needles pressing in a little way. The experience seems to be punctuate rather than smooth and continuous."

(2°-37°) "First a 'welling-in' of heat which increased in intensity, and then a slight decrease in intensity, but still preserving the heat quality. Bright pressures, sting, prick, and pain constituted the body of the experience; it remained very steady after the first rise and subsidence."

M, (temperatures 30°-43°) "First a cool neutral pressure which is indefinitely bounded. The cool is of a very low degree of intensity—barely cooler than body temperature. Almost immediately another experience appeared—a pricky pressure, not particularly intense, but with a suggestion of pain in it, so that it appeared at first as heat. The prick and pain components were apparent later."

(20°-43°) "Cold neutral pressure, then heavy prickly pressure with a little cloud of warmth around the edges. The pricky pressure was faintly unpleasant, faintly painful, and was perceived as heat. Of the qualities of cold, warm, heat, and pressure I can speak with certainty. They were clear as qualities. The heat experience, however, was dominating—its intensity was so great that it tended almost completely to overshadow the other experiences."

(10°-40°) "A neutral pressure which at first appeared to be warm—a welling, active warm—with an upper boundary of cold. A part of the warm experience changed to heat, *i.e.* to a pricky, painy experience that wasn't a real prick or a real pain, and that was faintly unpleasant. But around the heat there was a kind of aura of smoky, moving, welling warm which was pleasant."

(2°-43°) "At first an indefinite but limited area of neutral pressure which is also cold. The cold is rather intense. After a short time an experience of warm seems to well up inside the cold. The warm increases rapidly in intensity, and suddenly is gone, except for a faint suggestion which forms an outline around an intense painy, pricky pressure which occupied the position of the previous warm. This pressure, during the experience, moves closer to pain and farther away from prick. It is the experience known as heat, though I take it as a pricky-pain when it occurs. The cold, which surrounds the warm outline of the heat, increases in intensity, until it too seems more pressure than cold. It lies flat, but something sharp and light throbs in it. This is almost like the heat experience. It is just enough different to be different. The heat-pressure is lighter and more active and twinkling; the cold-pressure is slow and rhythmical in its recurrence, and it is heavier."

(2°-40°) "There was an indefinite neutral pressure to which was immediately added an experience of cold. Then warm arises gradually, welling up. After a short time the warm disappeared, except for a small bit of it. In its place there appeared a pricky, painy pressure that was not really prick or pain, though it was like both; it was, in other words, heat."

In the series of 80 experiments, D reports heat, as shown in Table I, 50 times. His characterizations of this quality are: drawy (33), puckery (25), pricky (17), stingy (11), pressury (10), astringent (6), tingling (5), smooth (2), burning (2), smarting (2), soft (1). (The numbers in parentheses indicate frequency of use.) 'Drawy,' 'puckery,' or 'astringent' appear in 44 of the 50 descriptions of heat. 'Stingy,' 'burning,' and 'smarting' refer to intense experiences only; 'pressury,' 'tingling,' 'smooth,' and 'soft' only to mild. For D, therefore heat is an astringent quality that lies in the qualitative continuum between pressure and pain, close to prick and nearer to pressure than to pain.

H reports heat in 70 of the 80 experiments. His characterizations are: prick (53), sting (42), bright pressure (30), tingle (25), burning (19), pain (16), biting (10), smarting (7), piercing (4), pressury (4), tickle (2), itchy (2), ache (2). The first four terms are most characteristic of his descriptions; the others are, for the most part, attempts at differentiating the intensive aspects of the experience. Heat, for H, seems therefore to be a tingling, pricky, stingy, bright pressure quality that lies in the pressure-pain continuum, closer to pressure than to pain.

M characterizes the heat experience, which she reports in 32 of the 80 experiments, as prick (29), pressure (28), pain (21), light (4), lively (4), active (3), heavy (3), sharp (2), and throbbing (2). Except for the early reports she characterized every experience of heat as a 'pricky, painy pressure;' the other terms describe the particular shadings of the pricks, pressures, and pains that were now and then observed. Heat very definitely lies for M in the pressure-pain continuum, near prick and closer to pressure than to pain.

Absolute agreement upon the characterizations of heat is not to be expected. If heat is a unique and simple quality, its description is an impossibility. All that one can do when called upon for a description of an ultimate is to exhibit it, or to name qualities that closely resemble it. Consider the situation facing a naïve *O* who is called upon to describe, say, a reddish orange—a naïve *O* because any *O* is necessarily naïve over against the world of touch as long as the qualitative series are not definitely established. Faced with the task of describing reddish orange this *O* might list the qualities that resemble it—and his compentency would be shown by his ability to do this—but he would find it an impossible task to describe it. Similarly in the case of heat. Our *O*s find heat to be a simple experience, and their characterizations are in close agreement; they thus confirm the findings of Alrutz,[7] Cutolo,[8] and Alston[9] that heat is a simple quality that lies in the pressure-prick-pain continuum, near prick and closer to pressure than to pain.[10]

Intensity. Though the *O*s were not instructed to report upon intensity, they nevertheless did so. We were able to classify their reports into intense and mild; the number of each class for every *O* and for every stimulus-combination is shown in Table I. It will be noted that the number of reports of heat and the proportion of reports of mild and intense heats vary with the differential of the stimulus-combination. Reports of heat are few when system A is at 30° and the differential small, but there are many of them when the temperature is 2° and the differential large. The absolute as well as the relative number of reports of mild and intense heat varies similarly with the differential.

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[7] Alrutz, *opp. citt.* [8] Cutolo, *op. cit.*, 455 ff. [9] Alston, *op. cit.*, 311.

[10] These results stand in disagreement with Titchener's touch pyramid (this JOURNAL, 31, 1920, 213), in which heat is placed as an intermediate between neutral pressure and prick, and pain as an intermediate between prick and ache. The location of heat is approximately correct—it lies closer to prick, however, than Titchener has represented it—but the location of pain is wrong. Pain is not an intermediate, but a terminal quality. These experimental findings necessitate the recasting of the figure.

Although our procedure does not permit us to make a direct comparison of the intensities of the various experiences of heat, and thus to determine which temperature combination conditions the most intense heat, the distribution of the *O*s' reports indicates that the larger the differential the greater the heat. When system A is at 30°, D and M report no intense heats and H—whose case we shall consider later—only a few; all the *O*s report the greatest number when system A is at 2°. Over and above this statistical evidence, all the *O*s describe the experiences of heat conditioned by the stimulus-combination 2°-43° as the most intense of all. When this combination was first given, D reported, "This is the most intense heat that I have as yet experienced;" H reported, "Experience of great intensity;" and M reported, "A very intense painy pricky pressure."

The intensive increases are characterized differently by the 3 *O*s. D says, "The quality of heat at the times of greatest intensity becomes more stingy, " "seems ever on the point of burning," "is smarting;" H describes the intense experiences as "burning," "biting," "smarting," "piercing," "painful," and "achy;" and M simply reports an "intense" or "very intense" painy pricky pressure. The intense experiences seem to differ in kind for D and H, and merely in degree for M. D's, and in a greater measure H's, intense heats are marked by an increase in the pain component. It is as if the quality of the intense experiences had shifted toward pain in the pressure-pain continuum.

The intense heats that H characterized were reported at the higher temperatures of system B. At 43° H invariably reported an intense heat. It is possible therefore that this temperature was not below H's critical temperature for pain and paradoxical cold, and that the qualitative shift noted by H in the intense experiences indicates the arousal of pain and the fusion of that quality with the true quality of heat.

In order to test this explanation we carried out a supplementary series of 5 experiments with both systems at 43°. The results are shown in Table I. D and M do not report heat; their experiences fall within the intensive continuum of warm.

D, (43°-43°) "At first blush it was an intense warm, then rapidly declined to a moderate warm, and then to a weak warm. The warm was confortable, snug, smooth, and soft."

M, (43°-43°) "The warm at first was rather intense, then it grew less and less so, but never entirely disappeared so that at the end of the experience I had a warm that was not as intense as it had been at the beginning."

H, however, reported heat in every experiment; and his descriptions in no way differed from those given in the regular series, as the following example shows.

H, (43°-43°) "Hot stingy burn with a background or matrix of warmth. The burn is penetrating and biting with bright components. It grew less intense as the experience continued. The matrix of warmth refers to the fact that behind the bright, stingy, pressury quality there was a warmth, glowing and solid in nature."

The temperature of 43° is thus found to be adequate for H to paradoxical cold and to pain. It is therefore probable that with temperatures of 43° in the regular series pain accrued to heat, and was reported as an increase of the pain component.

The explanation of the qualitative shift in D's reports on intense heat is not so clear. It should be noted, however, that the shift was not as marked in his reports as in H's. For D, the intense heats did not burn, they merely 'seemed on the point of burning;' they did not become more painful, but merely 'more stingy,' and 'smarting.' Heat is not a point upon the qualitative continuum any more than is orange. We have reddish oranges and yellowish oranges and so also we may have more or less pressury and painy heats. D's report may merely mark the qualitative dispersion of the heats in the pressure-pain continuum. We have no other explanation.

The intensity of the heat experiences was discovered by D to be conditioned in part by the pressure of the arm upon the grill. After the 39th experiment, with temperatures of 10° and 42°, he reported:

"Heat throughout the course of the experiment. It fluctuated in intensity. I observed, however, that the fluctuations could be produced by varying the intensity of the pressure of the arm upon the grill. I have been partially supporting the weight of my arm by my shoulder muscles. If I relax my muscles completely and let the entire weight of my arm bear down upon the grill the intensity of the heat is much greater. As the pressure upon the grill is varied so also is the intensity of the heat varied."

We seem to be dealing here with a phenomenon similar to that which occurs when electrodes are grasped loosely or tightly. How far the intensities reported by our *O*s are conditioned by the pressure of the stimulus we cannot say, as this was the only report that bore upon this point. We tried, however, to eliminate this variable by instructing the *O*s to relax when their arms were upon the grill.

Individual differences. We were guided in the selection of our stimulus-temperatures by the work of the other investigators. We selected values for system B which, we thought, would be adequate only to warm. That we did not succeed in doing this was, as it turned out, a fortunate error because it brought out the fact of individual differences, which now enables us to explain the diversity of results in the earlier investigations. Alrutz's warm temperature was 44°, Thunberg's and Kiesow's 45° plus, Cutolo's 43°-45°, and Alston's 43°-44°. If true heat experiences are to be studied, temperatures adequate to pain, ache, and paradoxical cold must be avoided. Thunberg's and Kiesow's *O*s, 2 of Cutolo's, and one of Alston's reported that the experience of heat was painful. Thunberg's and Kiesow's temperatures were above the critical point for pain; consequently the resulting experience would be a heat-pain fusion. Cutolo's and Alston's temperatures were presumably below the critical point, which is considered to be 45°, but we have no assurance of that. If Alrutz's, Cutolo's, and Alston's *O*s, like M, had a high temperature-pain limen. They would experience heat alone (at their temperatures); but if, like H, they had a low limen, they would experience pain in addition to heat (at their temperatures). In the first instance heat would be characterized as unique and simple; in the second it would be regarded as a fusion or complex in which pain played an important rôle. Individual differences may no longer be neglected in the selection of temperature stimuli for the arousal of heat.

Temporal aspects. All the *O*s report that, apart from pressure, the quality of cold most frequently precedes in experience. This result stands in agreement with those of Cutolo[11] and Alston,[12] and is probably due to the different latent periods of cold and warm. The cold may be followed by warm and then by heat; immediately by heat without the warm interlude; by a period of indifference and then by warm or heat or both; by a neutral pressure and then by heat; or several of these qualities, spatially patterned, may appear simultaneously. The progression is so irregular that we are unable to generalize. The only uniformity that our data suggest is a tendency for heat to be reported early when the stimulus-differential is large.

Spatial characteristics. At times, as we have just said, several qualities were observed together, not as fusions but as distinct experiences which ran their courses independently.[13]

D, (10°-37°) "The pricky drawy experience (heat), was confined to the central part of the stimulated area, the sides were warm, and the peripheral end was cold." (20°-40°) "The parts along the side and back were perceived as warm, the center and front was perceived as hot."

H, (10°-34°) "Hot, stinging burn, with cold edges." (20°-37°) "An experience that was hot and cold—not mixed. The cold in an area near the wrist, the hot in the fleshy part of the arm."

M, (10°-37°) "There were four experiences present at one and the same: a pricky pressure (heat), a cold pressure, a warm experience, and a neutral pressure." (2°-37°) "There is an indefinite area of cold pressure. Beside it there appears a warm neutral pressure. Then experience changes, and I got an outline of warm within which appears two sets of pressures which are much alike but different. One set is prickly, light, active, bright and a bit like pain (heat); and the other set is bright, but heavy and solid (cold).

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[11] *Op. cit.*, 444 f. [12] *Op. cit.*, 305.

[13] Cf. Alston, *op. cit.*, 311.

D and H localized the experiences of heat on the arm.[14] D always referred the heat to the surface. "The experience is surfacy." "The puckery, drawy, astringent, pressury sensations seem to be on the surface." H at times described the experience similarly : "heat is a surfacy, thin, filmy experience;" "it is surfacy and spread out;" "it is like a thin film about 1/4 mm. in thickness;" but at other times he reported that the experience was tridimensional. "The pricks and stings and pain components bite into the arm," "they penetrate," "seem to dig in from the outside," "to dart in from an even layer on the surface," and under these conditions "the experience becomes voluminous and gains a third dimensionality." This penetrating character was reported by H only when the temperature of 43° was used. Since this temperature is, as we have shown, above H's pain limen, we believe that the tridimensionality that he reports is due entirely to the fusion of the qualities of heat and pain, and is not a characteristic of heat itself.

M at times localized her experiences on the forearm—"experiences there on the forearm," "seemed to well-up from within"—but for the most part her experiences do not have a local reference; they are detached from her body,[15] and seem to be pure experiences.

(10°-40°) "I localize these [warm, cold, and heat] qualities in relation to each other in a sense, and also to myself. They are 'out there' in a field which I realize now is restricted, but at the time experienced seems all of existence; and they fit into each other as black smoke appears in a cloud of white smoke. There is a going-on-ness about them, as if they existed forever, yet there is no 'forever' in consciousness. They seem to be pure experiences, which would go on whether I was there to observe them or not."

(10°-37°) "These experiences are localized 'out there' some place in a small area that is the whole experiential world. I realize now how small it is; at the time, however, I don't think anything about it. It is all unreal, unrelated to me, except as it is 'out there.' "

(10°-37°) "The experiences occurred 'out there' where I could look at them. Though they occupied a rather small area—when one considers the world at large—yet that small area seemed all of existence. This is peculiar. It's as if one looked at a patch through a tube held rather close to it."

(10°-43°) "These [warm, cold, and heat] are localized in reference to each other and 'out there.' Ridiculous as it sounds, they do not occur in the wrist until I start to describe them, and then only casually. The things I am talking about are 'out there,' but I don't know where. They have limits, I know, but I can't find these limits, or anything beyond them. I don't, however, think of all this while I'm having the experiences."

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[14] So also were they localized by Alston's *Os*, B and P, *op. cit.*, 210.

[15] As Alston's observer Y, *op. cit.*, 310.

After-Images. The after-experiences that followed the removal of the arm from the grill were so pronounced and so insistent that the *Os* frequently mentioned them, though not called upon in the instructions to do so. D and M mentioned them casually, describing them in only 14 and 17 reports respectively; but H was more consistent, mentioning them in 78 of the 80 reports.

D reported after-images of heat twice, of burning heat 3 times, and after-experiences of wetness 9 times.

> (2°-37°) "After-images of heat are intense and of long duration. Even now as I write the pricky, drawy, qualities of heat play back and forth over the area of my arm that was stimulated."
>
> (20°-37°) "The quality of burn, a stingy, smarting quality, more pronounced in the after-image than in the perception."
>
> (20°-34°) "When the arm was removed I thought it was wet and actually examined it as well as the arm-rest to see whether there was any water upon them."
>
> (10°-34°) "When arm was removed I had an illusion of wetness. The illusion so strong that I had to examine my arm before I could be convinced of the contrary."
>
> (2°-34°) "After-image of wetness. This so strong that I examined my arm to see if perchance a few drops of water were not adhering to the skin."
>
> (2°-34) "After arm removed I had an unusually clear perception of wetness. I examined my arm fully expecting to see it dripping with water."

D reported wetness only at the combinations 2°-34°, 10°-34°, and 20°-34°. A greater degree of warmth than 34° seemed fatal to the illusion which, in line with the work of Bentley,[16] Thunberg,[17] Tung,[18] and Bershansky,[19] appears to be conditioned by the positive after-images of cold and pressure.

H reported after-images of burning heat 53 times, of heat 21 times, of warm twice. Twice, at the temperature-combinations 30°-34° and 30°-37°, he reported the absence of after-images; and in the two remaining reports of the 80, at the same combinations, he made no mention of the after-images. The two after-images of warm were reported at 20°-34° and 30°-37°; the after-images of heat were reported at 30°-34°, 30°-37°, 30°-40°, 30°-43°, and 20°-34°; and of burning heat at all the other combinations.

In H's case conditions of the post-experimental period seemed particularly favorable for the experience of heat. In 4 experiments in which heat had not been experienced during the experimental interval it was reported as an after-image. Examples are:

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[16] Madison Bentley, The synthetic experiment, this JOURNAL, 11, 1900, 414-425.

[17] T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen*, iii, 1905, 708.

[18] S. Tung, The integration of punctiform cold and pressure, this JOURNAL, 32, 1921, 421-424.

[19] Ida Bershansky, Thunberg's illusion, this JOURNAL, 34, 1923, 291-295.

(30°-34°) "A flash of cold at first then nothing except pressure... The after-image is a burning, welling experience."

(30°-37°) "Experience of warmth that was almost heat but not quite because the prick and tingle and pain components were not strong enough. The warmth is full, voluminous, smooth and agreeable... The after-image is one of heat."

H's after-images of heat in some of the experiments "are less intense and pale as compared to the originals;" in others they were of approximately equal intensity, "just like the originals;" while in still others "the after-images are hotter," "more intense," "burn, tingle, and hurt more than the originals," *i.e.* the heat aroused during the experimental interval.

The after-images of heat "waver," "come, and go" "fluctuate," they are "steady," "constant;" they are "of short duration," "of long duration," "persistent;"[20] and they are "surfacy," and also "penetrating." Our data are too few to determine the cause of this variability; we merely note the divergence.

M reported after-images of heat 7 times, cold 4 times, cold and prick twice, cold and warm twice, and of warm alone twice. These experiences were reported only incidentally.

(10°-34°) "There is even now a positive after-image of heat."
(2°-40°) "There is a positive after-image of cold."
(2°-43°) "There is a positive after-image of the cold and the prick."
(2°-40°) "I still have a positive after-image of cold and warm."
(30°-43°) "There is a faint after-experience of warm—but it is almost gone now."

The after-images of heat that M reported followed experiences of heat, but not every experience of heat was followed by after-images of heat. Too few after-experiences were reported to enable us to draw a correlation between the nature of the experience and the resulting after-image. We must be satisfied here merely with recording the various after-experiences that M reported.

Synaesthesis. M gives descriptions of her warm and cold experiences that are synaesthetic in type. "It is funny," she writes in one of her early reports, "but these things (warms and colds) suggest pressure and gray. I want to describe them, not in terms of warm and cold, but as curious pressures that aren't related to me, but are 'out there' in front of me and are gray." She consistently throughout the experiment described the warms as "smoky," "dark," "cloudy," and the colds as "misty," "grayish," "foggy."

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[20] The after-images of heat were frequently of extremely long duration. H once reported that "the burn, tingle, and prick persisted all afternoon and into the evening. At 4 o'clock I looked at my arm to see if a blister was there, so real and clear were the after-images from it."

(10°-37°) "The cold is grayish, misty, like a thin sheet of fog that is almost rain. Then a part of this cloud (the upper left corner) grew bulky, rolling, deep, and rather illusive and darker—this was a soft warm."

(20°-34°) "The cold is thin and white, the warm is soft and gray."

(10°-40°) "The warm welled-up in a dark cloud."

(20°-37°) "The warm became less intense, but it stayed dark."

These expressions are not mere figures of speech used to illustrate the differences between the cutaneous qualities, but are real concomitants of the experiences themselves, as the following report clearly indicates:

(10°-43°) "These experiences, as I get them, are 'out there,' misty and cloudy. They seem as objective as a book on a table. I stand off and look at them. At the same time that they are in a sort of pattern they have no definite boundaries. I can say, 'here is warm,' and, 'here is not warm,' but not, 'here warm stops.' There is a permanence, a stability about them as if they were entirely independent of me."

In spite of this apparent objectivity, M realized—as all synaesthetic subjects do—that the concomitants were imaginal and without objective reality. She gives two reports that bear upon this point.

(10°-37°) "The warm and cold appeared gray—the warm as a very dark blob, the cold as a thin lighter mist. Though they are localized 'out there,' there is nothing gray there, but that's the best way to describe the experience."

(20°-37°) "Two temperature experiences that appeared as cold and warm pressures. Although there is no visual stuff there, the experience can best be described as a large gray square which is rather dense, thin but compact, and in which at the top there is a strip of gray smoke, which at first is very dark, and finally disappears entirely except for a tiny spot."

While M does not positively say that these experiences are synaesthetic, it is obvious that they are:[21] they are stable—warm is always reported as dark, and cold as light; they are localized indefinitely in front—as are the visual concomitants of most synaesthetic subjects; they are imaginal—they are recognized by M to be without objective reality and secondary to the primary cutaneous experiences.

SUMMARY

The results of this study may be summarized as follows:

(1) Stimuli, which singly are adequate only to cold and warm and inadequate to ache, pain, and paradoxical cold, arouse, when combined, the experience of heat.

(2) Physiologically heat is a fusion of excitations that are normal to cold and warm.

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[21] The literature upon synaesthesis is voluminous, but it is devoted almost entirely to theoretical discussions of its origin, to questionary studies of its frequency, and to descriptions of the various types. The differentiae of the synaesthetic images have been neglected.

(3) Psychologically heat is a simple quality that lies in the pressure-prick-pain continuum, near prick and closer to pressure than to pain.

(4) The intensity of the heat experiences seems to be dependent upon the stimulus-differential.

(5) The concomitant arousal of pain and heat seems to condition the experience of burn.

(6) The temperature-pain limen varies greatly among different *O*s. Individual differences must be taken into account in work of this nature.

(7) The experience of cold usually precedes the experience of heat, doubtless because of the longer latent period of warm.

(8) Heat may appear in a pattern along with warm and cold.

(9) Heat is variously localized. At times it appears surfacy and on the skin, at other times voluminous and in the arm. One *O* localized it outside of the body.

(10) After-images of cold, warm, heat, burn, and wetness were reported.

(11) Characterizations of the temperature experiences were given by one *O* that are synaesthetic in type.

AESTHETIC PREFERENCE BY COMPARISON WITH STANDARDS

By EDITH F. SYMMES, Psychological Laboratory, Boston Psychopathic Hospital

Following the lines of experimentation with aesthetic preferences described by F. L. Wells in his paper "Reactions to Visual Stimuli in Affective Settings,"[1] this study of choice reactions by comparison with standards was undertaken. A simple chain reaction experiment of the sorting type was devised under the direction of Dr. Wells, in an attempt to observe whether analogous results were obtained when the relatively elaborate technique of the laboratory was not used. The only instrument of measurement used was a stop-watch with which the times were recorded in seconds.

As in previous experiments picture post cards had proved satisfactory stimuli, this type of material was utilized. Selecting the material presented some difficulties, since it was desirable to use such as could readily be purchased in sufficiently large and uniform quantities so that if further work were carried on with the same problem, similar material could easily be found. The group of stimuli consisted of post-cards of buildings and landscapes, and colored papers mounted on cards of post-card size. Important public buildings of the city in which the experiment was conducted were used as one set, regular post-cards (3½ × 5½ in.) being purchased through a news-dealer. Only post-cards on horizontal axes were used. Nineteen cards were given to 10 *S*s, who arranged them in order of preference, from best to worst. The card falling the greatest number of times in the position of median was chosen as a standard of comparison with 12 other cards, these 12 being chosen from the entire group to include 2 obviously worse, 2 obviously better, and 8 whose differences from the median card were smaller. A set of landscapes, mainly Canadian, comprised the second set, and the same procedure as for the selection of buildings was carried out, except that there was an arbitrary attempt to have a mixed group, including some pastoral and some water scenes.

The saturated colors, red, yellow, green, and blue, were chosen as standards of comparison with the colors. Four-inch squares in sets containing the spectrum colors were purchased from the Milton-Bradley Company. These squares were cut in half, making 2 × 4 in. rectangles, which were then mounted in the center of white cards 3½ × 5½ in. Six colors besides the standard were used, these 6 being varying shades of the saturated color. Among these 6 the attempt was made to have 1 color obviously better than the standard saturated color, 1 obviously worse, and the remaining colors requiring a more difficult choice. The buildings were numbered conspicuously on the back, the landscapes lettered, and the colors marked with a number and letter combination.

The cards were presented to the *S* with standard of each group placed directly in front of him, while the examiner said, "I am going to show you a group of post-cards (or colors) which I want you to divide into two piles, those which you like better than this (indicating the standard), and those which you do not like as well." The reaction-times for each group were taken in seconds. Then those cards indicated by *S* as better than the standard were marked B on a record sheet which had the cards listed according to the order in which they were presented. Similarly those cards designated as worse were accorded a W on the record. The time in seconds for each group was recorded below. A typical record might read as follows:

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[1] *J. Exp. Psychol.*, 8, 1925, 64-76.

Buildings		Landscapes	
Card No.	Designated as	Card No.	Designated as
1	B	A	W
2	B	B	B
3	W	C	B
4	W	D	B
5	B	E	W
6	W	F	W
7	W	G	W
8	B	H	W
9	W	I	B
10	B	J	B
11	W	K	B
12	W	L	B
Time 27″		Time 18″	

Green		Yellow	
Card No.	Designated as	Card No.	Designated as
1	W	1b	W
2	W	2b	W
3	B	3b	W
4	W	4b	B
5	B	5b	W
6	W	6b	B
Time 8″		Time 7″	

Blue		Red	
Card No.	Designated as	Card No.	Designated as
1a	W	1c	B
2a	W	2c	W
3a	W	3c	W
4a	B	4c	W
5a	B	5c	W
6a	B	6c	B
Time 7″		Time 6″	

In all, 103 Ss were given the series of buildings, landscapes, and colors. Over half of the tests were given by the writer, and the remainder by an assistant experienced in psychometric methods.[2] The group was a rather highly selected one. It comprised members of the professional class or students, salesmen, and clerical workers or executives, with a few semi-skilled and unskilled workers. The amount of education and training in aesthetic appreciation must necessarily have had an influence on the choice reactions.

The following table, Table I, shows the distribution of ages according to decades:

TABLE I

Distribution of *Ss* by Age and Sex

Age	No. Men	No. Women	Total
Under 20	1	1	2
20-30	23	18	41
30-40	4	13	17
40-50	5	11	16
50-60	8	11	19
60-70	1	7	8
Total	42	61	103

Between the ages of 20 and 30 there were 5 more men than women, but in all other decades women predominated.

Distribution curves of the reaction-times for the total group and for the men and women separately were made, which, however, were largely skewed. Scores were widely scattered, possibly because of too small a group or too selected a group. Then distribution curves were made according to decades, and only in the decade from 20-30, which was also that comprising the largest homogeneous group, was there anything resembling a Gaussian curve. Median times and the mean deviations are shown in Table II.

In general the total reaction-times tend to decrease with each group of stimuli, possibly from increasing familiarity with the task. The separate reaction-times for the men and women respectively show a slight exception to this, the men taking a somewhat longer time for landscapes than for buildings, and with no decrease in time between the blue and green colors. The reaction-times of the women were consistently lower with each group except for the last stimulus, shades of red, whose time was somewhat longer than that for the preceding stimulus, yellow. There was, from a superficial point of view, more difference between men and women on the reactions to red than any other color. Subjective interpretation given by certain individuals seemed to point to the fact that the men experienced an immediate sensation of pleasure from the standards of comparison, with a subsequent quick relegation of the remaining shades to the pile designated as worse.

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[2] For help in the development and administration of the test, the writer is indebted to Mrs. Esther C. Whitman.

TABLE II

Median Times and Mean Deviations

	Men		Women		Total	M.D. from M.T.	
Cards	M. T.	M. D.	M. T.	M. D.	M. T.	Men	Women
Buildings	33.8	21.8	40	26.5	37	21.9	26.4
Scenes	35.7	14.9	35	22.9	35.6	14.9	23.0
Green	13.7	7.1	14.7	7.5	14.2	7.2	7.5
Blue	13.7	8.0	14.4	8.3	14.1	7.8	8.3
Yellow	13.5	7.4	13.6	8.3	13.5	7.4	8.3
Red	12.2	8.3	14.7	10.4	13.4	8.6	10.4

Because of the wide range of reaction-times, the preponderance of older women, and the greater variations which appeared among the women, the median time by decades of age was worked out as shown in Table III.

TABLE III

Distribution of Median Times and Mean Deviations
according to Age Groups

	Age Groups							
Cards	20-30		30-40		40-50		50-60	
	M.T.	M.D.	M.T.	M.D.	M.T.	M.D.	M.T.	M.D.
Buildings	29.3	17.0	27.5	12.7	43.8	13.1	60.0	28.0
Landscapes	29.4	12.3	24.3	12.2	45.0	13.2	58.3	22.7
Green	12.5	6.2	12.5	7.0	17.5	7.1	17.5	9.2
Blue	11.4	5.6	13.3	6.0	14.4	8.3	22.0	11.8
Yellow	11.1	4.2	12.5	8.7	15.0	4.6	20.0	12.0
Red	11.3	6.7	12.8	5.7	15.0	11.2	20.0	13.9

As one can see readily, the times appear to increase fairly consistently with the older *S*s. However, with the small number of individuals tested and the wide range of times, this cannot be regarded as particularly significant.

The percentage of preference for every individual stimulus was determined, but with little appreciable difference in affectivity between the men and women.

That the men gave shorter reaction-times than the women is contrary to Wells's findings.[3] Possibly the extremely long times recorded of women in the later decades, which tended to raise the median, may account in part for this. Also in the experiment of Wells the number of *S*s was smaller by far.

Distribution curves were also made on the basis of educational achievement, dividing the *S*s into three groups—college, high school, and grammar school—and including within each group those *S*s who had education equivalent to the standard named. There seemed quite a definite difference in the reaction-times among the groups. The high school group showed considerably longer times than the college group, and the grammar school group longer times than the high school group. Here again, however, the disproportion of numbers should be considered. Sixty-two of the individuals tested were in the college group, 32 in the high school group, and only 9 in the grammar school group. The median times and median deviations are shown in Table IV.

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[3] *Op. cit.*, 76.

TABLE IV

Distribution of Median Times and Mean Deviations According to Educational Achievement

	College		High School		Grammar School	
Cards	M.T.	M.D.	M.T.	M.D.	M.T.	M.D.
Buildings	31.3	19.0	45.0	29.6	60	30.7
Landscapes	31.7	16.0	40.0	22.0	75	27.8
Green	13.0	5.4	16.0	7.7	30	15.1
Blue	12.3	7.8	17.1	3.0	25	12.3
Yellow	12.2	5.4	15.0	9.7	25	11.8
Red	12.5	9.1	13.6	8.5	35	11.1

Here again the mean deviations are so great as to make none but the most superficial conclusions justifiable. The difference in times suggested by the table was quite reasonably to be anticipated, for the degree of cultural education has in most cases attuned the individual to a certain degree of aesthetic appreciation and the emotional set is ready for response.

The question arose whether or not the presentation of familiar buildings did not carry some emotional value for different individuals. For this reason pictures of churches had been eliminated. The introspective comments of various persons are interesting as relevant to this point. One young woman who quite obviously was about to choose the picture of a Y. M. C. A. building as worse, suddenly changed her mind, saying, "Oh, that should be counted as better. Think of all the good it does." Again, it was noticed that people invariably designated the State House as better, although the view shown was not particularly attractive, and several times the remark was made that the State House was a beautiful building. It seemed probable that the *S*s were supplementing the picture presented with their own memories of the actual structure. Among the landscapes there was a post-card showing a glimpse of two church spires among the trees, and on looking at this, one individual observed, "I always think churches are pretty." It would seem a better plan, in an experiment of this sort, to present pictures of unfamiliar buildings, and also with the titles eliminated. This would, to a certain extent, limit the opportunity to weight the stimulus with emotional values.

This article has assumed the theory that sensitivity is expressed in shorter time of response. That may be a dangerous premise, and it may be that the more sensitive individual takes longer to give a decision than the one whose aesthetic appreciations are less delicate. Wells observed in the small number of cases used in his experiment that shorter reaction-times were shown by the women, commenting that this result might be connected with the greater affective sensibility recognized to exist among women, whereby the pictures would make a greater affective impression on them. But throughout the study described here, in practically every group the men showed on the average shorter times than the women. This can be regarded as arguing against the theory that higher sensitivity is expressed in shortened times of responses, or as indicating that for the men the pictures presented greater affective differences. Or it may be concluded that neither statement is quite justified, because of the mean deviation, which is so wide as to invalidate any positive inference.

AUDITORY THEORY: A CRITICISM OF PROFESSOR BORING'S HYPOTHESES

By H. Banister, Cambridge University, England

In an interesting paper entitled "Auditory theory with special reference to intensity, volume and localization"[1] Professor Boring has endeavoured to "bring the known facts [of auditory theory] into reasonable relation, to point out clearly the lacunae in our knowledge and the discrepances that remain in even the best interpretation, and to formulate the crucial experiments that might resolve these discrepances." He considers that the evidence points to a non-resonance theory of hearing (p.158), and he suggests, further, that a non-resonance theory is easily to be understood if we assume "some kind of integration in the brain where multiplicity of impulses is translated back into degree of excitation" (p.161), and if we accept Köhler's theory of the electrical brain field.

The paper calls for criticism on two grounds, (a) because very doubtful physics is made the basis for wide hypotheses; and (b) because the claim that the resonance theory fails to explain the experimental facts is not fully substantiated.

The non-acceptance of the resonance theory forces the explanation of auditory phenomena back to more central processes. This does not, however, appear to Boring merely another way of confessing complete ignorance. Apparently, if we assume that cortical excitation establishes electrostatic fields, all is clear. I for one am absolutely incapable of accepting such reasoning. It seems to me that with our present knowledge of the functioning of the cortex such explanations are bad physiology and worse psychology.

Boring considers the resonance theory inadequate because (p.158) he does not see how it can explain graduations in intensity: also, in his opinion, the non-resonance theory promises a solution of the 'primary problem' of localisation; the resonance theory, apparently, offers no solution. Hartridge[2] has suggested an explanation of the facts of intensity, while I have shown[3] how the facts of auditory localisation can be explained, on the basis of present-day physiological knowledge, in accordance with a resonance theory.

I do not propose, in this brief communication, to criticise Boring's argument in detail, but shall content myself with pointing out the difficulties in the way of the acceptance of his views. I will begin with those difficulties he himself acknowledges, and then I shall go on to others he has not mentioned.

The first class of difficulties—those which Boring points out—includes (a) Ohm's Law; (b) the length of the refractory period of the auditory nerve fibres; and (c) tonal lacunae.

(a) As regards Ohm's Law, Boring confesses (p.177) that "the hearing by an observer, presented with a complex periodic stimulus, of the tones that correspond approximately to the simple harmonic components of the stimulus as given by Fourier's analysis" is the "great stumbling block to a frequency [*i.e.* non-resonance] theory of hearing." He admits that the law can be explained immediately on a resonance theory.

(b) Boring notes that his theory requires that the refractory period be very low, and he thinks "we may assume that there is a reasonable possibility that the refractory period of the human acoustic fibres is as low as 0.05σ" (p.181). This value is about 1/40 the usual estimation. I believe no physiologist would be willing to make the assumption.

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[1] This Journal, 37, 1926, 157-188.

[2] H. Hartridge, A vindication of the resonance hypothesis of audition, *Brit. J. Psychol.*, 12, 1922, 362-382.

[3] H. Banister, *ibid.*, 17, 1926, 142-153.

(c) Tonal lacunae present a serious obstacle also. Boring surmounts it by suggesting that "recent research, however, indicates that there really are no tonal lacunae in the strict sense. Sensitivity for certain regions of frequency may become diminished, but even here the tones can be heard if they are intense enough" (p.185). It may be a fact that individuals who are normally deaf to certain tones can hear them if the tones are made sufficiently intense, but who is able to say that these intense tones do not affect other fibres of the basilar membrane? It is an experimental fact that even if the fundamental frequency of a complex tone is completely eliminated the tone is heard. It is identified by its over-tones. May not this be the explanation of the fact quoted by Boring? The deduction, that "there really are no tonal lacunae in the strict sense" is scarcely justified.

In this connection some experiments performed by Andréev,[4] in Pavlov's laboratory, are of interest. Five dogs were taken and the left cochlea of each was extirpated. Two of the dogs died almost immediately as a result of the operation. Conditioned reflexes, to tones varying from 100 d.v./sec. to 11,600 d.v./sec., were then established in the remaining three dogs; after which the bone towards the upper end of the right cochlea was opened and the membranous portion of the organ of Corti was injured by means of a thin neddle. On recovery from the second operation the reflexes gradually reappeared. Full details regarding one of these dogs are given. On the fifth day high notes, *e.g.* 8,000 and 14,000 d.v./sec., gave positive results, and gradually responses to lower notes appeared. On the 114th day a tone of 580 d.v./sec. was effective, and 28 days later responses were made to a tone of 360 d.v./sec. Numerous experiments have been made since then,[5] but, so far, 3 years after the second operation, no response has been obtained to a tone below 317 d.v./sec. The second dog is found to be deaf to tones in the middle of the scale. No mention is made in the paper of the third dog. One awaits with interest the results of the histological examination of the cochleae, which will show exactly what portions of the basilar membrane have been damaged.

The second class of difficulties—those not noted by Boring—have to do largely with the facts of localisation. These facts are not so simple as Boring would appear to assume. He points out (p.164) that there are three theories of localisation: the intensity theory, the time theory, and the phase theory. The last theory may be considered as a special case of the second, but the intensity theory, "that localization is towards the side of the ear in which the intensity of the sound is greater" (p.165), is not strictly true, in spite of its popularity among psychologists. If the intensities at the two ears are not equal an observer may judge that a tone of low pitch is localised towards the side at which the intensity is the greater, but, with more careful observation, two sounds will, as a rule, be discriminated, *vide* the experiments of Myers and Wilson,[6] Stewart,[7] and myself.[8] Boring's statement (p.166): "an intensive difference can be equivalent to a time difference and the two can cancel each other in effect," in support of which Stewart's results[9] are quoted, quite overlooks the fact that Stewart found that variations of intensity did not always change the localisation. "One image," Stewart remarks on p.428, "did not rotate but remained directly in front, or, by trial, in the position determined by phase difference." Stewart's apparatus was, admittedly, not very exact; but his finding has been fully borne out by other experiments.[10] Since the intensity theory, as expounded by Boring, does not cover these facts, the explanation would seem to fail.

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[4] — Andréev, *Pavlov's Jubilee Volume*, 1925, 339-362.

[5] Personal communication by Dr. Anrep who is in close touch with Pavlov's laboratory.

[6] H. A. Wilson and C. S. Myers, The influence of binaural phase differences on the localisation of sounds, *Brit. J. Psychol.*, 2, 1908, 370 f.

[7] G. W. Stewart, The function of intensity and phase in binaural location of pure tones, *Phys. Rev.*, 15, 1920, 427 f.

[8] H. Banister, Three experiments on the localisation of tones, *Brit. J. Psychol.*, 16, 1926, 266-279.

[9] Stewart, *op. cit.*, 438

With high tones two and even three apparent sources of sound can be localised when the intensities at the ears are equal.[11] I fail to see how this can be explained by electrostatic fields.

Nor do I understand how Boring will explain the fact that, with sounds of low pitch, time differences at the ears, if they are above a certain maximum, produce ambiguous localisation.[12]

Elsewhere[13] I have enumerated other experimental facts which are satisfactorily explained in accordance with a resonance theory. In order to be convincing, other theories of audition should also be able to explain these facts. The intensity theory, as expounded by Boring, seems incapable of doing this.

On page 175 of his paper Boring states that his theorising "implies that the intensity of a diotic frequency should vary when the phase-relation is varied dichotically," and he goes on to say that an experimental investigation of this relation "would be almost an *experimentum crucis.*" In the course of some of my experiments[14] I found that not one of four observers could detect any change in the intensity of a 'diotic frequency' when the 'phase relation' was varied 'dichotically,' though a change of intensity of 10% was easily appreciated. This does not mean that intensity does not change with phase, but the change, if any, could not be appreciated and it was less than 10%. This is, perhaps, as definite a finding as can be expected when the result is negative.

There is one attribute of sounds which I have not so far mentioned, but to which Boring ascribes very great importance, viz., volume. According to him, "volume is at the basis of auditory localization just as visual and tactual extent are basal to visual and tactual localization" (p.176).

Almost everyone who has experimented with sounds has had observers who have reported that sounds possess volume; but it is a very elusive attribute. The volume of any given tone may, my experiments show, vary with the observer; while, with the same observer, the volume may change with pitch, with intensity and with the relative phase at the ears. Reports from Rich's observers[15] indicate that they experienced similar difficulties: they were not at all sure that what they were observing was 'volume.' It is extremely questionable whether the present statues of the attribute 'volume' justifies Boring's statement that "experimental technique brings it [volume] easily under observation" (p.176).

I am not prepared to assert that sounds do not intrinsically possess the attribute of volume, but *a priori* one would not expect sounds to have volume. We do not conceive of light as possessing volume. The object we perceive through the eye has volume; but light has none. Similarly the object we perceive by means of the ear has volume: it is a piano or a person. But one would hesitate to postulate that the sound produced when a piano is played, or when a person sings, has volume. A sounding body vibrates, and the energy due to its vibration is transmitted, by means of wave-motion in the air, from the sounding body to the ear. When this energy has aroused nervous impulses in the auditory nerve fibres, and after the various processes necessary to perception have been completed, the sound is heard. However, in no case does the sound exist objectively as a sound. We have sounding bodies, but we do not have sounds as objects of perception. It is customary to say "I hear the sound of a bell," when it would be more correct to say "I hear a bell sounding," *i.e.* vibrating with a certain frequency. It is the 'bell' we hear, not the 'sound.' But we tend to speak of 'sounds' and to treat them as objects, and it seems possible that, as a product of this tendency to reify, we come to consider sounds as being possessed of the usual attributes of objects, and among them of volume.

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[10] S. P. Thompson, *Phil. Mag.*, (V) 4, 1877, 274; Wilson and Myers, *op. cit.*; Banister, *op. cit.*

[11] T. J. Bowkler, On the factors serving to determine the direction of sound, *Phil. Mag.*, (VI) 15, 1908, 318-332.

[12] E. M. von Hornbostel, Beobachtungen über ein- und zweiorhiges Hören, *Psychol. Forsch.*, 4, 1923, 64-114.

[13] Banister, *Brit. J. Psychol.*, 17, 1926, 143 f.

[14] Banister, *Phil. Mag.*, (VII) 2, 1926, 416-431.

[15] G. J. Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 151.

In addition to this tendency to reify, there is the experimental fact that the direction from which sounds appear to come is often very ambiguous. This ambiguity appears to be usually due to the fact that the sound is heard as coming from two or more directions at the same time. From other data, perhaps, than those furnished by ours ears directly, we know that there is but one source of the sound. Since the sound appears to come from many directions, its seems natural that we should judge that the 'sound' is big, that it occupies some space indicated, perhaps vaguely, by the plurality of directions.

If this is so, 'volume' may be only tantamount to a confession of failure of discrimination. Hence, until further evidence to the contrary is adduced, one might be justified in maintaining that 'volume,' as an attribute of sounds, arises (a) from our tendency to objectify our experiences, combined with (b) the difficulty experienced when we endeavour to localise the source of the sound.

There is another point which seems to be forgotten by many who write on sound localisation. The question is asked: "If we can make judgments of direction with one eye, why can we not tell directions by the aid of one ear alone?" Such thinking is fallacious, for the question too often overlooks the main difference (from our point of view) between these two manifestations of energy, viz., that the wave lengths of light are very minute, while the wave lengths of sound are relatively enormous. In consequence, light waves travel from one point to another, in a homogeneous medium, in a straight line, and it follows that objects are seen only when the eye is oriented so that the light rays fall on the retina. On the other hand, owing to their greater wave length, sound waves may travel from one point to another by innumerable paths. Accordingly, the interposition of a large object between the source of the sound and the ear has, within limits, no effect; the sound is heard whatever the relative positions of the source of the sound and the ear. Obviously, it is impossible for an individual who is blind, and also deaf in one ear, to tell the direction from which a strange sound comes, provided precautions are taken to prevent him from moving his head. If he is allowed to move his head he may be able to detect the direction from which the sound comes by the consequent variations in loudness and timbre. But a normal observer, assuming that the differences between the aerial vibrations at the two ears can be perceived, will obviously be able to discriminate the 'latitude' of a given sound with reference to the line adjoining his ears as axis. He will not, however, be able to make a correct auditory judgment as to the absolute direction of the source of the sound. For this he would require three ears, not in the same straight line. I suggest with all reserve, for my experiments have not yet advanced far enough to enable me to do more than suggest, that this may explain the individual differences found when observers are required to judge the 'direction' of a sound source. Their ears furnish them with information as to 'latitude:' when pressed to name 'direction' they judge it to be that for which, for some psychological reason, they have a preference. If this is so an inquiry into the reasons for these individual preferences would be of interest.

If our ears can not furnish us with the necessary cues for determining absolute direction, then is it not probable that they are even less fitted for the task of discriminating volume?

In brief, it seems improbable that sounds can have the physical attribute of volume. But, assuming that sounds have volume it appears improbable that our ears can give us any indication of what volume is. Accordingly, it would seem more improbable still that volume can be "at the basis of auditory localization" (p.176), as Boring states. In this connection I should like to mention that one of my observers, an organist who has been totally blind since the age of three, has never been able to experience or to understand the attribute of volume, although it has been suggested to him. Sounds have direction, distance, timbre, loudness, brightness, and pitch; but he can not discriminate 'volume.'

Conclusion. In this paper I have written as though Boring were an avowed opponent of the resonance theory. This is not the case, for, as he himself says at the outset of his paper, "If my paper can stimulate more theorizing and more research that may eventually establish the place [*i.e.* resonance] theory, I shall be just as happy at seeing Helmholtz's sixty-year-old hypothesis vindicated at last" (p.158). He considers, however, that the facts now point to a non-resonance theory of hearing. It does not seem to me, for the reasons I have given above, that the evidence justifies the rejection of the resonance theory; but I shall be just as happy as Professor Boring should the truth of a non-resonance theory be finally established.

SELF-AWARENESS AND MEANING

A Critical Discussion of Elisabeth Amen's Experimental Study of the Self in Psychology

By Mary Whiton Calkins, Wellesley College

I. The self, accustomed to neglect at the hands of scientists, is much set up to find itself the sole topic of discussion in Dr. Elisabeth Amen's "Experimental Study of the Self in Psychology."[1] Other investigators have incidentally discovered a self in the experimental study of recognition of choice and of problem solving, but Miss Amen for the first time tackles, as the main experimental problem, the question of the self in psychology. In a brief historical introduction she summarizes first what she calls the self of scientific psychology, as a "sum total or interrelated system of . . . experiences"[2] paralleled by " an organization of the nervous system;"[3] second the self of sensationalistic psychology "which attempts to reduce the experience of self wholly or chiefly to complexes of kinaesthetic or organic sensation" (pp. 5 ff.);[4] and finally the self of self-psychology, the self experienced as conscious of itself and its environment. She rejects the first of these conceptions on the grounds that it "fails to account for the observable" difference between alleged self-consciousness and mere consciousness without reference to self and the second because it over-emphasizes the "sensory and imaginal aspects of the self-experience" (p. 13). She thereupon formulates as follows the precise object of her study. "The present investigation," she says, "was undertaken with the purpose of discovering (1) whether there is an immediate unanalyzable experience of self in Miss Calkins's sense of the term self which is observable to introspection; if so (2) what are the attributive terms which most accurately describe it, and (3) whether such consciousness of self is present in all experience, or whether it accompanies one kind of experience to a greater extent than another" (p. 14).

(i) Dr. Amen's monograph consists essentially in the close study of the experimentally controlled introspections of her 4 *Os*. Her "experiments" fall into six groups. In three of these her *Os* chose between two conventional designs—the one "which you would like to have shown to you a second time for the purpose of describing." Most of these introspections were guided by the definite question: "what differences occurred introspectively between these two experiences (a) of choice and (b) of recognition" of the chosen design when displayed for the second time (p. 19)? In Group IV of the experiments, Miss Amen substituted "for the recognition experiment a more active type of experience—the solving of a problem" (p. 41), and in Groups V-VI her *Os* dealt "entirely with the type of consciousness involved in the mental solution of a problem" (p. 46). Many of the problems were selected by the experimenter from standard tests, such as the Terman Group Test of Mental Ability, though "some were included from miscellaneous sources" (p. 41). In Group VI the experimenter introduced the factor of competition by reading the problems aloud to the group of 4 *Os* (pp. 58 f.). All the introspectors in Groups III-VI were asked the "direct question . . . 'Were you conscious of yourself during these experiments? When? Describe the experience' " (p. 25).

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[1] E. W. Amen, *Psychol. Monog.*, No. 165, 1926.

[2] J. S. Moore, *Foundations of Psychology*, 1921, 75 (Amen, p. 1).

[3] W. B. Pillsbury, *Attention*, 1908, 217-218; The ego and empirical psychology, *Philos. Rev.*, 16, 1907, 405 (Amen, p. 4).

[4] As upholders of this theory Dr. Amen names James as well as Titchener. The passages which she quotes from James certainly support her interpretation of him; but James, it must be remembered, is an arch eclectic and may equally well be quoted in support of self-psychology. Cf. for example, *Principles of Psychology*, I, Chap. IX, 226, or *Briefer Psychology*, Chap. XI, 153 for the assertion: "We have no experience . . . of a mere thought which is nobody's thought. . . The only states of consciousness that we naturally deal with are found in personal consciousness, minds, selves, concrete particular I's and you's."

The following are typical selections from these introspections. Arabic numerals have been prefixed for convenience of reference.

OBSERVER F

(1) "I have a feeling that I was somehow vaguely conscious of myself all during the act of choice—in the sense of self-effort—that it was 'I' who was making the choice, but the experience was not in any way clear" (p. 27).

(2) "I made the selection very quickly . . . without any noticeable preceding movements or strain. I . . . was quite conscious of the fact that R was my choice—and then came something which felt very much like a sudden jump of blood pressure which merged into a momentary general bodily tension. But the decision itself seemed to consist in some sort of meaning that I can't tie up with anything else which made me aware of the fact that I was going to select R" (p. 27).

(3) "In choice I was conscious of some sort of relationship existing between the task and myself. I felt that 'I was a factor in the situation' " (p. 27).

(4) "I continued to look at R. As I was doing this, in a state of . . . irresponsible uncertainty, there gradually arose an attitude toward R which . . . resolved itself into the meaning, 'Well I may as well select this one as any.' . . . I was aware of no strain, tension, bodily movement of any kind, until the meaning became vaguely sub-vocal" (p. 27 f.).

(5) "First familiarity came without any consciousness of self so far as I am aware; then 'Yes, I saw that a moment ago,' like a flash. . . . There was no self reference sufficiently definite for me to describe; it was there I think but only as a minor part of the meaning 'I saw that a moment ago' " (p. 29).

(6) "I approached the problem with the attitude 'I must make a choice' and . . . I definitely said these same words sub-vocally. Accompanying the saying of the words there was a vague consciousness of general organic sensations. . . . But there was something which meant 'I,' beyond the sub-vocal saying of I" (p. 29 f.).

(7) "All through my hesitation there was a vague consciousness of self which once or twice popped out toward focality. Again it seemed to be basically a unitary complex of organics which helped, at least, to give the meaning 'I.' . . . When analyzed this gives mostly organics and the 'I' almost fades out but when taken as a whole, the consciousness means 'I' " (p. 29 f).

(8) "Many instances of self-reference bound up with struggling, bafflement, effort, etc. At times there seemed to be something which was I, and this something was struggling, trying to think clearly, etc. It was all bound up with sensations of various kinds and yet it seemed quite real in itself" (p. 42).

(9) "Self-reference when I reconsidered the problem after being told it was wrong. . . . There were plenty of organics, but they seemed only to intensify the self-reference that was already there, *i.e.* they welled up after I realized what a mistake I had made. I do not say that the self-reference occurred without organic sensations, but that the latter were not noticed until after the self-reference" (p. 48).

(10) "Repeated self-references, bound up intimately with organic sensations. Often there was struggling . . . without any definite self-reference and at other times . . . the struggling, the 'sinking,'

seemed to be in part activities of what I think of as myself. In these cases it was pretty definitely 'I' who was experiencing the sinking feeling. The sinking feeling wasn't just going on—it was my sinking feeling. That at least is the way observation gave it. Possibly it is just a matter of meaning but I am not sure that I am in this case able to differentiate meaning from experience. I think one could interpret it either way one wished" (p. 48).

OBSERVER M

(11) "Conscious of self in pleasure aroused by choice. 'I' did the feeling. In recognition, 'I' did the comparing, the verbal response and the recognizing" (p. 32).

(12) "Right hand one appealed because meaning of dots on left hand one was 'spotted plague' and this meaning created a kinaesthetic upset. Self reference slightly in getting the meaning and referring it to 'me' present in consciousness at that moment" (p. 43 f.).

(13) "It was 'I' who did the figuring and regarded the answers and felt the effort of strain in attempting to get them correct. (Implicit vocal imagery.) . . . . Integration is referred to a person 'me,' who stands over and above the bundle of kinaesthetics. 'Me' and kinaesthesis in attention at same time give experience of self. Kinaesthesis alone in consciousness is merely observable imagery" (p. 49).

(14) "Verbal response. Conscious of self during problem, not as active agent, but merely as being existent and in attendance. Self did not do problem—it did itself" (p. 60).

(15) " 'Me' did the work. Self reference in keeping visual imagery of problem in my mind. When problems get harder consciousness of self always comes, then actual reference to self as entity, agent of strained attention and activity" (p. 60).

OBSERVER R

(16) "Difficulty of taking in all the data makes it [the problem] unpleasant. . . . Detect at fringe of consciousness some attitude which means, 'O let children do this.' Meanwhile interesting thought comes up, 'How I have changed.' As a boy I took delight in mental arithmetic matches. Fleeting recollections of school days. All that interferes with the solution. Have to pick up again the threads, but find it easier now to collect myself" (p. 44).

(17) "Mistook bit for thread and lost most of the time wondering how many threads in a screw. Had visual imagery of bits and thread. . . . The only reference to self was the recollection of comparison between myself, and others, with judgment that I was inferior mechanically but superior in other ways" (p. 50).

(18) "This seemed easy but too many fractions to carry in mind and easy to make a mistake accordingly. . . . No self-reference, though when I thought of others not getting it I hoped I would get it" (p. 62).

OBSERVER H

Even H, who to the last stoutly insists that he observes no self, seems, as Miss Amen also holds, to come perilously near the description of a self-experience, at least in his attempt to state, "what in my experience compares, I suspect, with the experiences in others which they call a self-reference."

(19) "Certain situations, however, seem more 'selfish.' For example, two major responses may block each other. . . Frequently here the path of least resistance is an habitual one, very frequently, a gestural and vocal motor language habit such as shaking the head, scowling and exclaiming incipiently, 'No that's not right!' 'Now, let's see!' "

"Frequently also there occurs a visual image of the soma's own face involving all the psychosis of one's having to stand before the gaze of one's fellows. Thus really there seems to be an illusion of a unique entity, a sort of ghost within" (pp. 55 f.).

(ii) The first conclusion which Miss Amen reaches from the study of these introspections is that "there is an immediate experience of self observable to introspection" (p. 72). Of her 4 *Os*, 3 made use "in their reports of verbal terms which seem more or less explicitly to imply a self" (p. 16). One of these, Mr. Frazier, a second-year graduate student at Harvard, reported the consciousness of self in 80% of the introspections; a second, Miss Murphy, a third-year graduate student at Radcliffe, in 67%; a third, Dr. Roback, in 14% of the introspections (p. 70). The failure of the fourth *O*, Mr. Hulin, another second-year Harvard graduate student, to report any self-experience is explained by Miss Amen as due not at all to his lack of them but to his "conviction" that such experiences "do not belong to the realm of psychological report" (p. 58).

Miss Amen's second conclusion is curiously at variance with these figures, with her own rejection (pp. 4 f., 9) of the 'scientific' and the 'sensational' conceptions of self and finally with the introspective reports of her *Os*, taken at their face value. Far from coming out squarely for self-psychology she describes her *Os*' self-experience as a "meaning-sensory-imaginal complex of the perceptual order," and unequivocally opposes it to "the immediate unanalyzable experience of self in Miss Calkins's sense of the term 'self' " (p. 72). She justifies this interpretation of the introspective records by the following statement: "The descriptive terms," she says, "used by Miss Calkins in her characterization of the self as 'persistent,' 'unique,' 'fundamental . . . to its experiences' and 'related to to its environment' are not terms in which the observers in this experiment have characterized the self-experience."

II. The preceding section of this paper has briefly summarized Miss Amen's conclusions. The following pages attempt to subject her reading of the introspections of her *Os* to that close criticism which her investigation, by its open-mindedness, its admirable method, and its lucidity throughout merits.

(i) The criticism first to be urged challenges Miss Amen's right to deny that her *Os* have "an experience of self in Miss Calkins' sense of the term." For Miss Amen inexplicably conceives, and teaches her *Os* to conceive, of this as "unanalyzable" experience, whereas Miss Calkins has consistently urged that the self, as well as the consciousness of self, is complex. Curiously enough, Miss Amen herself quotes (p. 10) from Miss Calkins an unequivocal statement to this effect: "The self though indefinable is not on this account elemental and thus indescribable." In repudiating an elemental self-experience, these *Os* are therefore in entire agreement with Miss Calkins herself. But, burdened at the outset by so heavy a misconception, it is little wonder if they do not realize the essential likeness of their position and hers.

It is next to be noted that Miss Amen somewhat overstresses the criticism, made by her *Os*, on Miss Calkins's account of the experience of self. The *Os* were asked "whether or not they could accept as descriptively true of their own experience" for "characterizations of the self-experience quoted . . . from Miss Calkins" (p. 39). Upon their replies, which may be summarized as follows, Miss Amen bases her conclusion that her *Os* fundamentally diverge from the view of self-psychology.

(a) All 4 *Os* agree with Miss Calkins in the assertion that "a feeling is either mine or somebody else's," that is, in the conception of a self, both as basal to experience and as unique.

(b) This should certainly carry with it agreement in the implied assertions: "all genuine psychic processes . . . belong to an I" and "we can not talk about experiencing without an I which experiences and which constitutes the 'essential foundation' of the experiences." And though, as a matter of fact, H and R 'reject,' while F and M 'question,' these last two statements, the disagreement furnishes a shaky foundation for positive conclusion. For Miss Amen herself regards H as a biased reporter of introspection (p. 58); R offers only 14 cases of self-reference and is notoriously "self-blind;" F and M are concerned to deny not the occurrence but the *invariable* occurrence of the experience of self as fundamental to its experiences. Their directly reported introspections attest this. "In recognition, 'I' did the . . . recognizing," M says.[5] "It was I who was making the choice," F asserts.[6] "The sinking feeling was not just going on, it was my sinking feeling," she says later.[7]

(c) All 4 *O*s also are said to reject "emphatically" the statement that "not only mental imagery but the consciousness of myself as 'the same ego then as now' is essential to recognition," yet more than one of the introspective records clearly indicates the presence of this experience by such statements as "I was conscious of myself . . . realizing that I had experienced a similar situation and . . . was reacting to it in a similar manner."[8] In the face of these records of directly introspected 'persistence' of self, it is most natural to conclude that Miss Amen's *O*s objected to the phrase 'the same ego then as now' because of some occult metaphysical meaning, which it was never intended to convey.

(d) The rejection, finally, by all 4 *O*s of "relatedness" as a fundamental character of self is a similar instance of purely verbal divergence. For by 'relation-to-environment' Miss Calkins means consciousness,[9] and all these *O*s in their introspection, tacitly or explicitly attribute consciousness to self.[10]

The preceding paragraph has already suggested that, compared with the spontaneous and concrete agreement of Miss Amen's *O*s with Miss Calkins's account of self, the criticisms on her terminology, called out by Miss Amen's questions, shrink in significance. Indeed the reported introspections fairly teem with indications of a self in Miss Calkins's actual use of the term. In Observations (14) and (15), for example, M stresses the active aspect of the self in express contrast with its receptivity;[11] in observation (16), R is interested in himself as changing or changed;[12] and in all his reported observations R makes much of that comparison between himself and his fellow-selves so strongly emphasized by the self-psychologist, particularly in the analytic study of emotion.[13]

(ii) The preceding criticisms have all concerned themselves with Miss Amen's refusal to identify her self-experience with that of the self-psychologist. But no less questionable than the negative conclusion that these *O*s really disagree with Miss Calkins in their own account of self-awareness is Miss Amen's positive conception of their self-experience as "a complex integration of the perceptual type . . . whose characteristic feature is meaning" (p. 69). This conception is based, in the first place, on insufficient evidence. A careful study of the direct introspective reports, one hundred in number,[14] which Miss Amen quotes for the use of her readers discloses only 23 reports in which the words *meaning, mean* or *meant* occur at all. Since direct references to the self are found in 58 of these 100 reports—not to name at least 20 more of equivalent import which lack the word 'self'[15] it would seem, on the face of these reports, a hopeless task to seek in the apparently infrequent experience of meaning a clue to the far more often reported experience of self. Such a suspicion is strengthened by a closer study of the introspective reports which are marked by the occurrence of the term 'meaning.' In a third of them, or more exactly in 8 of the 23, the term 'means' or 'meant' or 'has meaning' is used in the ordinary sense of 'signifies' or 'regularly suggests' or 'indicates.'[16] Examples are:

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[5] Observation (11), above. Cf. Observation (13).

[6] Observation (1), above.

[7] Observation (10), above. Cf. W. McDougall, *Outline of Psychology*, 1923, 40.

[8] Quoted from Observation (23), below. Cf. Observation (5) and Amen, p. 37, (R, Jan. 22).

[9] Cf. M. W. Calkins, *First Book in Psychology*, 1909, 3.

[10] Cf. Observation (3).

[11] Cf. on self as active, M. W. Calkins *First Book in Psychology*, 3, 11 f., 181 f., 226 ff.; also Ach, and Michotte and Prüm, quoted by Miss Calkins this JOURNAL, 26, 1915, 508-516; also A. H. Martin, Experimental study of the factors of choice," *Arch. Psychol.*, No. 51, 1922.

[12] On the self as changing, cf. Calkins, "The truly psychological behaviorism, *Psychol. Rev.*, 28, 1921, 14 f.

[13] On the significance of the consciousness of *other selves*, cf. Calkins, *First Book in Psychology*, 12, 144 f., 179, 187 ff., 251 ff.

(20) F: "What tensing of muscles meant I do not know" (p. 29).

(21) F: "The meaning of the relationship [between figures R and L] was not that L was smaller but that R was larger" (p. 28).

(22) F: "Then a complex of bodily sensations which meant 'I must make myself remember this'" (p. 17).

The first two of these quotations can not record cases of self-experience characterized by meaning, for self-experience is not so much as mentioned; in the third quotation meaning is a mere pointing to self-awareness. In at least four other passages 'meaning' is clearly used without relation to 'self-experience'—twice attributed to figures (pp. 17 and 32) and once, in Observation (11), to dots. There is, in fact, among these 23 statements only one unequivocal identification of self-reference and meaning: the assertion, in Observation (7), "a complex of organics helped to give the meaning 'I.'"

In the face of this result, it is, at first blush, hard to account for Miss Amen's very different conclusion—based to be sure not on the 100 introspections which she quotes, but on all those of her *Os*' reports (149 of the total 313) which she regards as containing self-reference. She tells us that meaning was a component of self-experience in nearly 100% of F's 59 reported cases of self-experience (p. 64) and in over 80% of R's 14 cases (p. 68), though she points out also that M reports meaning in much under 1% of her 76 self-experiences (p. 67). With her usual candor, Miss Amen herself supplies the explanation of the glaring disproportion between her result and that of the preceding paragraph. She has attributed 'meaning' to experiences which do not claim it for themselves. Thus she says (p. 65) that "the experimenter has classified as meaning such statements of F as the following:

(23) "I was conscious of myself in the sense of realizing that *I had experienced a similar situation before, and that I was reacting to it in a similar manner.* And, when I thought, 'I may as well choose one as another,' I thought of myself in the sense *that I had no preference* and *that my choice would have no consequences for myself.*"

(24) "I was at times conscious *that 'I' must make this selection*; *that there was an obligation on my part.* This was not definitely formulated, but this feeling *that 'I' was responsible for getting this done* came to me on two or three occasions."[17]

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[14] By the writer's count.

[15] Cf. for example, Observation (18), above. In many of these 20 cases the term 'self' is replaced by 'I' or by 'me,' often italicised.

[16] Cf. Amen, *op. cit.*, 17, 27, 28, 29, 30, 47, 49, 60, 28.

[17] Miss Amen's only other example of what she calls meaning—"There was something which meant 'I,'" beyond the sub-vocal saying of 'I'—is another case of meaning in the neutral sense of 'signifying.'

But Miss Amen certainly is not justified in the assumption that these are cases of meaning. Any self-psychologist would unhesitatingly claim both quoted passages as typical statements of a self's incompletely analyzed experience—the first including a reference to the persisting or enduring self with its recurring experiences, the second stressing the individuality of self.

Miss Amen supplements this dubious interpretation of direct introspective records by appeal to the 'generalized descriptions of the self-experience' which she asked from F, M, and R (p. 53). F's statement follows, only slightly abbreviated:

(25) "I can not tell whether what I have been referring to as 'I' is really something which is directly experienced or whether it is meaning. Usually it appears to be the latter. . .

"When I say that such and such an experience carries the meaning of being uniquely personal—of being not the experience of Tom Smith but of the individual who I am. . . .

". . . Surely, taken on one level one would say, 'Here is an entity of some kind, something unique and real and active.' So says common sense. . . However, it seems far more probable that what I have denoted as 'I' is not a self but a consciousness of 'selfness,' . . . in other words that the 'I' is the meaning which has come to be attached to certain experiential processes.

"But this is guess work. I think that I experience self-reference and self-activity and, when taken on an uncritical, unanalytical level, I certainly do; but the explanation of self as meaning is so reasonable that one has a tendency to consider the first as mere naïveté. It is for *E* to decide which way she wishes to look at the matter."

Here unquestionably (save in the fourth sentence) naïve self-experience is opposed, and, in certain paragraphs, reduced to meaning; but no light is shed on the connotation of the term meaning, and F very obviously hesitates to abandon the interpretation of his experience as self-reference and self-activity for the more conventional conception of it as meaning. His appeal to the experimenter to interpret for him his own introspection seriously impairs the value of his opinion.

R does not share in these vacillations. He says unequivocally that:

(26) "The self is a meaning carried in the verbal image 'I,' 'me,' 'my.' "

M, on the contrary, in her first generalized description, does not allude to meaning. For her:

(27) "Self-reference . . . consists in consciously bringing one's self into relation with the problem in question" (p. 54).

After reading F's generalized description M, to be sure, grudgingly admits that the self-experience may "really consist of meaning," but she protests to the end that she experiences "self-reference and self working on problems even;" she deprecatingly adds, "if it is to consider the problem in a naïve way" (p. 57).

The main outcome of the 'generalized descriptions' by F and M is thus the hesitating interpretation by two *O*s that what they have already many times reported to be direct self-experience may be reducible to "meaning, sensory, and imaginal components." This interpretation certainly falls far short of carrying conviction; and R does not greatly strengthen the case for the meaning theory. Only 14 cases of self-reference are attributed to him; and he does not once use the word 'meaning' in any quoted introspection.

Even more serious than failure to prove that her *O*s really do conceive their reported self-experience as meaning is the utter uncertainty and vagueness of Miss Amen's use of the term. She everywhere assumes either that the connotation of the word is fixed or else that the divergences in its use are negligible. "Whatever the particular conception of meaning implied in these reports, meaning," she says, "is acknowledged by all four observers to be an aspect of experience which is reportable, which is a part content of the consciousness under introspection, and which is therefore experiential" (p. 38). This indifference to the signification of the term meaning is wholly unjustified, for as a matter of fact, in some conceptions of it, 'meaning' can not possibly be interpreted as a distinctive component of self-experience; and, in other conceptions, meaning is from the start merely a synonym for self-experiences.

(a) Under the first head, fall the repeated instances already referred to of meaning in its original sense of 'signifying,' 'pointing to,' 'suggesting.' That which is pointed to is sometimes, though not always, the self. But to say, (28) "The meaning of the whole experience was that this success was mine" (p. 48) no more signifies that the suggested self-experience essentially *consists in* meaning than the words, "the meaning of the dots was spotted plague"[18] signifies that spotted plague consists in meaning. In both cases meaning is simply the function of 'pointing to' or 'suggesting' something. Similarly, to assert that 'sensations' (p. 49) or 'tension' (p. 27) or 'something' (p. 47) *meant* self is far from saying that meaning is distinctive of self-experience. In this group belong also cases in which 'meaning,' used now in the sense of imaginal context, is indeed referred to self but not identified with self. An example is:

(29) "The meaning of each action appeared in the imaginal consequences as referred to *myself*" (p. 47).

(b) Besides these cases of the use of 'meaning' in a perfectly neutral sense there certainly occur repeated instances in which self-experience, or some aspect of it, is described by Miss Amen's *O*s as 'meaning.' Examples occur in Observations 2, 4, 5, 7 and 10, already noted. Miss Amen treats none of these cases in a fashion to justify her conclusion, for she really gives no glimmering of a notion of what she means by 'meaning.' If, as sometimes appears, she is employing the term in what Ogden and Richards call the "traditional and mystery sense" she is exposing herself to the judgment which she metes out to the conception of elemental self-awareness (pp. 69, 72). If she is falling back on the use of meaning as imaginal context, she is really reverting to the rejected concept of the scientific self. We can only guess that Miss Amen virtually uses 'meaning' in Titchener's sense of unscientific awareness of "the world of things and people."[19] In this case, however, meaning is merely a verbal synonym of the term self-experience; and the conclusion, alleged self-experience is a complex distinguished by meaning, reduces to sheer tautology.

In brief, Miss Amen's account of her *O*s' self-experience as essentially meaning is unsatisfactory first, because it is so insufficiently borne out by their direct introspections; second, because she never clearly tells us what she means by meaning; third, because many of the senses of the hydra-headed word meaning can not possibly serve as clues to the self-experience; and, finally, because the word meaning often is used in such a way as clearly to imply the very sort of self-experience which Miss Amen denies.

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[18] Cf. Observation (12).

[19] Description *vs.* statement of meaning, this JOURNAL, 23, 1912, 167. For Titchener's more frequent use of the term as 'imaginal context,' cf. *Experimental Psychology of the Thought Processes*, 1909, 175 ff.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

LVI. THE MOORE TESTS OF RADICAL AND CONSERVATIVE TEMPERAMENTS

By M. F. WASHBURN, H. KEPLER, N. McBROOM, W. PRITCHARD, and I. REIMER

At the 1924 meeting of the American Psychological Association, in a joint session with the American Sociological Association, Professor H. T. Moore presented a paper entitled, "Innate Factors in Radicalism and Conservatism."[1] It was based upon questionary returns from 163 Yale undergraduates and 163 Dartmouth undergraduates, and upon experimental results from two groups of students, selected by means of the questionary from a class of 225, presumably Dartmouth undergraduates. The questionary consisted of 20 questions to which the *S* was required to answer 'Yes' or 'No;' according to the nature of his answer his opinions were judged to be radical or conservative. The questions related to the entrance of the United States into the League of Nations, to its general policy of isolation from European affairs, to sympathy with the present Russian government, to pacifism, to the idea of a Superstate, to government ownership of railroads, to limitation of the power of the Supreme Court, to the formation of a Labor Party, to higher taxation of the wealthy class, to direct popular vote on governmental matters, to trade unionism, to a minimum wage, to coöperation of employees in management, to the influence of Wall Street in politics, to the relative influence of opportunity and ability in determining a man's industrial class, to the dangers and advantages of race prejudice, to the theory of evolution, to the relative weight of tradition and present usefulness in determining support of an institution, to the alleviation of poverty by social legislation, and to promiscuity in sex life on the part of unmarried persons. After this questionary had been tried out on the Yale and Dartmouth groups, it was given to a class of 225 students: those men who gave 'radical responses' to 14 or more of the questions were classed as radicals, and those who gave radical responses to not more than 7 questions were classed as conservatives. Two groups of 30 observers each were thus obtained, and were then subjected to certain experimental tests in the endeavor to see whether the test results would separate the groups.

The results were so clearly cut and so interesting that we were incited to repeat some of the tests with Vassar students. The questionary was accordingly given to 174 of these. The twentieth question was omitted, because of the probability that its being propounded to young women college students would be pounced upon by a certain class of newspapers as news of the most acceptable and highly flavored type. Since the popular opinion is that women are more conservative than men, it is of some interest to note that in the sum of percentages of radical responses to the 19 questions used Vassar, with 964, stands between Yale, with 904, and Dartmouth, with 1052. (The figures for Yale and Dartmouth are estimated from the graphs.) To five questions Vassar gave more radical responses than did Yale, and to only five notably fewer; only on the second question, relating to American isolation from European affairs, did Vassar exceed Dartmouth in percentage of radical responses. Ninety-three percent of the Vassar group expressed themselves against isolation: it is possible that in the two years elapsing between the answers of the men and those of the women there has been a shift of public opinion in this direction. Only about 76% of the Yale men and 70% of the Dartmouth men gave the radical answer to the question.

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[1] H. T. Moore, *J. Abn. Psychol. & Soc. Psychol.*, 20, 1925, 234-244.

Our chief interest lay in securing radical and conservative groups for testing. We found that we could not use Moore's criteria of fourteen radical answers as the lower limit of radicalism and seven radical answers as the lower limit of conservatism, for two equal groups could not be constructed at Vassar on this basis. It was impossible to find enough conservatives. There was one case of 3 radical answers, one of 4, three of 5, seven of 6, nine of 7, making a total of only 21 cases where the number of radical answers was 7 or less. On the other hand, there were eight cases of 18 radical answers, three of 17, fifteen of 16, twelve of 15, twenty-eight of 14, making a total of 66 cases where the number of radical answers was 14 or more. We are at a loss to explain why we had so few extreme conservatives as compared with the Dartmouth class of 225, when the group of 163 Dartmouth men made a considerably higher percentage of radical responses than did our Vassar group of 174.[2]

Moore's two groups contained 30 observers each. We shall present results from a radical section of 26 young women, comprising all those who made 16 or more radical responses, and a conservative section of 21, comprising all those who made 7 or fewer radical responses; also from two sections of 66 members each, comprising respectively those who gave 14 or more radical answers and those who gave 9 or fewer.

Professor Moore's first concern was to compare the intellectual ability of his two groups. Accordingly we secured information as to the percentile rank in her freshman test (the Thorndike Tests) of each student on our lists. The average percentile rank of the conservatives was 49 that of the radicals 44; in the conservative group 47% were better than the radical average, while in the radical group 50% were better than the conservative average. Thus, as in Moore's investigation, there was no significant difference in general mental ability.

Mirror-Drawing Test. The first experimental test performed in the Dartmouth research was the Mirror-Drawing Test. We made this test upon the *S*s in our two groups, following the directions in Whipple's Manual of Physical and Mental Tests, and recording for each *S* the average time for tracing five stars, the average number of errors, and a "total efficiency score computed by multiplying seconds by errors," as Moore says he did. He does not, however, state whether his *S*s were scored on one star only, or on more. His results were as follows: "On the score for accuracy 81% of the radicals made fewer errors than the average conservative and only 25% of the conservatives made fewer errors than the average radicals." In our experiments, on the other hand, using averages of errors in the tracing of five stars, only 51% of the radicals made fewer errors than the average conservative, and 65% of the conservatives made fewer errors than the average radical. When the errors only of the first trial at tracing the star are considered, 66% of the radicals did better than the average conservative, and 52% of the conservatives better than the average radical. "On the score for speed," Moore continues, "the difference is not marked, but in gross efficiency 91.3% of the radicals surpassed the average conservative." In our results from five tracings, 81% of the conservatives did better than the average radical, and 74% of the radicals better than the average conservative. The two averages were very close together, 3444 for the conservatives and 3393 for the radicals. The peculiar distribution of percentages is due to the wide range of scores, which was for the conservatives from 459 to 22514, and for the radicals from 308 to 21604.

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[2] Our omission of question 20 cannot explain it, for on this question the Dartmouth group of 163 gave an equal number of radical and conservative answers. No figures are reported on this point for the group of 225.

When the efficiencies of only the first trials are considered, 81% of the radicals surpass the conservative average and 79% of the conservatives surpass the radical average: the conservative average is 10176 and the radical average 9169, which means a very slight superiority for the radicals. In general, however, it is clear that we do not find the radicals significantly better than the conservatives as regards mirror-drawing. Moore interprets the superiority which he finds as meaning that the conservative temperament finds old established habits harder to break than does the radical temperament.

We also undertook to compare the larger groups. If the results from all those *S*s who gave 14 or more radical answers were compared with the results from those *S*s who gave 9 or fewer radical answers, the radical and conservative groups had 66 members each, including, of course, in the conservative group less extreme conservatives than was the case in the smaller conservative group, and in the radical group less extreme radicals. Of the radical group 59.7% surpassed the conservative group in efficiency, while 71.6% of the conservatives surpassed the radicals.

It may be noted that another factor besides the capacity to break habits readily is involved in success with the Mirror Drawing test: namely, the tendency not to be easily flurried. What makes the errors pile up is a panicky condition: phlegmatic temperaments ought, it would seem, to do better on this account, and if we accept the common view, the conservative is more phlegmatic than the radical.

Reaction Test. Professor Moore next determined the mean reaction-times of his two groups and found that the mean time of the radicals was 131.7 σ and that of the conservatives 154 σ, and that 91.3% of the radicals were quicker than the average conservative, while 27.3% of the conservatives were quicker than the average radical. He does not state how many reactions were made by each *S*.

We took for each of our *S*s two series of 10 reactions each, using the Sanford vernier chronoscope. In the case of our two smaller groups, comprising those who made 16 or more radical responses and those who made 7 or fewer, for the first series of 10 reactions the average time of the radicals was 156 σ and that of the conservatives 166 σ: 73% of the radicals were quicker than the average conservative, and 45% of the conservatives quicker than the average radical. For the second series of 10 reactions the average time of the radicals was 140 σ and that of the conservatives 149 σ: 69% of the radicals were quicker than the average conservative and 40% of the conservatives quicker than the average radical.

When the larger groups of 66 (less extreme) radicals and conservatives respectively were compared, the average times for the 20 reactions were 156 σ in the case of the radicals and 157 σ in the case of the conservatives: 58.2% of each group surpassed the average of the other group in speed.

It thus appears that there is only a very slight superiority, if any, of radicals over conservatives in the matter of reaction time.

Card Sorting Test. Another test used by Moore was "one devised by Münsterberg. The subject is asked to sort a special kind of pack of cards under conditions in which both speed and accuracy are important. What is especially significant for the purpose of this study is that the subject does not know the relative importance of the two factors. . . . Now under these conditions 70.9% of the radicals work faster than the average conservative. In other words, when the relative importance of accuracy is uncertain the radical more naturally inclines to quick snap judgments, while the fear of taking chances is sufficiently ingrained in the conservative to find its way into his behavior even in a psychological laboratory."

To repeat this test as well as we could considering the rather vague description of it given by Moore, we used the pack of cards supplied by Stoelting for the Card Sorting Test, being careful always to use the sam formula of instruction, which emphasized equally speed and accuracy. For each *S* the average of five successive sortings was taken. With our smaller groups, comprised of those who made 16 or more and those who made 7 or fewer radical responses, the average time for the radical group was 92.5 sec., and that for the conservative group 77 sec. The percentage of radicals who were quicker than the conservative average was 36; that of the conservatives who were quicker than the radical average was 94. The average number of errors made by the radicals was .53; by the conservatives, .56. There is thus no trace of a tendency on the part of the radicals to prefer speed to accuracy.

In the larger groups, of 66 *S*s each, the greater speed of the conservatives disappears, and the percentage of radicals working faster than the conservative average is 71.6, close to Moore's result. But the radicals are also somewhat more accurate: 67.1% of them averaged fewer errors than the conservative average. Moore gives no figures with regard to errors which justify his statement that the radicals preferred speed to accuracy.

It is probable that Moore like us used the average of five card-sortings. But in case he made only one, we may note that the average time required for the first sorting by our smaller radical group was 105 sec., and their average number of errors in the first sorting .86; while the average time for the small conservative group was 94 sec. and their average number of errors 1.2. In this case it was the conservatives who sacrificed accuracy to speed.

Free Association Test. "As a last experiment each man was given the 100 words of the standard Kent-Rosanoff list,[3] and was asked to write down the first word called to mind by the stimulus word. In scoring the papers every word having a frequency value of less than 10 in the Kent-Rosanoff tables was listed as an unusual association. The average number of unusual words for the conservative was 29, and for the radicals 38.2; but the mean deviation was larger than the difference in the means, so that we have here at best a mere suggestion as to greater originality, or eccentricity in the radical's simplest association of ideas."

For our smaller groups, the average number of unusual words was in the case of the conservatives 32.2, and in that of the radicals 37.4. In the larger groups the difference became negligible; the figures were 34.14 for the conservatives and 35.38 for the radicals. There is thus no tendency for the radicals to prefer unusual associations.

Hence it appears that our results throughout fail to confirm Moore's. The radicals do not tend to learn mirror-drawing noticeably faster, to have noticeably shorter reaction-times, to sacrifice accuracy to speed in card sorting, or to give more unusual responses in free association. This outcome of our work was a disappointment to us. The way of the deviser of tests for temperamental differences is indeed hard and discouraging. Is the fault perhaps, in the present instance, with the questionary? Is a radical temperament necessarily indicated by radical opinions on public questions? Is there a sex difference by virtue of which the opinions of women on public questions are less indicative of temperament than those of men?

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[3] For this list see this JOURNAL, 25, 1914, 592.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF STANFORD UNIVERSITY

II. The Rating of Psychologists

By Miles A. Tinker, B. D. Thuma, and Paul R. Farnsworth

Graduate students are interested in the relative importance of the older psychologists, philosophers and the like, as background for present-day orientations. With this fact in view, the writers prepared a questionary in order to gain an insight into current opinion. The data so gathered suffer from at least the following ills.

(1) An absolute scale, showing the importance of historical figures, can not be constructed. Importance is a relative matter and changes from age to age, and from one school of thought to another.

(2) Ratings from England, France, or Germany might possibly give quite different results. The present scale shows only the composite judgment of seventy-three American psychologists who hold the higher scholastic (and allied) positions in this country.

(3) There is always the danger of lack of coöperation in filling out questionaries. The writers hope, however, that they have passed the test laid down by Boring.[1] In general they feel that they have received excellent coöperation.

(4) The scale omits names which many psychologists will feel of prime importance. Space was left on the questionary for such additions, but the names added showed practically no agreement and few appeared more than once. This rating list, then, merely shows the rank and order of importance of historical personages arbitrarily selected by the writers and rated by seventy-three prominent American psychologists. The names selected for rating were those of psychologists (and others whose work affected psychology) who were either at present deceased, or were foreigners whose psychological reputations were made some years ago.

The questionary was as follows:

"We are interested in ascertaining the relative importance of those men who have contributed to the history of general psychology. We have in mind their importance from the standpoint of the student obtaining his doctorate in psychology.

"Please rate the following individuals on a scale of four: If the knowledge of an individual's contribution to psychology is extremely important, place (1) before his name; if quite important, but not of first importance, place (2) before his name; if of comparatively little importance, but still worthy of mention, place (3) before his name; if of no importance, place (4) before his name. Do not rate those names which are unfamiliar to you. If there are names not appearing on this list, which are important in your opinion, kindly note them on the back of this page with their respective ranks."

The names of the individuals to be rated were then listed in alphabetic order.

A scoring device suggested by Dr. T. L. Kelley was adopted. Rating 1 was given the value of 1, 2 of 2, 3 of 3, 4 of 4, and 0 (omission) of 4. Mean scores were figured for each name on the questionary, and a grand mean average for all. The names were then placed in two piles—the first for those whose means were above the grand mean, and the second for those whose means were below. The percentage of 'one' ratings was next figured for each of the two groups. The same was done for the 'twos,' 'threes,' 'fours', and 'zeros' (omissions). The percentages of ratings above and below the mean, are as follows.

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[1] E. G. Boring, this Journal 37, 1926, 632.

Rating :	1	2	3	4	0
Above :	94.5%	76.0%	45.5%	15.0%	13.6%
Mean :					
Below :	05.5%	24.0%	54.5%	85.0%	86.4%

Using the Kelley formulae for p and x, given in Appendix C of his *Statistical Method,* the ratings have the following values: 1 = 1.60; 2 = .71; 3 = −.11; 4 = −1.04; 0 = −1.10. Changing to positive whole numbers these become approximately: 1 = 4; 2 = 3; 3 = 2; 4 = 1; 0 = 1. The writers have adopted the inverse of these and have given the ratings their face value (4 = 4; 3 = 3; 2 = 2; 1 = 1;) excepting the 0, which thus gets the same value as 4.

The ranking of the men included in the list, as obtained by averaging the answers from 73 psychologists is given in the Table. It is interesting to note that in only one instance was the rating curve for a subject bimodal.

TABLE: RESULTS OF THE RATINGS

Rating	Order of Merit	Name	No. of Zeros	Rating	Order of Merit	Name	No. of Zeros
1.05	1	W. James	0	2.41	34.5	Kant	5
1.11	2	W. Wundt	0	2.44	36	Romanes	2
1.23	3	Binet	0	2.45	37.5	Lotze	5
1.32	4	Helmholtz	0	2.45	37.5	Spinoza	8
1.37	5	Ebbinghaus	1	2.53	39.5	Höffding	1
1.41	6	Fechner	1	2.53	39.5	Sanford	1
1.44	7	G. S. Hall	0	2.55	41	Lange	11
1.51	8	Galton	2	2.56	42	Hobbes	6
1.68	9.5	Herbart	1	2.63	43	Ach	13
1.68	9.5	Ch. Darwin	0	2.64	44	Hartley	11
1.71	11	Weber	3	2.66	45	Purkinje	7
1.74	12	Stumpf	0	2.68	46	Leibnitz	7
1.75	13.5	Locke	2	2.70	47	Marbe	11
1.75	13.5	Aristotle	2	2.73	48	J. Müller	22
1.77	15	O. Külpe	0	2.74	49	Bergson	2
1.81	16	Hering	2	2.77	50	Comte	5
1.84	17	H. Spencer	2	2.78	51	Royce	3
1.88	18	Ribot	1	2.81	53	Lombroso	5
1.92	19.5	Münsterberg	1	2.81	53	Nagel	18
1.92	19.5	Berkeley	1	2.81	53	Broca	11
2.01	21	Hume	4	2.84	55	J. Mill	13
2.04	22.5	A. Bain	0	2.85	56	Rousseau	7
2.04	22.5	Descartes	1	2.92	57	Condillac	14
2.11	24	G. E. Müller	9	2.93	58.5	Hartmann	16
2.22	25	Charcot	4	2.93	58.5	Meinong	18
2.27	26	Plato	2	3.00	60	Thomas Aquinas	10
2.28	27	J. Ward	4	3.01	61	F. Bacon	9
2.33	28	Mach	7	3.03	62	Socrates	11
2.34	29	Lipps	6	3.05	63	Lamarck	13
2.36	30	Loeb	7	3.11	64	Wernicke	24
2.37	31.5	Head	8	3.14	65	Flournoy	19
2.37	31.5	Brentano	9	3.15	66	Holmgren	17
2.40	33	J. S. Mill	2	3.18	67	Fichte	11
2.41	34.5	Mosso	5	3.21	68.5	Delboeuf	26

TABLE (continued)

Rating	Order of Merit	Name	No. of Zeros
3.21	68.5	Wolff	22
3.22	70	St. Augustine	15
3.25	72	Hegel	8
3.25	72	Schopenhauer	9
3.25	72	Vierordt	22
3.26	74	Carpenter	24
3.30	75	M. Müller	25
3.32	76.5	Volkmann	25
3.32	76.5	Avenarius	24
3.33	78.5	Democritus	15
3.33	78.5	Plotinus	20
3.35	80	Malebranche	17
3.36	81	Roger Bacon	17
3.37	82.5	Galen	22
3.37	82.5	Epicurus	13
3.42	84	Beneke	28
3.47	85	Bell	35
3.52	86	Drobisch	35
3.55	87	Pythagoras	19
3.57	88	Bonnet	35
3.58	89	Empedocles	24
3.62	90	Tetens	35
3.66	91	Schelling	22
3.67	93	Protagoras	24
3.67	93	Anaxagoras	20
3.67	93	Zeno the Stoic	20
3.68	95	Diderot	28
3.70	97	Averröes	40
3.70	97	La Mettrie	39
3.70	97	Waitz	46
3.73	100	Abelard	22
3.73	100	Erasmus Darwin	27
3.73	100	Magendie	43
3.74	103	Anaximander	24
3.74	103	Jacobi	42
3.74	103	Boehme	41
3.75	105	Herder	30
3.77	107.5	Parmenides	33
3.77	107.5	Duns Scotus	30
3.77	107.5	Schleiermacher	25
3.77	107.5	Theophrastus	41
3.78	110	Albertus Magnus	33
3.79	111	Gassendi	42
3.82	112	Anaximenes	29
3.86	114	Alhacen	51
3.86	114	Meister Eckhart	44
3.86	114	Xenophanes	31
3.88	116.5	Origen	40
3.88	116.5	Paracelsus	38
3.89	118	Ptolemy	26
3.93	119	Campanella	52

This exception occurred with Plato, who was rated very high by one group and very low by another. The correlations by the rank method between rank order of merit and number of zero ratings was 0.93: paucity of ratings for any given psychologist and low rating appear together. A majority of the names appearing in the two upper quartiles are those which have meant much to experimental psychology while the two lower quartiles are made up largely of philosophers.

CONCLUSIONS

(1) The questionary was developed to aid the graduate student in ascertaining the concensus of opinion concerning the relative importance of persons contributing to the development of psychology.

(2) With a few exceptions, the philosophers, to whom many of the present day historians of psychology devote most of their attention, received average or low ratings.

(3) All ratings were distinctly unimodal except that of Plato.

(4) A correlation of 0.93 (rank method) was found between rank order of merit and number of zero ratings (absence of ratings).[2]

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[2] The writers are grateful to Professors L. M. Terman and Walter R. Miles for affixing their names to the questionary, for it otherwise might not have obtained the consideration it received. Dr. Terman also arranged for the financing of the study.

BOOK REVIEWS

Outline of Abnormal Psychology. By W. McDougall. New York, Charles Scribner's Sons, 1926, pp. xvi, 571.

The reviewer has approached McDougall's *Outline of Abnormal Psychology* with great interest and lively hopes. The publishers, in their circular had described the book as "of the highest significance to the medical profession," adding that McDougall's preparation for this work has been "undeniably unique," instancing his thirty years of prominence in "the academic study of psychology and in the leading American and English organizations for psychic research," and his excellent opportunities for the study of mental abnormalities during the war. With nice discrimination, the publishers have not claimed that the book has any specific importance for the psychologist; but neverthelesss, the reviewer, realizing the opportunities that the author has actually enjoyed, was really disappointed at finding the character of the animal which the labor of the mountain has produced.

As material for the study of McDougall's own psychological development, the book is indeed interesting. But as a text for students, it is impossible to see how it can be otherwise than confusing and obstructing. The book is in fact, astounding.. It is devoted to the magnification of three men; McDougall, Freud and Jung; but while much space, (with copious quotations from his other writings), is devoted to the justification of McDougall's doctrines, every doctrine of Freud and Jung is denounced, with such expressions as "the wild and untrue generalizations which we meet on nearly every page of Freudian literature" (132), or refuted with subtler words. Contempt is poured also on the lesser psychoanalysts.

Were it not for the well-known seriousness and solemnity of McDougall's method, we might well think the volume an ironical assault on the whole aggregation of psychoanalysts. While announcing flatteringly that: "I believe that Prof. Freud has done more for the advancement of psychology than any student since Aristotle" (viii); "I have enjoyed the great advantage of having my dreams analysed by Dr. C. G. Jung" (xi); "I have the greatest respect and admiration for Dr. Jung's work and person" (150); and performing many other profound acts of worship, all strikingly dragged in; McDougall completes the most devastating analysis ever made of the psychoanalytic doctrines. My own brief assult on Freudianism is a paean of praise in comparison. The progressive renunciation by Freud of his wonderful discoveries; the rejection of the important Freudian principles by his most serious and able disciplies; the utter confusion of conflicting doctrines among the psychoanalysts; the stupid arrogance with which skepticism in regard to a wonderful psychoanalytic discovery is assumed to demonstrate the ignorance of the skeptic, (although the psychoanalyst himself abandons it later, and other equally great psychoanalysts have scoffed at it from the beginning); are pointed out in detail. "Obstinate skepticism" is expressed in regard to some of the most sacred psychoanalytic doctrines. Others are scored as "flying straight in the face of common sense." There are even sprinkled in some innuendoes, such as the "dream" remark: "He should learn psychoanalysis and he could pick up a few guineas and keep the whole family in comfort" (151).

But McDougall is not a joker. We have to assume that he is serious in his demolition of the 57 varities of psychoanalysis, and also serious in his emotional attitude (or is it the "submissive instinct"?) towards Freud and Jung. He must be rated equally sincere in his proclamations that Freud has made contributions of immense value, and in his showing that every specific contribution is junk. Very interesting psychoanalyses can be made of

McDougall's attitude from the standpoint of Freud's original sex theories, from the standpoint of Jung's older mother complex and newer "psychological types," and from the standpoint of Alder's inferiority complex. But these analyses would be merely scurrilous. Let us depend upon actual psychology.

McDougall has felt keenly the criticism passed on his theories. This is evidenced by his constantly recurring defense of his instincts, and his denunciations of "mechanism." (Whatever mechanism really is, it is for McDougall the combined opposition to his views.) In this book he emphasizes his standing as a *medical* psychologist and obviously lumps the contemner's of his theories as academic. He has misunderstood the criticism directed against him, and feels driven from the camp of the academics into that of the medicos, and wishes to emphasize the greatness and honor of his crowd, in spite of some difficulty in his acceptance of them. This is by no means an 'unconscious' desire or purpose.

On the other hand, there is a real affiliation between McDougall's position and that of the psychoanalysts. Jung accepts one instinct where McDougall accepts several; but they are, alike (the "libido" and the "instincts"), wellsprings of mystic energy. Whereas McDougall in certain pages denies the "unconscious mind," in other places he accepts it, under the different name of "subconscious." Moreover, in freedom of generalization, facile adaptation of data to illustrate theories, and fluidity of terms, his methods are strongly psychoanalytic. Thus, after insisting on the importance of purposes, as psychologically conceived, he proceeds to declare that the purposes may "operate wholly on the unconscious plane" (38), and that "motivations" (233) and "aversions" (251)—both of which are probably other names for 'purposes'—may be "subconscious." At times the "subconscious" is identified with Morton Prince's "coconscious" (254), but not consistently. In many cases, "subconscious" means precisely what is often meant by the Freudian "unconscious," namely, what psychologists mean by *conscious*, but where something is not reflected upon, or where the reasons for the occurrence of the conscious process are not recognized.

Throughout the volume, as has already been indicated, there is a recurrent antagonism to the theory of "ideas" as entities (a theory which probably none but the psychoanalysts hold), and to a dreadful theory called "mechanism." Yet here, as in previous writings, McDougall absolutely refrains from committing himself to any theory which is not as truly mechanistic as those of the "crude radicals." That the "instincts" and "purposes" operate in a non-casual way; that "psychic energy" comes out of nothing; that natural law does not hold in the mental realm; these anti-mechanistic doctrines *may* be held by McDougall; but he nowhere admits them. Actually, McDougall's psychology is as strictly mechanistic as that of Freud—he does not mention the widely heralded 'greatest contribution' of Freud *i.e.* the 'discovery' that mental life is as rigidly ruled by cause and effect as is the world of physical phenomena. The mechanism of Freud and McDougall really differs from that of |the 'regular' (McDougal would say "crude") psychologist as the mechanism of the auto driver who refuses to inquire what goes on under the engine hood, but conceives merely of a mysterious "horsepower" driving his car, differs from the mechanism of the expert who is interested in every detail of the working of the engine.

So far as the understanding of the problems of abnormal psychology go, McDougall offers the student little help. When we abstract from the boosting of McDougall's theories, the adulation of Freud and Jung, the tearing of their theories to pieces, and the sneering at the crude psychologists,, there is little left. Even in the discussion of hypnosis, where McDougall might have been expected to make good use of his technical experience, the exposition suffers from the ignoring of methods which do not come under his classification (the method of gentle persuasion, for example); and no light is thrown upon the nature of the processes involved: but, of course, this illumination could be given only by a descent to the level of the "crude" psychology which has shed what little light had fallen on hypnosis. Again, no attempt is made to connect the phenomena of the neuroses with the well understood phenomena of normal life. No use is made of the principles of habit; not even of the fact that the conscious responses of the present depend, in normal, and probably in abnormal life, on the responses of the past *whether those responses are remembered or not.* But then, this too, would involve a descent into "mechanistic" psychology; and habit is peculiarly anathema to McDougall.

Strangely enough, the reviewer is of the opinion that the Outline of Abnormal Psychology is a useful book, quite aside from its valedictorian significance. Like the vagaries of the magnetists, the Couéists, and psychoanalysts, it points definitely to the conclusion that the confused fields of abnormal psychology will not be cleared up until the painfully gathered data, and the laboriously careful methods of scientific psychology are applied to it, and applied by 'crude' and mechanistic' psychologists. Mythology after mythology has been applied—and has passed into innocuous desuetude. Psychoanalysis has passed, in so far as any scientific interest goes; let us give McDougall credit for applying the final blows. Of course it lingers in practice on account of its great financial profitableness. Scientific psychology, *i.e.* academic psychology, already has a vast fund of material to apply to the problems when it can obtain the chance, now denied it, to apply it. Medical psychology, when it becomes a reality, will be as thoroughly founded on 'academic' psychology as medical biology must be on 'academic' biology.

Johns Hopkins University KNIGHT DUNLAP

Social Psychology. By KNIGHT DUNLAP. Baltimore, Williams & Wilkins Co., 1925, pp. 261.

Professor Dunlap's treatment of social psychology in this volume is somewhat of a departure from that of other writers on this subject, although there has been little enough agreement heretofore as to its proper field. Much of the material he presents has generally been considered sociological or anthropological rather than psychological. At times the normative nature of his discussions seems to take him over into the field of social ethics. His method is to consider various phenomena of social life and organization from a psychological point of view, and to interpret them as far as possible in the light of psychological principles. There are chapters on sex differences, marriage and the family, civic, martial and religious organization, social progress, social organization and propaganda.

Social psychology is based, according to Dunlap, not on any single principle or phenomenon of mind, such as sympathy, imitation, or instinct, but on the total results of general psychology. Mind being "the sum total of the conscious responses or reactions of the individual," and psychology "the study of the conscious functions of the organism," social psychology is defined as "the study of whatever stimuli from other persons have contributed to our conscious lives, and to the activity which results from conscious life."

The author definitely rejects the concept of instincts, which are, he says, coming to be viewed merely as convenient logical classifications of behavior and not as explanatory factors or ultimate psychological or biological entities. The place accorded to instincts in some of the older treatments, and to 'drives' by other writers is largely taken in Dunlap's scheme by the 'desires.' The desires are regarded as organic conditions, outside of the nervous system, but capable of stimulating receptors, and therefore producing and modifying conscious reactions. Provisionally the desires are classified under the heads of alimentary and excretory desires: desires for rest, activity, shelter, conformity, preëminence, for progeny, and for sex gratification.

A social mind is held to exist only in the sense of the enumerative total of individual minds, or in the sense of an individual mind considered as typical of all in the group; in either case it involves social consciousness, or, in the individual, consciousness of being a member of the group. Social feeling and social sentiment are also important factors in the social mind.

The problem of individual differences, those of race, stock and class, comes in for discussion, followed by a full chapter on sex differences, anatomical, physiological and psychological. He emphasizes especially the types of sexual desire, emotion and sensitivity, and the differences between men and women with respect to them.

Several chapters are given over to various types of social organization. The author gives an interesting and valuable discussion of marriage and the family, largely from an historical and comparative point of view. His treatment of the origin of exogamy and incest taboos is particularly enlightening. In the chapter on religious organization he touches on the nature and origin of religion, its relation to magic, the difference between personal and social religion, the relation of the desires to religion and religious needs, and the social value of religion. The various types of civic and martial organization are discussed from the point of view of classification, function, and relation to the individual.

Social progress as dependent on heredity and training leads to a brief summary of the modern theories of heredity and the rôle and program of eugenics. The negative program, *i.e.* the control and limitation of reproduction among the obviously 'unfit' is advocated as the only practicable one at present in view of the impossibility of determining who are the 'fit' eugenically.

The principles of social groupings are to be found, the author states, in their psychological organization. The rôle of social consciousness and social feeling, of communication, language, culture, and contiguity of various types, are pointed out. Type of organization, the mob, the crowd and higher forms, and types and standards of conduct are considered. The author considers the historical views as to the nature of duty, and accepts a Kantian conclusion that it is 'an ultimate fact' of human life, a specific instance of those awarenesses of relations which are fundamental facts of experience. Generally accepted laws and conventions, with emphasis on the need of conventions to support laws, conclude his treatment of social organization. A chapter on social control, principles of propaganda, and the reduction of the latter to a definite set of rules concludes the volume.

The book contains much interesting and valuable, and some original, material. The treatment of the subject as the application of psychological principles and standpoints to what must necessarily be a brief survey of social life seems far less objectionable to the reviewer than the tendency to draw conclusions of a normative nature and the attempt to play the rôle of a social reformer. A weakness of the book lies in its neglect of an analysis of the social nature of the individual, which is assumed as an ultimate fact along with duty.

Harvard University EDWARD N. BRUSH

Collected Papers, Vol. IV. By SIGM. FREUD. Authorized translation under the supervision of Joan Riviere. The International Psycho-Analytic Library, No. 10. London, Leonard and Virginia Woolf and the Institute of Psycho-Analysis, 1925, pp. 508.

This forms the last volume of the papers thus far published by the founder of psychoanalysis; and for the non-professional reader it is the most instructive and perhaps the most important of the four volumes. Here we find the groundwork of psychoanalysis represented by such papers as "Formulations regarding the Two Principles in Mental Functioning;" "On Narcissism;" "Instincts and their Vicissitudes;" "Repression;" and "The Unconscious." In this portion of the book, Freud deigns to make indirect references to the physiological mechanism of the unconscious, and perhaps that is the reason why the eight papers of the first part are labelled by the curious name "Meta-psychology." All these essays have appeared between 1911 and 1917, and some have already been widely discussed and reviewed.

The second part of the volume, which is headed "Papers on Applied Psycho-Analysis," deals with the cultural aspect of the subject. Here Freud makes delightful excursions into mythology, drama, sculpture, philology, etc., in order to prove the value of the psychoanalytic method. His "Thoughts for the Times on War and Death" has, it seems, been published in English as a booklet. Of the other essays, the most outstanding are: "The [Antithetical Sense of Primal words;" "Some Character-Types Met with in Psycho-Analytic Work;""The Uncanny;""Dreams and Telepathy;" and the three "Contributions to the Psychology of Love," dealing with (a) special types of love-objects, as selected by men, (b) degradation in erotic life, and (c) the taboo of virginity.

It is in these three papers that Freud reveals himself as a sociologist. Nearly every page contains a challenge to the thoughtful reader. Nearly every observation Freud makes stimulates reflection, and nearly every conclusion he comes to sounds labored and far-fetched. At times the author himself is conscious of his failure to convince the critical and avoids the responsibility by statements which smack of an appeal to intuition or to an *ad verecundiam* argument. ("I do not consider it necessary to advance any justification for my method of working out my observations; here, as in the matter of anal-erotism, the aim of it is first of all to single out extreme types in sharp outline" p. 202.)

What these conclusions are, which we take exception to, need not be given here *extenso*, as they are pretty well known. Throughout the book, the incest *motif* plays an important rôle, and from this dominant there develop overtones of a great variety. When he says that "Whoever is to be really free and happy in love must overcome his deference for women," we may perhaps follow him in view of the illustrations cited—cases of psychical impotence—long ago discussed by Krafft-Ebing; but, when he adds in the same breath "and come to terms with the idea of incest with mother or sister," we feel that there is a lacuna in his reasoning.

Freud is a 'pilpulist' *par excellence.* In reach of a simple explanation of a curious fact, he travels to all sorts of regions in order to show the bearing of all his journeying on the theory he wishes to establish. His cultural range is truly to be admired. His genius for discovering problems is incontestable. His invariable return to the subject after considerable meandering is one of the qualities which distinguish him from so many of his disciples. But, with all these qualifications which set him above the majority of mental scientists, his hypotheses still require verification.

Let us take a single instance: Freud comments on the fact that tipplers do not have to change the brand of their wine in order to gratify their craving for drink as do lovers, who soon tire of their sexual objects of love, and he makes this significant utterance with regard to the alleged difference: "However strange it may sound, I think the possibility must be considered that something in the nature of the sexual instinct itself is unfavorable to the achievement of absolute gratification."

Freud does not stop to consider the difference between an *attitude toward an object* like wine and an *attitude toward a personality.* In other words, granted that the difference which Freud has acutely discerned does hold, we are not justified in believing that a sex object even for a savage is *merely* a sex object. Other factors too must be consulted; and the analogy might be instituted between the object of food and the sex object. Is not the epicure one who would have as great a variety of dishes as possible?

There are many significant passages in this volume. Perhaps the most significant, in view of the fact that Freud is often represented as a foe of every sort of repression, is the following: "It is easy to show that the value the mind sets on erotic needs instantly sinks as soon as satisfaction becomes readily obtainable. Some obstacle is necessary to swell the tide of the libido to its height. . . . In this context it may be stated that the ascetic tendency of Christianity had the effect of raising the psychical value of love in a way that heathen antiquity could never achieve" (p. 214).

Since it has been claimed in some quarters that Freud is inclined to believe in telepathy, it may be pointed out that in spite of the title of the article "Dreams and Telepathy," the author does not commit himself on the subject, and those who can read between the lines will gain the impression that his attitude toward telepathy is one of tolerance.

For the intelligent layman or non-professional psychoanalyst, Vol. IV is the most important of the entire set, and is not only of a wider scope than the other volumes but on the whole brings out deeper issues. The sociologist especially will derive a great deal of benefit from a number of the papers included in the second part. It is here that Freud approaches the world from a phylogenetic point of view; and his speculations on the archaic mind and behavior are interesting even if not convincing.

The English rendering in general runs smoothly, but in some cases it is evident that the translation or revision is not adequate. What, for instance, can one make of the following? "Is it not a fact that wine always affords the drinker the same toxic satisfaction—one that in poetry has so often been likened to the erotic and that science as well may regard as comparable." Nor is this sentence very clear: "Naturally the effect upon a woman is just as unfavorable if the man comes to her without his full potency as if, after overestimating her in the early stages of falling in love, he then, having successfully possessed himself of her, sets her at naught" (p. 211). The use of the word "instinctual" for "instinctive" we may pass over, although it is apt to bother the reader who is accumstomed to the latter form.

Cambridge, Mass. A. A. ROBACK

Fortschritte der Sexualwissenschaft und Psychoanalyse, Vol. II. By WILHELM STEKEL, in coöperation with Anton Missriegler and Emil Gutheil. Leipzig, Franz Deutricke, 1926, pp. iv, 574.

Stekel is the last of the three revolutionaries against Freud to have formed a school for himself; and the bulky periodical of nearly 600 closely printed pages is ample proof that the new school is prospering. Indeed we are told by Stekel himself that his organization includes more professional men than Freud's society in Vienna. There are about 30 articles in this annual besides reviews. The writers are apparently all medical men. Theoretical articles on psychoanalysis are discouraged. For the most part the papers are reports of clinical cases. Only three writers' names are familiar to the reviewer.

Dreams and sex symbolism in general loom large in these reports, which do not lend themselves to reviewing. The most interesting part of the book is Stekel's "Zur Geschichte der analytischen Bewegung," in which he gives an intimate account of the early conflicts in the psychoanalytic camp with details and personal references which make the reader gasp. More than once Stekel asserts that it was he who spread Freud's fame, that Freud was ungrateful to him, that he had put Freud on the right track on many occasions without the latter having acknowledged his debt. He is willing to admit Freud's superiority of intellect, but insists on exposing his narrow-mindedness, tyranny, intolerance, etc. Stekel's grievances against Freud are many. He also pleads for the other two erstwhile disciples who were lost to the master. Of course he recognizes that Freud can still point to Eitingon, Ferenczi, Rank, Jones, Brill, Sachs, Pfister, van Emden, and Reik; but he rejoins triumphantly: "How do these names compare with Jung, Stekel, Adler?"

Such open frankness may not be without its psychological value. The personal allusions, however, are not likely to raise the reader's esteem of the leaders of psychoanalysis. The impression gained by some will be that they are all a petty band of megalomaniacs lacking taste, breeding, and self-control. Of course this is an exaggeration. Those who have followed the history of the movement know that self-aggrandizement was not the only factor in the various schisms, and that Freud does not hold the respect of so many disciples solely because of the material advantages (recommendations to patients) that he commands. Stekel is apt to do himself more harm by recounting the trivial wrongs he suffered at the hands of Freud (such as coolness, ingratitude) than his alleged enemy, and, even if it is true that "the majority of practitioners come to Freud on account of Stekel," he is not justified in saying about himself in comparison with Freud: "I am the sober observer who draws his compelling conclusions out of clinically observed cases."

Cambridge, Mass. A. A. ROBACK

A Universal Scale of Individual Performance Tests: Examination Manual. By PAUL C. SQUIRES. Princeton, N. J., Princeton University Press, 1926, pp. 158.

In this manual the author presents a scale of individual performance tests which dispense completely with the use of language. He has attempted to construct tests that differentiate even under the extremely difficult situation presented by a superior homogeneous group of adults, though the scale has also been extended downward into the lower levels. In a circular accompanying the manual, Professor C. C. Brigham states, "Mr. Squires objective was that of establishing valid, non-language performance tests at college levels. Some progress was made in this direction." There are fourteen graded problem situations of various types, which may be presented without the use of language.

The apparatus necessary for these performance tests is complex and extensive. This fact in itself should, of course, in no way decrease the value of an adequate scale; but it does present obstacles in the way of validating the material. Detailed descriptions of the test material, of the procedure for administering and for scoring are presented in the manual. All directions are given to the subject in pantomime, so that this scale is in no manner linguistic.

As the author and Professor Brigham point out, the final validation of the scale would require extensive individual examinations over a wide range. Such examinations have as yet not been obtained; and it seems that the procuring of such data will be of unusual difficulty, not only because of the complexity of the test material, but also because of the time required to administer the scale to a single subject; for the period averages about three hours and a quarter (usually in two sittings), with extreme cases running over four hours and necessitating three sittings. Inasmuch as the scale is at present in a tentative state, there are no norms; and naturally so, for at the time the manual was written only a very small number of Princeton students and Vineland school subjects had been examined.

The data thus far collected have been used to calculate validity coefficients. These coefficients are not high, being in the neighborhood of .70. The reliability coefficients (by random halves) is low, being .75. Validity estimates at the feebleminded levels are considerably lower: namely, about .50. These coefficients are not encouraging; but it must be remembered that the scale is not yet in its final form and that the author hopes to stimulate sufficient interest among investigators in the development of the scale to permit widespread application for purposes of validation and standardization.

However, we may question the advisability or the need of developing non-language tests of intelligence to be used at the college level, for the linguistic ability demanded in the language tests is of a nature which any individual in college, or hoping to enter, may be expected to possess without making any undue demands upon his equipment. The advocates of performance tests at all levels make much of the 'school factor' in the tests involving language. Although no one will deny that an individual who has never attend school, nor come into contact with the materials of the schools, will be handicapped in dealing with the usual mental tests, yet it has not been demonstrated how potent the school factor is, and how exorbitant in demands on school training these tests are, especially at college levels.

Of course, for purposes of comparative studies of races, and nationalities, a non-language scale would seem to be desirable. But here we meet a difficulty in standardization, for to be a scale truly universal, the norms must be derived and the material must be validated through universal application. The difficulties of obtaining such norms and validation are patent, and we are once more faced with a problem fully as serious as that presented by language in constructing a scale for universal use.

At present the scale presented by Squires cannot be finally evaluated, for it is still in the formative stage. We must have more data and statistical studies.

Cornell University FRANK S. FREEMAN

The Psychology of a Musical Prodigy. By G. RÉVÉSZ. New York, Harcourt Brace and Co., 1925, pp. 278.

The author presents in this book his own translation, but slightly modified, of a work published originally in German under the title of *Erwin Nyiregyhazi* (Leipzig, 1916). "This work," says Révész, "the first of its kind, is an attempt to portray the development of a richly endowed artist from his earliest youth up to a definite period of his existence, to analyze his artistic and intellectual capabilities, and to unite these into an integral whole" (p. vii).

After a brief survey of Erwin's musical inheritance, early musical abilities and training, Révész proceeds with an account of the investigation which he personally carried out upon the artist from the sixth until his twelfth year. Results show that Erwin remained from 2 to 3 years above average in general intelligence, and that in both elementary and complex musical abilities, he was not only richly endowed, but also rapid in his development.

On the basis largely of this investigation, Révész proposes a theory of the general nature of 'musicality.' It is, he holds, a specific, innate entity in the individual's make-up. Musical emotions are likewise specific. In consequence of the specificity of musicality, musical creation is less dependent on general mental development, experience, and practice than is creation in the other arts.

This theory, however, does not seem well supported. In the first place, it was designed to meet the 'fact' that musical creation appears in the individual at an earlier age than does creation in the other arts. Considering the work of some modern art schools for children, we question whether this 'fact' still deserves the name. Again, Révész frequently makes statements which may easily be interpreted as modifying his theory beyond recognition. For example, the reader is left in doubt as to whether he holds that musicality depends very little, or very considerably, upon general intelligence. A more striking example occurs in his exposition of musicality on its aesthetic side. Musicality, which is "innate," "distinct" and "primarily the ability to enjoy music aesthetically" (p. 22), nevertheless, he claims, "finds expression . . . also in the course of many other activities which do not stand in any . . . immediate relation to music" (p. 23 f.). Without good evidence, which in this case is lacking, it seems arbitrary, in the face of this last statement, to call the ability to enjoy music aesthetically the *primary* characteristic of musicality; indeed, it seems extremely arbitrary to attribute to a particular musicality any of the activities in which it is said to manifest itself. Finally, Erwin's case does not seem to point to Révész's conclusions. From his theory alone, one might infer that Erwin possessed, besides his great talent for music, only the most ordinary equipment. On the contrary, however, he was above the average in intelligence; he was rich in his general emotional nature, and he had excellent musical training.

On the whole, it seems that though many sound observations are made by the author, they do not well support his central doctrine of the specificity of musical ability.

Vassar College HELEN K. MULL

La psychologie de la conversion chez les peuples non-civilisés. By RAOUL ALLIER. Paris, Payot, 1925; Vol. I, pp. 595; Vol. II, pp. 509.

This rather formidable but very interesting study of the conversion experience among primitive peoples is replete with illustrative examples drawn from missionary activities in all parts of the world. Attacking the subject systematically, the author considers the psychological problems involved in the prodromal stages of the experience, in the experience itself, and in the conditions following the crisis.

An extended discussion of the obstacles which the missionary meets in his very ambitious attempts to affect an almost complete change in the orientation point of the beliefs and conduct of the native is presented. The hostility, the language handicaps, the differences in mentality, the apparent propensity for absurdity shown by the native (M. Allier explains this by a 'paralogic' theory rather than by Lévy-Bruhl's 'prelogic' theory) are among those he takes up. The problems and mechanisms involved in the resistances offered by established habit systems, in the superficial assent often obtained, and in the tendency for social conformity, are pointed out and methods of overcoming these difficulties discussed. An examination of fatalism and magic and the more elusive and subconscious factors which slowly relate the primitive self to the new viewpoints is followed by a discussion of the more immediate elements, such as dreams and hallucinations, which enter into the conversion experience.

The actual crisis raises the problem of the reorganization of the dissociated personality about a new point. M. Allier considers the various aspects of the situation, discussing the "death" involved and the emotional phenomena which accompany conversion. He defends the theory which considers emotion as the essential phenomenon in the conversion against that which sees in it merely a means of provoking the crisis.

In the last section, devoted to the social and individual consequences of the conversion, he enters into a lengthy discussion of the psychological factors involved in such problems as the relationship between the changed individual and the environment, the battle with negative and formalistic morality, discipline, the change of habit systems, and the gradual development of autonomy.

Except for occasional notes of religiosity which mar the study, these volumes are a genuine contribution towards the understanding of the innumerable problems of primitive mentality and religious psychology. They should be of value to students of anthropology and abnormal psychology as well as to those interested in religious phenomena. They might well serve as handbooks for the missionary.

Harvard University DAVID SHAKOW

Ventilation: Report of the New York State Commission on Ventilation. By C. E. A. WINSLOW and others. New York, E. P. Dutton & Co., 1923, pp. xxvi, 620.

While the present report raises a number of ventilation problems needing further investigation, it is undoubtedly the most elaborate study of this important problem that has been made in recent years. There is not space here to present more than a few of the more significant conclusions. (1) The data show that "the effects of chemical impurities in the air of the ordinary occupied room are of relatively slight importance in their influence on health and comfort, and that the main objective of ventilation is the preservation of the proper degree of temperature, humidity, and air movement which will facilitate the elimination of heat from the body surfaces and will stimulate those surfaces by an agreeable degree of coolness and air movement." (2) No significant differences in physical defects between groups exposed to air with an average relative humidity of 28.7 per cent and 42.3 per cent were noted. In other words "artificial humidity does not produce any striking improvement in the health or efficiency of school children." (3) The use of recirculated air reduces fuel cost materially, shows no harmful effect upon the health or mental processes of pupils, but does cause marked feelings of discomfort. (4) Over-heating is one of the greatest dangers to be avoided. (5) The needed air change may fall materially under the 30 cubic feet per occupant per minute as required by present day standards. (6) Finally "we are forced to conclude that window ventilation with ample direct radiation, window deflectors and adequate gravity exhaust seems the most generally promising method for the ventilation of the classroom where local conditions permit its use." The slower air movement and the lower temperatures characteristic of this method as compared with a mechanically forced supply are important factors in this conclusion. At the same time it must be recognized that this method may not give best results where noise and dust are bothersome or where rooms are crowded as in an auditorium or a factory.

Cornell University JULIAN E. BUTTERWORTH

Our Minds and Their Bodies. By JOHN LAIRD. London, Oxford University Press, 1925, pp. 122.

The author has designed this little book for the consideration of the mind-body problem at different levels of discussion. He begins with 'the attitude of common sense,' which he says has been predominantly and persistently dualistic. He defines common sense as the trend of current opinion, untrained and uninstructed. To guard against any materialistic interpretation of his accepted form of dualism, he defines spirits as "things invisible and intangible and in no sense a subtle breath or dry tenuous flame-auras and ectoplasma."

The author's interpretation of mind is essentially animistic, and much of his discussion is given over to a criticism of purely behavioristic and mechanistic conceptions. He does not, however, agree with MacDougall's animistic conception which he holds is too similar to the Scholastic interpretation. Apparently Laird believes mind to be, not of bodily origin, but simultaneous in its origin and development with that of its body. "It seems easier," he says, "to suppose that the roots of man's spirit are not to be discovered in particular human minds, and so the disparity between spirit and matter implies the belief in some cosmic order of spirituality as natural as our minds and as real as our bodies." "It is easy to speak," he continues, "like some late-born Averroist, of a spiritual radiance, broken up, colored and particularized by the prisms of our bodies. This is to assume, however, that men's differences and their personal characteristics are fundamentally bodily. On that theory, men *are* what their *bodies* can attract and re-fashion from spiritual springs; and this quite precisely is *not* what we take our spirits to be. For we believe that although we may be sustained and aided from sources not our own, and perhaps neither mundane nor human, our principal task in this mind-body partnership is to develop our minds in accordance with their own self-accepted standards, and to use our bodies as the mind's servant and friend."

Harvard University C. A. SELZER

Personality. By R. G. GORDON. New York, Harcourt Brace and Co., 1926, pp. xiv, 302.

Personality is considered by the author from the standpoint of the 'identity hypothesis.' Physiological processes have their psychological correlates, but unfortunately, he neglects the conscious correlates on account of his emphasis upon the physiological side. The spiritual aspect of personality he adds at the end as if to counteract the accentuation of the physical that runs through most of his discussion.

Physiology is thus the dominant note. The study of temperament includes an enumeration of glandular activities with some slight mention of the psychological consequences involved. Food metabolism furnishes the only energy which acts within the personality. Talents, intelligence, and genius are functions of cortical integrations. Thought is a new emergent from eccentric combinations of impulse, depending upon inhibitions and facilitations.

The book covers too much ground. Personality is delineated in terms of *Gestalt*, engrams and engraphic patterns, relatedness, and emergence. Personality, like consciousness, is an emergent, for it "has no meaning in terms of nature alone, nor yet in terms of nurture alone, but it is an emergent from the interrelatedness of the two." Man is destined to emerge on the level of Deity.

The review of the psychoanalytic schools is concise and to the point. On other topics the author is less coherent. For the reader who wants a bird's-eye view of selected high spots in psychology, with physiological speculations to lend color to the treatment, the book would hold an appeal.

Harvard University W. F. VAUGHAN

Motion Pictures in Education. By DON CAROLOS ELLIS and LAURA THORNBOROUGH. New York, Crowell Co., 1923, pp. xvii, 284.

This volume is primarily a practical handbook for users of visual aids. As such it gives specific information regarding the types of school work in which films may be profitably employed, types of films available for instruction, and suggestions as to how they are to be used. There is also a chapter on the technique of installing and operating the necessary equipment. A part of the book is devoted to the history of education with regard to visual aids and an analysis of objections to and advantages in the use of films for instructional purposes. The results of several experiments on the relative effectiveness of different methods of presenting materials are given. In one test, abstract and concrete subjects were taught by (1) film alone; (2) a superior teacher; (3) an average teacher. The scores were 75.5, 66.9, and 61.3. Another test gave lowest score to the lesson presented by the teacher alone, next lowest score to the study of the printed lesson, third lowest to the film alone, and highest to the film accompanied by explanatory comments. The authors' generalization from these and similar data is that "while the majority of tests are favorable to visual education and may prove helpful and should therefore be encouraged, they are not conclusive unless supported by reason and sound pedagogical principles." While the book is frankly a defense of teaching by means of moving pictures, it gives the impression of being a restrained and well-balanced presentation.

Cornell University JULIAN E. BUTTERWORTH

Special Disabilities in Learning to Read and Write. By E. LORD, L. CARMICHAEL, and W. F. DEARBORN. Harvard Monographs in Education, Series I, Vol. II, No. 1. Cambridge, Mass., Harvard Graduate School of Education, 1925, pp. 76.

This monograph is a report of two case studies—one on mirror-writing by Dr. Carmichael, and one on congenital word blindness by Miss Lord—with a critical survey by Professor Dearborn of the important literature on word blindness. The survey justifies the theses offered in the studies that both disabilities are due to inhibiting habits based upon specific intellectual shortcomings and are not due to congenital defect.

Miss Lord bases her conclusion on the fact that a boy, once diagnosed as feeble-minded, showed normal reading ability five years after she gave him a period of intensive training in reading by all of the recognized methods. She also refers to other similar instances which one would like to analyze more thoroughly before accepting the generalizations to which she commits herself.

Dr. Carmichael's study of mirror-writing agrees with Ladd's claim that this disability originates in extreme left-handedness, but attributes the phenomenon to the trial and error scribble period of the process of learning to write, when the separate hit-and-miss responses made by the left-handed child are gradually integrated into wholes of the mirror-script rather than of the normal type.

Harvard University H. MOORES

Pleasure and Pain: A Theory of the Energic Foundation of Feeling. By PAUL BOUSFIELD. New York, E. P. Dutton & Co., 1926, pp. 114.

Bousfield's general hypothesis is that all life is tension, and that pain is the affect accompanying tension. All stimuli raise the tension above the normal level and therefore tend to cause pain. Pleasure is the affect accompanying a discharge of tension. Pain and pleasure are not opposed under this hypothesis but may exist simultaneously in the organism. Fear is conscious fore-pain or the anticipation of tension.

The author abandons the two primitive instincts of self preservation and reproduction, and substitutes for them the instinct for the avoidance of pain (tension). This instinct serves incidentally to preserve life. A second instinct is assumed: that by which the organism seeks to reduce tension to a minimum. The end would be death, could it be attained without tension or pain. Continual fore-pleasure serves as a control to the complete functioning of this instinct.

The libidinous instinct of Freud is not regarded as purely sexual. The energy of the libido may arise from any somatic discharge of tension and is sexual only when it flows through sexual channels.

The author's final suggestion is that there may be but one basic tendency in both living organisms and inanimate matter. All substance is in a state of tension and the basic tendency is towards a reduction of this tension.

Harvard University MORGAN UPTON

Industry and Civilization. By C. DELISLE BURNS. New York, The Macmillan Co., 1925, pp. 273.

In this book, Burns discusses the ethical principles implied by conduct in different fields of economic life. The ethics of labor, in his view, is social and class-conscious; of managers, individualistic and optimistic; of capitalists, cautious. No clear distinction is made by the author between 'good' and 'right' as applied to conduct. Judging the ethics of different classes with reference to a more ultimate standard, which he assumes to be a 'form of harmony,' Burns advocates a gradual extension of coöperation in industry.

Opposing the view of the utilitarians, among whom were numbered the classical economists, that the sole or chief motive is pleasure calculated in units, the author attributes behavior to 'the horme' which is creative and expresses itself in instinctive behavior. He indicts the minute division of labor in modern industry because it prevents adequate instinctive activity through making remote the end of labor.

Consumption is regarded as social, since no person consumes in society what he would consume if isolated, even with the same range of products. In the sense of coöperation, Burns writers of 'mind groups' which he appears to favor, thus assuming a position related to that of political pluralism.

Harvard University J. C. KELSON

Pluralistic Philosophies of England and America. By JEAN WAHL. Translated by Fred Rothwell. London, The Open Court Co., 1925, pp. 324.

This is an important and timely work, being a brief history of the new significant movements in philosophy in England and America. Mainly it is a history of pluralism, the new, democratic, social philosophy made famous by its chief American representative, William James. Incidentally, it is a history of all the more virile movements in recent American and English thought, treating of the closely related philosophy of pragmatism and explaining the background in English, French and German thought from which both philosophies sprang. Considerable space is given to Ménard and Renouvier, to Bradley and Bosanquet, to Russell and to the American neo-realists and critical realists, to James most of all and incidentally to all the lesser lights, especially in American universities. If the study is carried on in "a patch work, piecemeal fashion," this method the author thinks was advisable. A thoughtful evaluation of pluralism forms the concluding chapter.

University of Iowa G. T. W. PATRICK

Experience and Nature: The Paul Carus Foundation Lectures. First Series. By JOHN DEWEY. Chicago, Open Court Pub. Co., 1925, pp. xi, 443.

In this book Professor Dewey has made a metaphysical analysis of his views. It is a systematic development of the metaphysical implications of non-reflective experience. The book holds rich rewards for the student of philosophy, but the psychologist will find little material directly bearing on his field.

NOTES

SOME DIFFICULTIES WITH WEISS'S BEHAVIORISTIC POSTULATES

In a recent volume Professor Weiss proposes first to establish behaviorism upon certain postulates of physics and then to make it a basis for the social sciences.[1] Having definitely committed himself to a given set of postulates he can scarcely deviate without subjecting himself to the criticism of internal inconsistency which less explicit authors happily escape. It appears to me that Weiss has failed both to establish a sound theoretical basis for his doctrine, and also to remain faithful to the postulates which he has espoused. His distrust of metaphysics has not led him to master the technique of that art.[2] Furthermore, we may question whether he has really made his science, as he professes, "a study of the material, biological, mechanical, and social antecedents that are at the basis of human achievement" (259).[3] Let us consider certain fundamental difficulties both with the theoretical basis and also with the 'psychological' structure reared thereon.

I. CRITICISM OF THE PHYSICAL POSTULATES

(*A*) *The physical postulates are inadequate.* Weiss proposes to take as his basis electrons and protons (or whatever ultimate units the physicist might postulate) and from them to work out a complete psychology in quantitative terms of motion (389). This does not mean that the behaviorist deliberately ignores what is traditionally known as psychological material, such as sensations, images and emotions; instead he proposes to improve the explanation of these materials (vii). The 'psyche' and the sensations into which it is analyzed Weiss claims are but assumptions like those of the ether or the electron (44, 53, 62). They are unnecessary, however; and, moreover, they have not maintained their status in the other sciences (68, 72). They merely complicate the psychological problem because they are undefinable and immeasurable (71, 282). Thus confronting himself with two sets of assumptions, the physical and the psychical, Weiss chooses the former (34, 47, 49, 63, 70, 138 f.) The assumptions of 'physical monism' have two advantages over psychical assumptions; first, the unit of explanation, *i.e.* stimulus-response, is composed of two variables of the same entity (motion) which can accurately be measured; and, secondly, this type of explanation is common to all the physical sciences. Electrons and protons can be synthetized to atoms, molecules, protoplasms, animals, man, and even social organizations (44-45, 20, 47, 391). "The individual is the locus in the movement continuum" (392, see also 52, 142). "Human behavior and achievement are assumed to be forms of motion" (389, also 138, 141). On the other hand, the "*ultimate realities* of the professional metaphysicians seem incapable of synthesis into anything more unified than is implied by the term *uniqueness*, which can neither be demonstrated nor defined" (45).

It is difficult to understand why Weiss should set up such obviously artificial and restricted alternatives. He appears naïvely to believe that if he accepts experiences, he is bound, then, immediately to reify them into psychical substances and to regard them as a 'soul;' but that if he rejects experiences he is left with the single alternative of electrons and protons. It seems never to have occurred to him that our experiences do not necessarily imply such an entity as 'mind.'[4] Most psychologies of the past generation have no such 'psyche' and yet they treat of empirical and demonstrable phenomena. Electrons and protons, on the other hand, are constructions which physicists have inferred in order to explain certain observed facts within their realm. I say 'certain' because the physicist has so defined his ultimates as to apply only to his physical problems.[5] This behaviorist, then, rejects data common to all science for a set of limited physical assumptions.

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[1] A. P. Weiss, *A Theoretical Basis of Human Behavior*, 1925. See also his article, One set of postulates for a behavioristic psychology, *Psychol. Rev.*, 32, 1925, 83-87.

[2] See, for example, Weiss's reply to A. E. Davies (Mechanism, meaning, and teleology in behavior, this JOURNAL, 37, 1926, 2-24), A rejoinder, *ibid.*, 450-459, 628.

[3] The page references in the text refer to the *Theoretical Basis of Human Behavior* except where otherwise specified.

(*B*) *The physical postulates are tacitly rejected.* Weiss seems to have been aware that such categories as "stimulus character (meaning) of responses" (56), "the biosocial response which exists in a number of biophysical variants more or less interchangeable" (330), and "social status" are not describable in terms of his physics. He makes provision for a new basis of measuring human achievement, with the following apology.

"Because of the fact that *time*, *place*, antecedent social conditions, play a much more important role in the changes that take place in man than they do in inorganic changes, new classifications in which man is regarded as a co-operative unit in a relation of sensori-motor interchangeability with all the other members of the community, will need to be invented before the fundamental units of physics . . . . will be fruitful" (395, also 46, 54-55, italics mine).

It is hardly necessary to point out that here the postulates of physics are surreptitiously discarded. Weiss's excuse for throwing out the physical standards of measurement is "the fact that time, space, [and] antecedent social conditions play a much more important role" in psychology! But are not time and space measured physically in terms of centimeters and seconds or of the speed of light? It is not these concepts which Weiss would reformulate for psychology, but the third, "social condition" which, being independent of the specific neuromuscular items involved, is not described in physical terms.

(*C*) *The meaning of the terms 'External World' and 'Response' is ambiguous.* There could be no objection to a behaviorism which consistently followed its postulates and definitions, and accepted with good grace the limits of those postulates. But Weiss does not heed the boundaries, playing fast and loose with his electrons and protons and stretching the elastic term 'response' until it extends beyond its legitimate physical interpretation.[6]

(1) The concept of the external world is used ambiguously.

(*a*) The external world is a fiction. "Of course, the electrons and protons are metaphysical constructions which belong to the same category as 'thing-in-itself,' neutral stuff, élan vital, psychic force" (44).

(*b*) The external world is visible electrons and protons. "For the physicist, then, the electron-proton hypothesis is a guess as to what would be observed visually, with a microscope of sufficient magnifying power" (41).

(*c*) The external world is a human response. "Reality is merely the term that designates this type of relationship between responses. It is the basis of the fiction of an external world of stimuli" (43).

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[4] This was pointed out, ineffectively it seems, by Davies (*op. cit.*) and constitutes the essence of W. B. Smith's criticism of Weiss (Motion? or emotion? *Psychol. Rev.*, 33, 1926, 159-165).

[5] "Physics . . . . is unable with its limited intellectual implements, created for limited special purposes, to exhaust all the subject-matter of science." Ernst Mach, *Contributions to the Analysis of the Sensations*, 1897, 1-2.

[6] Considering this matter in another field, Robert Lowie remarks, "From one angle it might seem that a man has a right to define his terms as he will, provided only he adhere to the usage once established, ... This position is inexpugnable so long as it is avowedly no more than a personal evaluation; it is shattered as soon as it pretends to give an objective classification of the pertinent data . . . . it recalls that comical classification of some aboriginal language by which male members of the tribe are of one gender, while all other persons are lumped in a vast complementary category together with animals, plants and objects" (*Primitive Religion*, 1924, xi).

(*d*) The external world contains the qualities of experience. "To have the *sensation of blueness*, requires at least: (a) the spectral blue stimulus, (b) the specific retinal elements" (272).

"The traditional psychologists would affirm that there is in this so-called experience [perceiving a square of red paper] a quality of *redness* which is not in the stimulus, not in the sensori-motor conditions, not in the reaction, as reaching nor in the verbal naming of the color" (266, also 267, 251, 270 f.).

"In their complex forms stimuli form the objects, situations, and events and the records of the objects, situations, and events, which form or have formed the environment" (134). "The problem stimulus" (323). "The mother stimulus" (340).

(*e*) The external world is composed of movements known as light, sound, etc. "The electron-proton movements may be grouped into various geometrical types. Some movements, as those occurring in a luminous body, are transmitted enormous distances and at a rapid rate, as in light, magnetic, and electrical waves. Others are . . . . vibratory motions in air particles, as in sound waves, etc." (26-27).

The external world then is (*a*) a metaphysical fiction, (*b*) electrons and protons that might be seen, (*c*) my response, (*d*) the source of 'secondary qualities,' and (*e*) the traditionally called 'adequate stimuli.' These are certainly contradictory assertions; but it is obviously convenient to have so many places to put 'sensory qualities.' For example, by putting colors into the stimulus ("the spectral blue stimulus") he answers to his own satisfaction Professor Washburn's criticism that behaviorists explained sensational qualities by attributing them at times to the stimulus and at other times to the response (271 f.). As if to justify Professor Washburn's criticism, the qualities previously were "discriminatory responses . . . making up the environment" (197-199).

(2) The term 'response' is inflated to universal proportions. The response, or human behavior, is variously conceived as:

(*a*) Movements between electron-proton systems.

"Thus, in the last analysis human behavior is reduced to movements between electron-proton systems" (36).

(*b*) Contractile effects.

"A response is a unified group of contractile effects which as movements form the basis of the co-operative, receptor-effector (sensori-motor) interchanges between individuals. (The response begins with the chemical change in the receptor, and ends with the contractile effects)" (143).

(*c*) Bodily movements which serve as stimuli.

"For the behaviorist life, action, and the so-called psychical conditions can only be adequately studied as movements (in contractile elements) of such magnitude that they may act as stimuli and release responses in others" (152).

"The actual neuromuscular factors that are involved in human behavior are relatively unimportant as compared with the *stimulus character of the response for other individuals*" (102, also 188, 273, 289).

(*d*) Sounds, written characters, printed symbols, or meaning.

"When I respond to the voice of my fellow man I react to the series of sounds that he produces" (288).

"Differentiation can only mean that *two* different sets of movements have been made: (*1*) a subject has said *e.g.* 'this is red,—this is green,' and (*2*) an observer has recorded the movements of the subject" (195-196).

"Thus there are two criteria with respect to which human movements may be classified: (a) as neuromuscular effects of *preceding* movements, (b) as neuromuscular causes of *subsequent* movements . . . when a reaction is classified with respect to its stimulus character (meaning) either for other individuals or for the same individual at a later time" (55-56).

In this last case the "movement," as a stimulus for the same individual at a later time, seems to be printed matter.

(*e*) Experiential qualities, which in turn make up the environment.

"Those discriminations that have a high degree of uniformity between various individuals I have arbitrarily designated as elementary discriminations. . . . The following list contains the important elementary discriminations: size, shape, volume, distance, extent, intensity, direction, red, green, blue, yellow, vocality, tonality, loud, warm, cold, pressure, pain, sweet, salt, sour, bitter, fragrant, foul, resinous, burnt, fruity, spicy, movements of the body or parts of the body, up, down, right, left, posture, hunger, thirst, fatigue, duration. This list is not complete. It represents merely the type of responses that are relatively uniform from individual to individual. In combination these discriminative responses make up the objects and events of the individual's environment" (197-199).

(*f*) *Everything* is a response.

"Consciousness as an implicit form of behavior or as an obscure physico-chemical process is best described as behavior . . . the writer has made several attempts to carefully work out the behavior equivalents of the more important traditional psychological terms, but implicit reactions and subvocal speech seem to explain everything" (253).

"Thinking is the stimulus-response relation, but so is every other form of human behavior" (331).

"All human conduct and achievement reduces to *nothing but*:

(a) different kinds of electron-proton groupings . . . (b) the motions that occur when one structural or dynamic form changes into another" (50).

"Reality is merely the term that designates this type of relationship between responses" (43).

"For the behaviorist the concept of the *self* is a specific response" (203). "The solution response" (329). "At ease response" (338). "Snuggling response" (340).

Although it is good nineteenth century materialism to reduce everything to 'nothing but' something else, it remains true that what we perceive is precisely what we perceive. Furthermore, with Weiss the term 'response' has become almost as general as the term 'being.' The explanation, then, that "this is a response" has practically no significance, since no distinction is made. The differentiae are to be considered in the following sections.

II. Criticism of the Psychology

But Weiss is writing a psychology, and it is only fair that this psychology be considered apart from the more or less irrelevant and labored 'physical foundations.'

The chief criticism which our author makes of the traditional psychology is that sensations, images, and conscious states will not explain human behavior. These may be all right for picturing a cross-section of an individual; but a cross-section tells nothing about its origin or future (240). It leaves out of account the functional continuity. Psychology "must assume he [man] is the product of biological, anthropological, and social antecedents" (4, 388). Accordingly, the behaviorist proposes to investigate the ontogenetic and phylogenetic development of the educational, vocational, administrative and other activities of the individual" (191). Psychology is to be the link between the physical and biological sciences, on the one hand, and the social sciences, on the other.

(*A*) *The arbitrary concept of movement is not followed.* The first difficulty lies less in aim than in method. Weiss proposes to ignore the 'cross-sections' entirely, and to consider the genetic aspects of human behavior in terms only of physical movement. But one may well ask, upon what is the concept of "movement" based?

"All definitions are self-limiting and based on other definitions. The fundamental assumption of all science presupposes that individuals are able to understand each other, and that the limited range of definition may be supplemented by demonstration. Such fundamental concepts as movement, space, time, are best presented as a demonstration" (20-21).

Aside from the fact that it is not the concepts of movement, space, and time which he demonstrates, but only objects or events which appear to move, to have extension, and to endure, we may well ask why he does not demonstrate colors, sounds, odors, images? When it is dark one can no more see movement than one can see shadows, as Jurgen observed. All are equally dependent upon an intact organism. Weiss has rejected them, however, on the basis of his physical postulates, which, as we have seen, make a distinction between primary and secondary qualities. By granting 'reality' to movement only Weiss has ruled out all descriptive psychology. Those phenomena, which, like colors, images and odors, are not movements, cannot be taken at their face value, but have to be defined in terms of movement.

"The image is rather the functioning of internal and rather obscure motor patterns which were at one time released" (256, also 234).

"For the behaviorist, sensations and images must be *movements* and have specific sensori-motor configurations" (265).

Now if what is traditionally called blue is for the behaviorist but an obscure motor process, what is this process? If it is what non-behaviorists call kinaesthesis that is another 'sensory quality' of the same order as the visual blue. For the behaviorist it must be *another* obscure movement. However, if the traditional blue moves, the behaviorist as well as the non-behaviorist can directly observe and demonstrate that.[7]

The question now arises as to how these other phenomena are defined. Although everything that cannot be demonstrated as movement is to be defined in its terms, the definitions do not take the form merely of quantitative differences. That is, there are qualitative aspects which do not appear in the postulates. The obvious criterion, the movements of the various muscle-groups or neuromuscular patterns, is the one which Weiss starts out with (36, 56, 143, 152). We might expect that these "internal rather obscure motor patterns" (256, 271), which constitute the images and sensations, would play a large part in behavioristic psychology. Indeed, as Davies pointed out, only so long as Weiss keeps the term 'response' limited to the contractile elements does he follow the method and point of view of physics. But the bare movements are rejected; although all things may be only movement, the significant thing for psychology is not what the movements are. What is important is the stimulus character of the responses for other individuals. "The actual neuromuscular factors that are involved in human behavior are relatively unimportant" (102, also 142).

"The individual is classified not on the basis of physical and physiological properties but on the basis of his co-operative status in the social organization of which he is a unit" (142, also 275 ff., 309 ff.).

Since a number of biophysically different actions may be biosocially equivalent, and biosocially different acts may be biophysically equivalent (287-333), socially significant 'responses' cannot be differentiated according to the antecedent neuromuscular factors involved, but according to their results in establishing the individual's social status. Weiss builds a psychology by ignoring all demonstrable phenomena save movement, and then loses interest in the movements as such. We shall now consider the 'movements' as biosocially significant.

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[7] In man we have a condition in which an organism "*responds to its own responses*" (255).

(*B*) *'Social Status' is inadequate as a differentiating principle.*

The analysis of human behavior considered biosocially may be outlined as follows:

(1) "Human behavior is the totality of the response systems that establish the individual's status in the social organization of which he is a member" (143, also 35, 133, 191, 207-222).

(*a*) "The study of human behavior and human achievement is a study of the educational, vocational, administrative, recreational and personal categories of movement" (394, also 36, 135 f., 143, 191, 209 ff., 388). "This qualitative analysis is to be subdivided under two heads: (*1*) the degree of excellence or skill, and (*2*) the degree of co-operative activity in social organization" (216 ff., 394).

Participation in golf, for instance, would fall under 'recreation.' Within this class all individuals would be ranked in accordance with their excellence or skill. This constitutes the quantitative aspect of the behavioristic program, involving tests, correlations, and distribution curves. Finally, the coöperative activity which the individual displays in social organization is somehow to be given an index number. To aid in this analysis Weiss converts such concepts as language, thinking, desire, purpose, volition, motivation, and emotion into the "major response categories" (287-383).

The definition of human behavior as those response systems that establish the individual's social status falls short because it fails to take account of the conventionally organized responses, morality, tradition, and human institutions. These have elsewhere been behavioristically defined.[8] If it be argued that "the totality of the response systems" refers to these antecedent facts, the answer is that the five-fold analysis of behavior into educational, vocational and other activities is then incomplete on two accounts, since it refers only to the individual behavior. (a) There are a great many things besides the individual's behavior which establish his social status. In many countries social position is inherited. The social organization of India, and perhaps of China, does not give the individual much opportunity to establish his social status. That type of movement which establishes an individual's status in such a society where he is born into his position could scarcely be imputed to the innocent offspring. (b) The corollary of this is that an individual's acts are in part a result of his social status, and not always a cause.

Although Weiss set out to include an ontogenetic and phylogenetic account of the development of responses (191), the classification he suggests is again a cross-sectional picture, and leaves out of account the social determination of acts.

(*C*) *The classification of human behavior is sociological.* The primary difference among the sciences depends upon their points of view. The chemist, for example, views water as the compound H_2O; the geologist may consider it as an erosive agent; the physicist considers it as a fluid of a definite specific gravity. The science of psychology likewise has a definite point of view. It "seeks to describe and to understand experience and the activities of the total organism in which experience plays an essential part."[9] The analysis of human behavior into educational, vocational, administrative, recreational, and personal activities neither explains nor describes the individual's performance; it gives neither functional nor structural differences in the individual acts which get themselves thus classified. These classes are not ways in which the organism functions. Weiss has given a classification of human behavior from the standpoint of social organization, that is to say, a sociological classification.[10] Psychology has its own definite problems and point of view which are not to be confused with those of other sciences; nor is psychology the handmaid of the social sciences.

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[8] S. Smith and E. R. Guthrie, *General Psychology*, 1923, 220-239.

[9] Madison Bentley, *The Field of Psychology*, 1924, 15.

SUMMARY

This paper has attempted to show that Professor Weiss's system of psychology based on the postulates of the physical sciences encounters difficulties and inconsistencies.

I. With the physical postulates:

(*A*) The assumed postulates of the physical sciences are inadequate by definition as the basis for psychology;

(*B*) Weiss tacitly admits the futility of these postulates; and

(*C*) He maintains the appearance of consistency by using ambiguously the terms "stimulus" and "response."

II. With the psychological structure:

(*A*) The concept of movement is taken arbitrarily, and then discarded;

(*B*) The differentiating principle, establishing psychology as "those responses establishing the individual's social status" is inadequate; and finally

(*C*) The basis for the classification of behavior is sociological, not psychological.

University of Illinois RALPH GUNDLACH

EMPIRICAL PSYCHOLOGY

Professor Carmichael has recently discussed the different meanings of the term "empirical psychology" and has advocated the use of but one of the three meanings which he finds.[1] Our choice of meanings, he says, lies among the following. (1) "Empirical" may be equivalent to "experimental," for an experiment by nature is empirical: it makes use of experience by way of observation. (2) "Empirical" may mean "genetic," and more particularly "ontogenetic," as when one opposes an 'empirical' to a 'nativistic' theory, especially in theories of perception. (3) Or "empirical" may be used to indicate whatever it was that Brentano meant in his *Psychologie vom empirischen Standpunkte*, a use that Titchener has made of the term in describing the antithesis between Brentano and Wundt.[2]

This third meaning Carmichael finds vague and ambiguous. "Empirical" and "rational," he thinks, are the antonyms as applied to method. Brentano, he suggests, uses a rationalistic method, and in that is not empirical at all; but Brentano also goes to observation and experience, and to experiment where it is available, and to this extent (if I understand Carmichael aright) Brentano is an experimentalist. Thus it would seem that the third meaning either is a contradiction of terms or reduces it to the first, and may thus be dispensed with. The second meaning may, according to Carmichael, also be discarded. It is clear and distinct from the first, but it is never desirable to have two meanings for the same word. For the second meaning he thinks that the word "genetic" is adequate and in accordance with modern usage. Thus we may well, the entire argument runs, equate "empirical" to the first definition.

By this process of elimination, Carmichael is convinced that "empirical" and "experimental" should be equated, and that a fiat should issue thus restricting the polygamous term. It is my own impression that this argument tends rather to abolish, than to define, the term 'empirical.' 'Experimental' is a fairly precise and univocal term; why still risk confusion by preserving two words, which, being two, imply a distinction, when their meanings are identical?

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[10] J. R. Kantor also makes this point, *J. of Philos.*, 24, 1927, 22-25.

[1] L. Carmichael, What is empirical psychology? this JOURNAL, 37, 1926, 521-527.

[2] E. B. Titchener, Brentano and Wundt: empirical and experimental psychology, *ibid.*, 32, 1921, 108-120.

Such at least would seem to me to be Carmichael's proper conclusion, but it is not my own. I have tried during the past year in a course of lectures to see if I could proceed without the word 'empirical,' and I have not been successful. My consequent dissent from Carmichael is on two grounds. (a) I believe that Carmichael has overlooked a fourth meaning for 'empirical,' where the word carries a real distinction. (b) Hard and fast definition by fiat seems to me undesirable.

(a) 'Empirical' seems to me to be simply the adjective for 'experience,' the usual substitute for the more awkward 'experiential.' (Cf. the forms 'empiricism' and 'experientialism.') 'Experience' and 'experiment' are, however, far from being interchangeable terms. The one is the underlying substrate of science and of knowledge. The other is, even in its broadest meaning, a particular method. The method of experiment is nevertheless dependent upon experience; it is a method for getting experience to yield the generalized relationships which we call facts. It is a method of control by the repetition of isolated events with predetermined variations. It is Mill's method of concomitant variations in the concrete and in the laboratory, with the entire stellar universe as a possible laboratory. I am not seeking rigidly to define an experiment; I am merely indicating the degree of its particularity. Every experiment is 'experiential' and hence 'empirical;' but all 'empirical' methods are not necessarily experimental.

The philosophy of nature and, to a less extent, natural philosophy are empirical, but they may not be experimental. The modern philosopher who writes psychology with little or no recourse to psychological experiment may be empirical.[3] Like the experimentalist he goes to experience for his basal material, but it is to casual observation or his own personal experience that he goes and not, very often at least, to experiment. It was in this sense that Herbart wrote a psychology "gegründet auf Erfahrung" but not on experiment. Lotze's psychology was similar. I have always supposed that it was this philosophical, non-experimental streak in Brentano that Titchener had in mind when he distinguished between Brentano and Wundt as empirical and experimental psychologists.

It is doubtless this philosophical bent that Carmichael is thinking of when he hints that Brentano's psychology might be called "rational." I question, however, the feasibility of carrying this distinction through. Experiment is certainly not irrational: it is inductive logic put into the concrete, and usually artificially controlled. Moreover, both Brentano and Herbart are speaking of "Psychologie als Wissenshaft," and we cannot deny that it is from experience that they proceed by the rationalistic method. If experience is fundamental to Brentano's psychology, even though the experience does not, and sometimes can not, be made to, conform to the experimental method, shall we deny him the title of "empirical psychologist?"

I think then of 'empirical psychology' as something broader than 'experimental psychology,' something which includes 'experimental psychology.' In such a sense it cannot be opposed to 'experimental psychology.' Nevertheless, when the opposition has been explicitly formulated, as it has by Titchener, I believe that no confusion results. 'Empirical psychology,' as opposed to 'experimental psychology,' is simply that portion of experiential psychology that is neither *a priori* or wholly rationalistic on the one hand, nor experimental on the other.

That empiricism leads more readily to rationalism than does experimentalism seems to me to be simply an exigency of system-making and science, but it is a useful relationship to know. To say, for instance, that Wundt's tri-dimensional theory of feeling was in the first instance 'empirical' and not 'experimental,' is at once to suggest to the wise that it depends upon casual and individual observation and to hint that emphasis upon the rationalistic method is likely to be found.[4]

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[3] Almost any philosopher-psychologist will do for illustration; I might cite W. E. Hocking, *Man and the State*, 1926, 199-336, and especially 199-218 to the psychologist who likes to see how little the philosopher thinks the psychological experiment can accomplish.

(b) My other dissent from Carmichael lies in his attempt to control the use of the word "empirical." Let us have clearness in scientific writing, but let us gain clearness simply by being clear. In writing, it seems to me, we use, not words, but language; and the attempt to secure clarity by lawful definition as often defeats itself as it accomplishes its end. Those who write words, rather than sentences, are forever coining new terms and limiting old in order to conduct their arguments. It is seldom that these terms gain scientific acceptance; all one is apt to have is difficult exposition with obfuscation corresponding to the difficulty. But let me be concrete.

I seem to be saying that 'experimental' is the word for Carmichael's first meaning and 'empirical' for the third, and thus to be implying that "genetic" or "ontogenetic" is the term for his second. What then about 'English empiricism?' Perhaps, if my meaning for 'empirical' be accepted, it is still some kind of 'empiricism;' that all knowledge comes out of experience was Locke's fundamental thesis. Nevertheless the important characteristic of this school is that knowledge develops from and by experience; it provides a genetic theory of knowledge. Shall we turn to "English geneticism"[5] as a better term? Scarcely, for the meaning of the usual phrase is too well established; we can not violate history and tradition so cavalierly.[6]

What then about theories of perception? Shall we speak of 'nativistic' and 'genetic' theories but not of 'empirical' theories? Here I quite agree with Carmichael that the word 'genetic' is ordinarily clearer. Wundt's theory of space perception seems to me not to be greatly distorted when it is described as 'genetic,'[7] especially where his emphasis is on the development of the perception of space and not on its origin in experience. Helmholtz, however, preceded Wundt in his theory, and I should still like the freedom to call his theory 'empirical.' If one reads for example his chapter "Concerning the Perceptions in General,"[8] one sees how different is his interest. He emphasizes not the process by which visual perceptions are developed (*der unbewusste Schluss*), but the variations of perceptions from the central tendency established by the sensory stimulus in so far as these variations can be referred to the experience of the individual (and sometimes of the race). To call Helmholtz' theory a 'genetic' theory is first to miss the proper emphasis, secondly to avoid the correct implication that the theory is the direct outgrowth of English empiricism, and thirdly to be historically inaccurate where accuracy is easy.

We are all grateful to Professor Carmichael for clarifying the several meanings of the word 'empirical.' All I ask further of him is that he leave us free to use words as they may best be used in different contexts, insisting as much as he pleases that we be clear, but letting us try to achieve clarity without restrictive legislation as to definitions.

Harvard University EDWIN G. BORING

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[4] Titchener, *loc. cit.*, remarks that Brentano's psychology is largely a matter of "argument," Wundt's of "description;" but this is not to say that Wundt was never 'argumentative' nor Brentano 'descriptive.'

[5] Or "English ontogeneticism"!

[6] I should suppose that Carmichael, having equated "empirical" to "experimental," might speak of "English experimentalism" for Locke and his successors, if he were emphasizing, not the development of knowledge, but the notion of the *tabula rasa*; but such a result would surely be a *reductio ad absurdum*.

[7] Nevertheless Wundt consistently writes "*empiristisch:*" cf. W. Wundt, *Grundzüge der physiologischen Psychologie*, II, 1910, 710-716. For his use of "genetisch" and "Entwickelung," see p. 717.

[8] H. L. F. v. Helmholtz, *Handbuch der physiologischen Optik*, 1866, sect. 26 (or in the later editions or the Eng. trans.).

THE 'RANGE OF ATTENTION' EXPERIMENT

Certain differences of definition are evident with regard to experimenters working on the range of attention problem. I am particularly interested in the series of papers by Gill and Dallenbach,[1] and by Oberly.[2]

Dallenbach and the writer would seem to be in complete accord in regard to a number of points—primarily that the older examples of the 'range of attention' experiments (and Dallenbach lists 10 from Hamilton, 1859 to Kraskowski, 1913,) cannot possibly be considered to have attacked the problem of attention. This was my point of view in 1921, when I published on the subject.[3] Because of the complexity of the material and because of the form of the report, the experiment was most certainly on the cognitive and not on the simple attention level.

As a result of this opinion, Oberly approached this type of problem using simple stimuli and employing a report of the number of elements for each exposure card. He further required a characterization of conscious process following each exposure. These characterizations were based on a preliminary introspective study in which it became evident that there were three conscious patterns which might underly the type of judgment required. These were (1) an immediate process in which the elements had an equal clearness; (2) an immediate process in which the elements were grouped or did not have an equal degree of clearness, and (3) a conscious process that involved some mediating reimaging (counting). Oberly then statistically combined his results, calculating the limens for (1) alone; for (1) and (2); and for (1), (2) and (3). These he called respectively 'attention,' 'cognition,' and 'apprehension' and they became experimentally defined as follows: 'attention' is a process which is immediate and involves only one degree of clearness; 'cognition' is a process which is immediate but which may involve one or more degrees of clearness; 'apprehension' is a process which may either be immediate or mediated by reimaging. This last was most certainly what had been the basis for all former range of attention experiments.

I am sure that Dallenbach and Oberly and the writer would agree that the last two—'cognition' and 'apprehension'—are merely different degrees of cognition based upon different degrees of complexity of the mental process. But I am sure that Dallenbach and I would disagree on the first—Dallenbach would insist that this again is a lower degree of cognition while I would insist that it is as close as one can get to experimentally working with attention.

Dallenbach would emphasize his contention by quoting results of the study by Gill and himself—a study in which the stimuli were the usual ones presented to the *O*s in the range of attention experiments and his *O*s were merely asked to report if the material was of a single, dual, or multiple level of clearness. On the basis of these results, these investigators concluded that the range of attention (defined in terms of sensory clearness) was from 17 to 42 elements.

The writer is completely unable to comprehend these findings—primarily on the basis of a single criticism. He does not question that all of the elements seen were at a single level of clearness if they were so reported. But what assurance did Gill and Dallenbach have that the *O*s *saw* all of the elements on a given card. This is apparently assumed. It has been our experience that, with exposures as rapid as 60 sigma, many elements may be on a card which are not seen at all. My own and Oberly's findings indicate that this happens when stimulus cards contain only 3 or 4 elements. With cards as complicated as those containing 10 or more elements, this failure to see all of them was common rather than the unusual state of affairs.

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[1] N. F. Gill and K.M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 247-256.

[2] H. S. Oberly, The range of visual attention, cognition and apprehension, *ibid.*, 35, 1924, 332-353; The range of visual attention, cognition and apprehension experiment, *ibid.*, 37, 1926, 132-138.

[3] S. W. Fernberger, A preliminary study of the range of visual apprehension, *ibid.*, 32, 1921, 121-133.

The situation seems to be something of an impasse unless one accepts Oberly's definition of attention. Otherwise one must accept Dallenbach's experimental definition which is perfectly workable from the qualitative point of view. When one attempts a quantitative determination, however, one must have a check that *all* of the elements on a given card were in consciousness. Such a check involves either a report of number or of position or of something of the sort—all of which involves cognition and therefore raises the experience above the simple attentional level as Dallenbach defines it.

The writer is personally convinced that Oberly's experimental definition of attention as an immediate process all of the elements of which have an equal degree of clearness is as close as one may get to an experimental quantitative attack on the range of attention; and the writer is not sure that Oberly's experimental defintion of Cognition as an immediate process in which the elements appear in consciousness with either equal or different degrees of clearness is not as close as one may come to an experimental attack on the problem of the range of two level or multilevel attention. Both definitions imply the immediacy of the conscious process and one implies the equal clearness of the elements.

University of Pennsylvania SAMUEL W. FERNBERGER

DR. FERNBERGER ON THE 'RANGE OF ATTENTION' EXPERIMENT

Through the courtesy of Professor Fernberger, I have the opportunity of answering his Note, so that the readers of the JOURNAL may have his criticism and my reply in the same number.

The disagreement between us is a curious one. We are agreed upon all the experimental facts, but we are at odds regarding the terms that should be used in their discussion. The facts are these.

I. The so-called 'range of attention' experiments that have been undertaken during the past 100 years have been concerned with the determination of the number of objects that can simultaneously or successively be correctly apprehended. The justification of designating these experiments as 'range of attention' experiments rests upon two assumptions: (*1*) that all processes attended to are reported; and (*2*) that all processes reported had been attended to. In an experiment upon "Attributive *vs.* Cognitive Clearness,"[1] I discovered (1) that neither assumption was correct: clear processes are not always reported, nor had the processes reported always been clearly perceived; and (2) that many other factors besides clearness of experience condition the report.[2] I therefore felt justified in concluding: "the statement in current textbooks that the 'grasp of visual attention covers from four to six simultaneously presented simple impressions' is a statement concerning visual apprehension, not attention."[3] In a study made approximately at the same time, Fernberger reached similar conclusions. "From a consideration of the introspections we are convinced," he says, "that the range of attention is an erroneous title for this sort of experiment."[4]

II. The next step was taken by Oberly[5] who, working under Fernberger's direction, "attempted to differentiate the thresholds of attention, cognition and apprehension.' "[6] He followed the traditional procedure in his experiments, but in addition to the report of (*1*) "the number of dots apprehended," he required his *Os* to give (*2*) their "degree of assurance or certainity," and (*3*) "the method of determining their report."[7] His *Os*, under (*3*), differentiated three conscious patterns, which he characterized as "immediate," "grouping," and "counting."[8] Using the reports that were correct, and that *O* was 'sure enough about to put even money upon,'[9] Oberly computed, by the method of constant stimuli, limens for every one of these categories.

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[1] K. M. Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 183-230.

[2] *Ibid.*, 213-233, 228 f.

[3] *Ibid.*, 229.

[4] S. W. Fernberger, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 121-133.

[5] H. S. Oberly, The range of visual attention, cognition and apprehension, this JOURNAL, 35, 1924, 332-353.

There is no disagreement between Fernberger and me regarding Oberly's data. I have, now that Oberly has pointed them out, repeatedly observed the three conscious patterns that he has described. Of Oberly's experimental findings there is no doubt; of the interpretation of his data, however, as much cannot be said. Because the characterizations 'immediate,' 'grouping,' and 'counting' did "not correspond to the systematic categories that have appeared in literature," Oberly 'transformed his results' into attention, cognition, and apprehension.

He transformed the 'immediate' pattern into 'attention' because his *Os*' description of this pattern "agrees with the definition of attention as given by Titchener;"[10] the 'grouping' pattern into 'cognition' because "Dallenbach found in an experiment on sensory clearness that two levels were possible, and . . . [that] the experiment dealing with two such levels is an experiment in cognition,"[11] and also because Dallenbach, "associates the 'lower level' of clearness with cognition;"[12] and the 'counting' pattern into 'apprehension' because "Fernberger has so defined the range of apprehension."[13]

As I pointed out in a note published in criticism of Oberly's paper[14]—to which, by the way, Fernberger make no reference—the transformation of his first two patterns is not justified and Oberly misunderstands both Titchener and me. Attention for Titchener is a conscious pattern that is divided into the clear and the unclear.[15] It is not an cognitive term. No one, not even the functional psychologists, regards attention as a level, a step, or a degree of cognition, and yet that is just what Oberly makes of it when he designates the 'immediate' pattern as attention. His *Os* must *cognize 'number'* before giving a report. His experiments were therefore upon the cognitive level.

Curiously enough, Fernberger makes me "insist that this [Oberly's 'immediate' pattern] is a lower degree of cognition."[16] Quite on the contrary, I believe that it is the highest degree of cognition with which Oberly was concerned. Oberly's results, as do the results of all the 'range of attention' experiments, show that the constant varies with the degree of cognition. The higher the degree of cognition, the smaller the constant; the lower the degree, the higher the constant. When the *Os*, for example, are required to name the stimuli exposed the constant is about 5; when they are required merely to report the number exposed the constant is about 9.

III. Cognition is inherent in report, and the degree of cognition required for the report determines the range of the report. It may very well be, therefore, that the question of range is not a proper one to ask regarding attention. Nevertheless Miss Gill and I thought that by restating the question so that the problem became one of discovering how many stimuli may be placed upon an exposure card that should correlate 50% of the time with one level of clearness, and that by requiring the *O*s to report, not upon the stimuli themselves, but upon the distribution of the clearnesses of the impressions, we should be able to eliminate the cognitive complication, or at least to reduce it to a minimum, and thus obtain a constant that would be significant for attention.[17] Our study led to positive results: "under the conditions of our experiments the individual limens varied from approximately 17 to 42 stimulus objects."

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[6] *Ibid.*, 333. [7] *Ibid.*, 335. [8] *Ibid.*, 333. [9] *Ibid.*, 336. [10] *Ibid.*, 332, 335, 347. [11] *Ibid.*, 348. [12] *Ibid.*, 333. [13] *Ibid.*, 350.

[14] Dr. Oberly on 'The range for visual attention, cognition and apprehension,' this JOURNAL, 36, 1925, 154-156.

[15] Cf. E. B. Titchener, *The Psychology of Feeling and Attention*, 1908, 220 ff.; *A Text-Bookof Psycholgy*,,1909, 266 f.

[16] *Vide supra*, 478.

[17] N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 249.

Fernberger says that he is "completely unable to comprehend these findings" because he finds no assurance "that the *O*s *saw* all the elements on a given card."[18] Truly, we have no such assurance, and what is more to the point we did not care whether our *O*s saw all the elements, half of them, or only a small fraction of them. All the elements had the same opportunity of being seen; beyond that our interest did not go. The question whether all the items were seen no more concerned us than the question whether all the grams in a 100-grm. weight were sensed would concern Fernberger in a lifted-weight experiment. The method of constant stimuli, as Fernberger knows, is adequate to such subjective variables. What we were solely interested in was to discover, by the method of constant stimuli, the number of dots that would correlate 50% of the time with one level of clearness.

Fernberger is no more interested than we are in the number of dots that the *O*s saw. Both he and Oberly had to face the same problem in their studies upon range, and they solved it in exactly the same manner that we did—by computing their limens by the method of constant stumili. They both used in their computations only the reports that we were objectively correct. If, for example, an *O* reported 8 dots when a stimulus card of 10 was exposed, it was counted as incorrect, although, as Fernberger points out, 8 may have been the actual number seen. It mattered not at all to Fernberger or to Oberly how many dots were seen; the thing that interested them was whether the report was correct. So also with our work: it mattered not to us how many items were seen; the only thing that we were interested in was whether the report was of one or more levels of clearness.

Oberly is explicit in his definition of attention, and it may seem to many as though he should have the right, as long as he is consistent in his usage, to define terms as he will; but until he can point to a system of psychology (or give us one) in which attention is regarded as a level or degree of cognition he is not justified in altering the accepted usage simply to classify his own data. To do so only compounds the confusion which already exists.

Cornell University KARL M. DALLENBACH

THE TWENTY-SECOND MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The twenty-second meeting of the Southern Society for Philosophy and Psychology was held in the University of Georgia, Athens, Georgia, April 22 and 23. This is the home of the president of the Society, A. S. Edwards, who had laid careful plans for the meeting and had arranged to have exhibits of psychological apparatus by C. H. Stoelting and Company and of standard tests by the World Book Company, and a preschool exhibit by B. T. Baldwin (Iowa). After a short address of welcome by Chancellor Snelling of the University, the program was begun by a report on further experiments on the problem of sleep by H. M. Johnson (Pittsburgh). By means of his well known methods of recording bodily movements of subjects during the hours of sleep he had found that with better sleep at night there was associated a loss in efficiency in the substitution test the next morning, but that on the following evening efficiency in this test was increased. The curves of shifts in position during the night show not only a rapid decrease in such activity soon after going to sleep, reaching a minimum point at a little over one hour and thence rising gradually toward the hour of waking with acceleration toward morning, but also somewhat regular rhythms in amount of movement throughout the night. These rhythms hold a somewhat constant relation to the time of going to bed. One experimental group put to bed earlier yielded results showing that these rhythms shifted forward in correspondence with the earlier time of going to sleep.

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[18] *Vide supra*, 478.

R. M. Dorcus (Hopkins) reported the results of experiments on the effects of fixation of the head upon post-rotation nystagmus time as contrasted with non-fixation. Ten subjects were used of whom five were rotated with heads free and five with heads fixed. The latter group showed the greater reduction in nystagmus duration after rotation. G. C. Bassett (Kentucky), who had observed his own delusions during delirium in a spell of illness, reported the fulfillment in the delusions of certain childhood desires which had been denied expression. The desires to have roller skates, suppressed by parents because of fear of danger to the head, to join the navy and become a sailor, and to build railroads and become a train dispatcher were appropriately provided for in the dreams of the delirium. Subject found himself skating with head protectively wrapped in a pillow to prevent injury; he experienced sailing dreams involving capture by pirates and later escape; and recognized himself as building and operating railroad systems. He contended that these dreams were not of a sort demanded by the Freudian doctrine; sex drives were entirely absent and dreams were expressions of unfulfilled wishes rather than of suppressed desires. It developed in the discussion of the report that since these wishes were often indulged in during childhood years, the dream revivals of the earlier pictured fulfillment may have been shaped in a measure by frequency laws and intensity of image; also that the memory report after the delirium might be influenced in a degree by selective factors emphasizing content that had obvious relations to past experiences. Buford Johnson (Johns Hopkins) discussed the activities of two-year-old children as observed in the new child laboratory under her direction. Forty children of one and one-half to three years of age had been under observation both while restrained under certain test conditions and while unrestrained. These children were found to indulge in activities mainly of speech and of motor coördination. It was reported that delay in the development of these motor coördination activities lead to later lack of social adaptation. Studies of problem children also revealed early hamperings of these childhood activities which normally establish basic coördinations. In the discussion that followed it was admitted that the relation between hampered early activities and later mal-adaptation was not established as a causal one, but that both conditions may in part at least be results of some common factor. Even if this should prove to be the case, however, it would not negate the importance of the early activities.

Joseph Peterson (Peabody College), ending the forenoon program, gave a report as Chairman of the Committee on the Status and Teaching of Psychology. Mimeographed tables of the qualifications of 302 teachers of psychology and of psychological books and journals available in 158 institutions in sixteen Southern states were distributed for study and discussion. It was shown that the more foreign to psychology the training of persons actually teaching this subject had been the more willing they seemed to be to undertake such teaching with limited academic training.

Two-thirds of the 158 institutions reporting had only two or less than two psychology journals available to students. The committee emphasized the need of campaigns to increase the number of psychology journals and books in the numerous small colleges now offerring work in this line. A mimeographed list of all coöperating institutions in the sixteen Southern states, which reach the standards outlined by the Society in its 1926 meetting was also presented for discussion and correction. The institutions were arranged alphabetically by states, and specifications were made as to whether all courses, or only elementary courses, met the standards. In cases where some, but not all, instructors were properly qualified the work of those instructors only was approved. In the discussion it was brought out clearly that there may be serious injustices in the rating of instructors merely on the basis of their academic training or degrees and on published work; but it was agreed that with due care these injustices could be largely avoided and that a published list of the kind presented, distributed to university registrars and to professors of psychology in higher institutions of learning would do much good toward the prevention of transfer of credits in the case of unsatisfactory work. The work of the committee was thought to be invaluable toward the establishment of better standards of psychology work. The list of institutions meeting the standards of the Society will be worked over thoroughly and published in due time. It is important that all qualified institutions be on this list.

The afternoon program was devoted to philosophy. A. O. Lovejoy (Johns Hopkins) read a paper on emergent evolution. The roots of present views were traced and it was asserted that emergent evolution theories are neither affirmations nor denials of possibilities beyond our present realm of experience. We must be open to facts as they come. Important questions are what sort of new events appear and how they come. Emergences may be either functional (novel manifestations of old types of events) or existential (real additions to what actually existed before). Just what constitutes new events as distinct from merely new manifestations of old events is a question not easy to solve and one on which different answers are given by different authorities. Some writers maintain, for instance, that only psychical, or conscious events are but new mainfestations of old events. This is the question as to whether dualism can be established on a factual basis. The paper was too detailed and replete with content for adequate summary and may profitably be read in full by psychologists. In the discussion Lovejoy held (against Sanborn) that prediction is possible without quantitative analysis. C. G. Thompson (Emory) as to the status of the 'value judgment' held that philosophers used to regard value judgments as having reference only to ultimate realities, but admitted that, like everyone else, they have found it difficult to get beyond their world of experience. All knowledge is an organization of events in our world. If our knowledge does not reach reality, we have no knowledge; were this true we could not even think. Religion has been as agnostic as philosophy, looking beyond the world for reality. The universe is really coherent and in it (not outside of it) all things have their place. To the question, has the autonomy of the individual anything to do with his behavior? Thompson answered, the individuals' response might be different if his organism were different. E. H. Henderson (Georgia) gave an exposition of Vaihinger's philosophy 'As if,' holding that this instrumental view was not to be identified with pragmatism. Since this was an exposition only it is unnecessary to summarize here. H. G. Sanborn (Vanderbilt), discussing methodology and psychology, held that science has to do only with quantitative prediction. Science is not, as is often claimed, without presuppositions; every science begins with assumptions which it does not and as science cannot investigate, and it ends with results which it as science cannot evaluate. Practical science simplifies the complexity of the world as presented empirically and renounces knowledge in order to predict. All methods must be either mechanical in the mathematical sense or vitalistic. The only evolution we understand at first hand is psychic evolution; mechanical evolution is a contradiction in terms. If there is real evolution it is not mechanical. All evolutionary doctrines represent an attempt to extend a truly functional or psychological method downward, as the mechanistic method has been extended upward from physics to history. There is no valid objection to an extension of either of these methods into the realm of the other as all methods are relative to their purposes. Lack of time prevented the discussion of this paper which was scheduled.

In the presidential address, given in the evening after the dinner, Edwards reviewed the various definitions of intelligence and added one more, the capacity for variability, or versatility, of response. He did not bring out the common ground in many statements of what intelligence is, and neglected, unfortunately, to include with his own theory, not a new idea so far as variability is concerned, the fact of some sort of organizing function.

In the Saturday morning program R. L. Bates (Virginia Military Institute) discussed, in connection with the presentation of data which he had gathered, the complexity of factors leading to the withdrawal of students from college. These factors were shown to operate in various combinations making prediction of an exact sort difficult. Joseph Peterson (Peabody College) presented data from experiments on the effect of practice on individual differences. It was shown that correlation methods alone were inadequate here, and that the coefficient of variability, though not without weaknesses, was indispensable. An historical survey showed that most practice effects in different kinds of performances had resulted in the subjects' becoming more alike, but that in certain complex motor coördinations, like ball-tossing, there was wide divergence with practice. An experiment on twenty 12-year-old children in Peabody College gave divergence in substitution practice and in mental multiplication, and convergence in complex card sorting. Extended to 96 subjects of a more heterogeneous nature—one of the Peabody experiments applied to Idaho children by M. C. Barlow—the mental multiplication practice yielded convergence. It was found that training in most school subjects, including psychology, also gave decrease in the variability coefficients; this, however, may be accounted for in part by the fact, brought out in data from Myers Mental Measure on large numbers of children of ages from 6 to 14 years, that marked convergence in variability of mental test scores through successive years occurs. Studies extending through a considerable length of time may be influenced by a natural convergence of individual differences with age. P. F. Finner (Florida State College) followed with a paper on the variability of individual performance. Using a variety of performance tests he obtained wide ranges of variability coefficients on the same individuals in different performances, varying from .07 to .18. He interpreted this wide variation as due to the fact that some processes practiced were newer to the individual than the others. The variability coefficients of the several individuals regularly decreased with practice, a fact, he pointed out, which does not contradict any of the results of the previous paper which dealt with changes in percentage variation in groups. It was maintained that the variability coefficient was significant and that every student should know not only his level of performance in different activities but also his degree of variability in performance. No relation was found between the level of efficiency in performance and the variability coefficient.

W. W. Reitz (Georgia) showed, in carefully prepared charts of students' average college grades as related to their intelligence test performances, that several factors lowered the correlation. Students with high abilities as revealed by the tests were able to get along in their college work with little effort while in numerous cases those scoring lower had worked hard to compensate and had consequently made great gains in scholarship during the year. Lines connecting the ranks of individual students in intelligence tests on entrance with rank in college subjects at the end of the year crossed very much, connecting high points on one side with low ones on the other. Interpretation was, however, confused with facts, the author assuming too much for the accuracy of placement as to ability by the intelligence tests and explaining divergences between scores and college grades as due to differences in effort, a point to be established. The results were admittedly marred by the fact that the different instructors assigning the grades had no uniform system of grading. Paul Boynton (Kentucky) presented results of a moral judgments test, which he rightly distinguished from a test of morality. The test was his own construction. Applied to college students it showed a certain reliable sex difference as to standards regarding the use of stimulants, and the appropriation of the property of others, but differences in judgments as regards the relations of the sexes were not reliable. S. C. Garrison (Peabody College) reported on the reliability and validity of certain group intelligence tests. Two hundred forty-nine children in the Peabody Demonstration School, distributed through grades of 4 to 10 had been given a number of different intelligence tests. These tests were administered at year intervals during a period of 5 years. Every child was thus tested 6 times but, due to irregularities in attendance, no one particular test was given to all the children. In all, 10 tests were administered and the smallest number of children taking any one test was 66. As a measure of the validity of any test the following criteria, weighted equally for a composite score, were used: The Stanford-Binet I. Q., a composite intelligence test score, teachers' judgment of ability, and a composite educational test score made from 37 different measurements, each of these also being given equal weight. The results of the correlation processes placed the National and the Terman tests at the top in reliability and validity, both for an interval of one year and of five years. Dearborn Series II and Haggerty Delta 2 ranked at the lower extreme. E. Bagby (North Carolina) analyzed two cases of simple hysterical reactions pointing out what he called both functional and organic factors. He emphasized the importance of showing in detail what all the contributing factors of such reactions are, rather than simply attributing the finally resultant reactions to a disease entity 'hysteria.'

At the business meeting Saturday morning Virginia Military Institute, Lexington, Virginia, was designated as the place for the 1928 meeting and Josiah Morse (South Carolina) was elected president for the next year. New council members elected were L. R. Geissler (Randolph Macon Woman's College) and R. L. Bates (Virginia Military Institute). A motion by Lovejoy empowering the new president to appoint a committee on the teaching of philosophy was passed. Considering the fact that the meeting was held in the far South the attendance was good. A number of the west Southern states were, however, not represented.

Peabody College JOSEPH PETERSON

THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The second annual meeting of a group of midwestern experimental psychologists was held on May 14, 15, in Wieboldt Hall on the McKinlock Memorial Campus of Northwestern University at Chicago, Illinois. The program opened on the afternoon of the first day with a session which was devoted to a discussion of the content and subject matter of psychology and its relation to the other sciences. The most important changes in psychological theory and practice during the last twenty-five years were also described and some ideal experimental programs for future investigation were suggested. Professors D. T. Howard (Northwestern), C. L. Hull (Wisconsin), M. F. Meyer (Missouri), Karl Schneider (Wittenberg), L. L. Thurstone (Chicago), and A. P. Weiss (Ohio State) and others participated in the discussion. After the informal discussion the group was pleasantly entertained at tea in the rooms of *The Monist*.

In the evening the chairman of the group, Professor A. R. Gilliland, introduced Dean G. F. Arps, who gave an address on the subject of laboratory psychology, urging more interest and attention to a thorough-going laboratory course in psychology as a preliminary to all further research as well as an integral part of the elementary course. After the address a committee was appointed to consider the question of organizing the group on a more permanent basis. Professor Gilliland appointed Professors Weiss (chairman), Carr, Hull, and Ruckmick to consider this question and to report.

The next day there were two concurrent sessions during which about a dozen papers were presented on experimental results and interpretations. In one session presided over by Professor J. J. B. Morgan the following papers were presented:

M. Metfessel, Phonophotographic Studies of Negro Singing;

S. R. Hathaway and J. P. Porter, An Improved Psychogalvanic Apparatus and Preliminary Findings;

C. W. Darrow, The Significance of Blood Pressure and Galvanic Changes in the Study of Emotions;

H. M. Johnson, Some Further Experiments Bearing on the Problem of Sleep.

At the other session, at which Professor C. A. Ruckmick presided, these papers were offered:

H. Woodrow, The Effect of Duration of Limiting Sounds upon Estimates of Short Temporal Intervals;

A. G. Bills, The Influence of the Quantitative Set upon Initial Efficiency

E. Culler, Nature of the Learning Curve;

E. Jacobson, Productivity of American Psychologists;

Valentine, Visual Perception in White Rats.

Later in the morning opportunity was afforded for inspection of the medical laboratories and of the law building in the new imposing units recently erected on this downtown campus. Afterwards Professor L. L. Thurstone read a paper on: A Criticism of the P.E. Unit in Scale Construction.

In the afternoon, which was a four-hour session, Professor C. L. Hull who presided called for reports of research that were being carried on at the various laboratories represented, including the University of Chicago, Clinic for Juvenile Research at Chicago, University of Illinois, Indiana University, University of Iowa, Iowa State College, University of Minnesota, Ohio State University, Ohio University, Ohio Wesleyan University, Washington University, University of Wisconsin, and Wittenberg College.

In the evening of the second day a banquet was held in the rooms of the Women's City Club at which President Walter Dill Scott was toastmaster and Professor H. A. Carr read a paper on 'Teaching and Learning.' In this paper he showed by means of experiments on white rats and on human subjects that under the conditions of the procedure often guidance in learning was less effective than punishment, but that in other series too much guidance was less effective than none at all. At this banquet, attended by approximately seventy-five psychologists, the report of the committee mentioned above was read and approved.

This report provided for a permanent organization known as the Midwestern Psychological Association, composed of those members and associate members of the American Psychological Association who have registered at any annual meeting of the former organization. Professor A. R. Gilliland was elected president for the current year and an executive council was provided for composed of Dr. C. L. Hull, Dr. M. Metfessel, Dr. S. Renshaw, and Dr. L. L. Thurstone. The duties of the president and the executive council were covered in the by-laws which were adopted. It was decided that the organization meet next year at the University of Wisconsin. Appreciation for the unusual facilities and courtesies provided were expressed by a rising vote of thanks.

The meetings were unusually well attended by about two hundred psychologists and guests and the papers were in the main of an experimental nature based upon data either in process of investigation or obtained from recently finished work. The papers are too numerous to be reported in detail, but the more valuable ones will undoubtedly be published in due course.

University of Iowa CHRISTIAN A. RUCKMICK

TRENDS IN CHINESE PSYCHOLOGICAL INTEREST SINCE 1922

The trend of psychological interest in China since 1922 is clearly shown by the type of the articles published in *The Chinese Journal of Psychology* that was founded in January 1922 by the Chinese Psychological Society. The *Journal* is edited by the president of the Society, Professor Y. C. Chang of Peking National Normal University. It is published irregularly, but has now reached its fourth volume.

The following is a complete list of the subjects of the articles which have appeared in the *Journal* up to and including the March 1926 number. The classification is the original one adopted by the *Journal*. Most of the articles are elementary introductory discussions.

Abnormal. History and content of abnormal psychology, psychoanalysis (2 articles), dreams (2 articles), interpretation of dreams, arguments against the existence of prophetic dreams, criticisms against Coriat's theory of the interpretation of dreams, Freud's dream theory.

Adolescence. Juvenile crime (2 articles), sex problems, sex psychology of Chinese students.

Aesthetics. Translation of article on the psychology of music by J. E. Weathery.

Applied. Principles of applied psychology, physiognomy of Chinese handwriting, work and efficiency.

Animal. Psychology of crow, pigeon, ant; animal intelligence, recognition of nest-mates by ants, red and anger in domestic animals, animal instincts.

Biography. James, Wundt.

Business. Psychology of Chinese merchants as seen from 5000 signboards.

Child. Methods of studying children's knowledge; color appreciation; learning to reason; suggestibility in children; interests of Chinese children; fear, interest, and the moral sense; history of child study.

Educational. Origin of educational tests; tests for recognition of Chinese characters; experiment on Chinese phonetics; reading test; Chinese philosophers on human nature as good or bad; American standardized educational tests; a test of educational conditions in South Manchuria; measurement of elementary school handwriting by T. scale; three types of educational scales; science test for secondary schools.

Experimental. Geometrical illusions, conditioned response, mirror drawing, eye movements in reading.

General. Work and fatigue; psychology of fancy; names for noises found in Chinese literature (2 articles); mental imagery of several great Chinese writers; instinct (3 articles); attention and interest of Chinese poets (2 articles); genesis of emotion; a statistical study of the colors in national flags of the world; 203 special names for colors found in Chinese literature; 267 more color-names in Chinese literature; words and thoughts; first memories in life; self and soul; emotion in Chinese new poets; preference in the use of the ten figures; hair-fetishism and brow-fetishism; Henning's experiments on smell; the attention value of Chinese sparrow-cards; behavioristic interpretation of thinking; a new interpretation of the formation of concepts; classification of laughter and its physiology; anger; mathematical method of the associative inference of psychology.

Heredity. Transformation of silkworm eggs into goldfish (2 articles).

History. Of psychology (4 articles); modern psychological movements; evolution of modern psychology; latest developments of Russian psychology; German schools of psychology.

Intelligence Tests. Origin, history, use; Courtis arithmetic test; definition and limit of intelligence; experiment on "pa-kua;" Japanese adaptation of the Binet-Simon tests; a list of American group tests; the "fruit test;" elementary school common-sense test; intelligence tests and education; Chinese group tests; national intelligence tests of Japan; translation of article by Pintner on comparison of American and foreign children in intelligence tests.

Psychological terms. Standardization of Chinese terms in psychology.

Psychological tests. An emotion test; psychological tests found in China during the Han and Wei dynasties (206 B. C.–264 A. D.).

Religion. Child's conception.

Senile. Learning capacity of an aged person.

Social. Test of people's opinions (2 articles); Chinese superstitions (3 articles); a statistical study of Chinese words expressing virtues and vices (2 articles); correlation of name and fame; geographical, educational, and vocational distribution of 750 leading contemporary Chinese; geographical distribution of the graduates of the Imperial Academy of Learning during the Ching dynasty (1644–1911); the psychology of fortune-telling.

Systematic. Buddhist psychology; introspective *vs.* behavioristic psychology; Wang Yang-Ming's psychology (1472–1528); life of Freud and progress of his thought; Chu-tse's (1129–1200) and Hsin-Tse's (about 600 B. C.) thoughts on psychology.

Vocational. A vocational study of Peking Middle School seniors; vocations preferred by 850 Chinese school children.

Stanford University SIEGEN K. CHOU

WILLIAM ALLEN NEILSON CHAIR OF RESEARCH

The establishment of the William Allen Neilson chair of research at Smith College, Northampton, Mass., has recently been announced. The chair is the gift of friends and admirers of Dr. Neilson in honor of the tenth year of his presidency of Smith. It was presented by Mr. George Bliss McCallum of Northampton, a member of the Board of Trustees, speaking for the donors.

The terms on which the chair is established are somewhat unusual, since the holder is not expected to teach and is left absolutely free to study and experiment as he pleases, publishing or not as he prefers.

The chair will be held for five years by Professor Kurt Koffka of the University of Giessen, Germany.

Edward Bradford Titchener

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Edward Bradford Titchener

1867-1927

ASSOCIATE EDITOR 1895-1920; EDITOR 1921-1925

After thirty-five years of continuous service to psychology at Cornell University, Edward Bradford Titchener, the dean of experimental psychology in America,[1] died unexpectedly in Ithaca, New York, on August 3rd, 1927, at the age of sixty. The death of no other psychologist could so alter the psychological picture in America. Not only was he unique among American psychologists as a personality and in his scientific attitude, but he was a cardinal point in the national systematic orientation. The clear-cut opposition between behaviorism and its allies, on the one hand, and something else, on the other, remains clear only when the opposition is between behaviorism and Titchener, mental tests and Titchener, or applied psychology and Titchener. His death thus, in a sense, creates a classificatory chaos in American systematic psychology.[2]

Titchener was born on January 11, 1867, at Chichester, England, an old Roman town with walls still standing, about seventy miles south of London and not far from the coast. There is an insistent historicity about this little town that accords well with the historical cast of Titchener's mind in adulthood.[3] The Titcheners were an old Chichester family. In 1532 John Tychenor, was the head master of the Prebendal School in Chichester, which our Titchener attended in boyhood. William Titchener, the great-grandfather, was mayor of Chichester in 1832, and lent money to the city in return for which he received a lien on the regalia of the city. His son Edward (the grandfather) was a distinguished lawyer, according to a long list of appointments which he held. He is said to have run through several fortunes and to have died relatively poor, having been obliged to move from a residence in an old abbey, Greyling Wells, to a smaller house. He had two sons, William and John; and John was the father of our Titchener. John was the second son, but anyway, there was no longer a fortune for him to inherit. He went on an adventure to America at the time of the Civil War, and returned to England, after fighting with the Confederate army. In 1866, he married Alice Field Habin of Kingsham, which is not far from Chichester, and in 1867 Edward Bradford was born. John Bradford died not many years afterward while still in his thirties. Edward Bradford Titchener therefore had his own way to make. There was no parental assistance forthcoming for him; he had, so far as his family was concerned, simply the consciousness of a very respectable descent.[4]

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[1] E. B. Holt's phrase, in reviewing Titchener's *Text-book*, was "the dean of American empirical psychology," *Psychol. Bull.*, 8, 1911, 25; but Titchener's name is since connected with the creation of an antithesis between 'empirical' psychology (Brentano) and 'experimental' psychology (Wundt, hence himself).

[2] This biographical sketch is indebted throughout to many of Titchener's friends and colleagues who have been glad at this time to revive memories and to impart information. Besides Mrs. Titchener, I should mention especially Professors Frank Angell, Warren, Pillsbury, and Dallenbach.

[3] He himself once suggested this relationship.

When he was about fourteen he went on a scholarship to Malvern College, an important English public-school, then about twenty years old. The college is in Worcestershire, over a hundred miles from London and very near Wales. There he spent four years.

After Malvern he went to Oxford in 1885, and became a member of Brasenose College. For four years he was senior scholar of Brasenose in classics and philosophy and senior Hulmian 'exhibitioner' (a term applied to the holder of what, in America, is called a scholarship).[5] In his fifth year he became a research student of physiology under Burdon-Sanderson, either because of the attraction of the subject or of the man, or perhaps because of both. He always expressed a great admiration for Burdon-Sanderson and acknowledged an intellectual debt to him. His first book he dedicated to him. He received the A.B. degree from Oxford in 1890, when he was twenty-three years old.

The conjunction of philosophy and physiology in Germany had produced physiological psychology and it is not strange that the combination of these two interests at Oxford should have led Titchener toward the new psychology. It was really quite new, much newer than the thirty years since Fechner's *Elemente* might lead one to suspect. Wundt's laboratory at Leipzig had been in existence only eleven years. On the other hand, interest was growing rapidly. Wundt's *Philosophische Studien* was in its sixth volume; the *Zeitschrift für Psychologie* had just been begun; Stumpf was completing the *Tonpsychologie;* Ebbinghaus' experiments on memory were five years old; G. E. Müller had begun on psychophysics; there was plenty to read in German. America was beginning to follow suit. Hall had opened the first formal laboratory at Hopkins, based on the German model, had gone to Clark and had begun another there. Cattell, back from Germany, was experimenting at Pennsylvania. Three other American laboratories had started and three more were about to be gotten under way. *The American Journal of Psychology* had reached its third volume. Ladd's *Physiological Psychology* was three years old, and James' *Principles* was to appear in the fall. The year before there had been the first International Congress of Psychology at Paris with one hundred and twenty psychologists in attendance. England was slow to learn from Germany, but the founding of *Mind* by Bain in 1876 had been in a sense a recognition of the birth of a new psychology. It was at such a time that Titchener, trained in philosophy and physiology, went to Wundt at Leipzig to study experimental psychology.

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[4] These things seem to me important to mention, not because of biographical convention but because they were important in Titchener's mind. The strong family tradition with its roots in English soil may be a partial explanation for the surprising fact that Titchener could spend the last thirty-five years of his life in America without once visiting England, and yet remain a staunch British subject without apparently a thought of becoming an American citizen. During the War his "we" meant Great Britain and his "you" America.

[5] An exhibitioner is appointed in part because of need of financial assistance. The benefaction was named for its founder, William Hulme (1691).

At Leipzig he found Külpe, as *Dozent,* Meumann, Kirschmann, Kämpfe, and Pace, Scripture and Frank Angell from America. This was the beginning of a lifelong friendship and mutual regard between Titchener and Angell, based upon similar tastes and Angell's admiration for Titchener's "impersonal frankness." The second year Pace, Scripture, and Angell left, and Witmer and Warren came. Titchener roomed with Meumann. Like the graduate students of any laboratory the world over, this group was much together in informal psychological discussion. Külpe was planning and writing his *Grundriss* which he published in 1893 and which Titchener translated into English in 1895. The thought was somewhat communal, and Titchener published in 1896 his *Outlines* on a similar plan. Some of the discussion, I have been told, hinged upon the fundamental methodological issue. Wundt had separated psychology and physics by reference to immediate and mediate experience, but younger men are always anxious to improve upon the master. Mach's *Analyse* had appeared in 1885; Avenarius had brought out the first volume of the *Kritik* in 1888 and the second in 1890. We all know that these books formed the basis of Külpe's and Titchener's definition of psychology. Külpe, as the more philosophical of the two, is said to have been especially concerned with Avenarius; Titchener, perhaps already turning from philosophy to science, preferred Mach, for whom his enthusiasm was never dampened.

The contact with Wundt was not personally intimate. Wundt was thirty-five years older than Titchener. He had not yet reached the height of his fame, but he was already the great man, expecting and receiving deference and homage, and capable of becoming "white and trembling with anger when some of his 'Zuhörer' . . . . slipped out of a lecture on philosophy shortly before its close." With the graduate students "he dealt . . . . somewhat autocratically, and prescribed the lines and methods of research . . . . minutely."[6] Probably Titchener's work on the *Erkennungsact*[7] and the doctoral dissertation, *Über binoculare Wirkungen monocularer Reize*,[8] were arbitrarily assigned in this way.

But if Titchener's intercourse with Wundt was not intimate personally, it was nevertheless both intimate and effective academically. The similarity between Titchener and Wundt may be partly merely a coincidence that they were similar personalities, though one was an Englishman and the other a German. Nevertheless this fact is not enough to explain all of the many overt items of resemblance. At least a dozen attitudes of Titchener's he got from Wundt.[9] There is his own autocracy in the Cornell Laboratory, not only in the matter of theses, less obvious perhaps than Wundt's, but, supported by his dominating personality, equally effective.[10] There is his persistent assumption that all the men of the Cornell Laboratory represent a single intellectual endeavor, opposed to an uninitiated outer world. There are both his tolerance in self-criticism and his inflexibility in polemics. There is his constant effort to demonstrate that psychology is a science. There is his consequent belief in formal psychological apparatus, and the need of 'building up' a laboratory by the collection of standardized pieces. There is his conviction that the elementary lectures should be accompanied by experimental demonstration, and also his habit (but here he excelled Wundt) of dramatizing his lectures. One device was his invariable custom of lecturing in an Oxford master's gown: "It confers the right to be dogmatic," he said. Another was the tradition under which the members of his staff have always attended the elementary lectures—and listened often, let it be said, with suppressed excitement to hear how, after some new discovery at Cornell, he would alter the familiar treatment of the subject. Could the picture be more like Leipzig and less American or English?[11]

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[6] For the picture of Wundt in 1890-92, see the sketches by Pace, Frank Angell, and Warren (from the last two I have quoted) and Titchener's letter written in 1890, *Psychol. Rev.*, 28, 1921, 159-169. Students of Titchener's character may well read these and the adjacent accounts of Wundt, since there can be no doubt that Leipzig formed the pattern for Cornell.

[7] *Philos. Stud.*, 8, 1892, 138-144.

[8] *Ibid.*, 231-310. The offprint with the short *Vita* is also extant.

[9] The concluding paragraphs of this biography discuss other more subtle ways in which the Wundtian impress upon Titchener appeared.

[10] American students could not of course accept Titchener entirely without conflict. He excited both fear and affection (or admiration) in his associates.

Actually then there were only two years of Wundt. In 1892, when twenty-five years old, Titchener received the degree of Doctor of Philosophy and returned to England. Here the record shows that he was an extension lecturer in biology at Oxford for a few months, a very natural arrangement in view of his relation to Burdon-Sanderson and of the fact that he had already since 1889 published ten articles in general biology, all of them in *Nature*. Titchener would have liked to stay on at Oxford, but Oxford has never, then or now, adequately recognized psychology, and experimental psychology as a science would have had there a particularly hard row to hoe.[12] At any rate on leaving Leipzig he had already agreed to go to America.

Frank Angell, his Leipzig friend of 1890-91, had gone back to America to Cornell to open a psychological laboratory initiated by a benefaction that phrenology is said to have inspired. After his first year at Cornell he was called, at its inauguration, to Stanford University, and he recommended Titchener for his Cornell post. "In those days a laboratory was a laboratory," Titchener has often said; and this young laboratory psychologist, still only twenty-five years old, hurried to Ithaca, New York, with its laboratory, as an assistant professor of psychology. He never left Cornell; and within psychology Cornell and Titchener have become almost interchangeable words.

Ithaca was certainly a remarkable change from Oxford and Leipzig. The scenic beauty of the Cornell campus, between two deep gorges, was then, except for mud, as it is now; but the university, architecturally at least, was raw. Nevertheless Titchener had his laboratory. The first years were strenuous ones. The six-room laboratory in White Hall had to be equipped with bought standard pieces and with home-made apparatus.[13] He married in 1894, Sophie K. Bedlow of Portland, Maine, and she helped him greatly in the Laboratory and later with drawings for the books. He may have been discouraged, for in 1893 he applied for the post at Toronto, and his effort to leave so soon after he had come was not looked upon with favor by the University.[14]

He plunged immediately into scientific work and writing. His list of publications contains sixty-two articles, long and short, from 1893 to 1900 inclusive.[15] He began to put the master into English. With Creighton he translated Wundt's *Human and Animal Psychology* in 1894, and revisions of the succeeding editions in 1896 and 1901. With Gulliver he translated the first volume of Wundt's *Ethics* in 1897, and the revisions in 1902. He had actually translated the entire third edition of the *Physiologische Psychologie* at Oxford, and, after revising it at Leipzig, took it to Wundt, only to find that its publication was prevented by the fact that the fourth edition was then ready to appear. In Ithaca he translated the fourth edition, and, holding it for last revisions, found it in turn displaced by the publication of the much enlarged fifth edition. Not discouraged he began again on the fifth and published immediately (1902) the first six of its twenty-two chapters, when his work on the *Manuals* took him away. He translated Külpe's *Grundriss* for publication in 1895, and with Pillsbury he translated Külpe's *Introduction to Philosophy* for publication in 1897. In addition to all this he wrote his own *Outline of Psychology* in 1896, his *Primer of Psychology* in 1898, and began the *Experimental Psychology*, of which the first two volumes appeared in 1901, and which we shall return to presently. It was a remarkable accomplishment, even as great men go.

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[11] Titchener was often mistaken by those who knew him slightly, or who knew only about him, for a German—once even by a younger English psychologist.

[12] It was only in 1882 that the physiological laboratory was founded at Oxford for Burdon-Sanderson against opposition of the most violent kind, first against the building, and three years later against its equipment.

[13] Titchener brought with him, all the way from Leipzig, Wundt's original demonstrational tachistoscope, a massive wooden affair over seven feet high and weighing about fifty pounds, with a heavy wooden shutter, whose drop on felt can be heard far down the hall.

[14] Angell had stayed only a year and here was Titchener trying to leave after a single year.

Meanwhile he was 'settling in.' He was made a full professor in 1895, when still only twenty-eight years old. Graduate students were beginning to work in the laboratory, which was moved to larger quarters in Morrill Hall in 1896.[16] Miss Washburn was his first Ph.D. in 1894. Pillsbury took his degree in 1896, Miss Gamble and Bentley theirs in 1898, Whipple his in 1900.[17] Titchener spent much time on his lectures and insisted, even against the President, upon not lecturing more than once a day. He took over the introductory lectures from President Schurman in 1895 and succeeded with them at once.[18] They remained extremely popular until he relinquished them for a time in 1909.[19]

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[15] A complete bibliography of Titchener's writings up to 1917 was published by W. S. Foster, in *Studies in Psychology: Titchener Commemorative Volume*, 1917, 323-337 (publ. in Worcester, but now available at the Cornell Psychological Laboratory).

[16] There had been six rooms for the Laboratory in White Hall; there were at first nine in Morrill, all on the fourth (top) floor. During the next decade the number was increased to eleven. In 1907 the third floor was added, giving 26 rooms in all, and the large psychological lecture-room and demonstrational laboratory were built in the new Goldwin Smith Hall. In 1909 two more rooms were added in Morrill, and the space has since remained unchanged. The elementary lectures were first given in the large lecture-room in the basement of the Library; then they were moved to the Laboratory in Morrill Hall; in 1907 they went to Goldwin Smith Hall. *Vide infra*, note 19.

[17] And others; see the list at the end of this article.

[18] As if he had not enough else to do, he had himself appointed from 1896-98 as "professor in charge of music," and thus really began the Department of Music, which was formally recognized after he had accomplished the preliminary work. Throughout his life he was devoted to music, and he was always proud of this little known activity.

[19] I took the course in 1905 in Morrill Hall with people spilling over into all the adjacent rooms. I can not exaggerate the magnetism of the lecturer. I used to retail the lectures to my room-mates, and the interest must have carried over for they always wanted to be told. I remember the lecture on tonal beats as especially exciting; imagine making beats exciting! I was then an engineering student, and yet it was the memory of these lectures that caused me to change to psychology five years later.

It was during this period that Titchener began to draw away from the philosophers. He was a member of the Sage School of Philosophy, but science, he thought, had nothing to do with philosophy—or at least with philosophers, for he always made the distinction. In all it was a struggle of fifteen years or more, but Titchener finally won his fight for a separation and a division of the spoils of scholarships and fellowships.

With all his working and fighting, the Titchener of the nineties found time for society. He was a member of a congenial group, and I am told he was the 'lion' of many social occasions. He was then, as always, an unusual mixture of decorum and of informal, intimate, sympathetic friendliness.[20] He always felt respect for men much older than himself; and he always expected, and even exacted, it from younger men, no matter what the degree of intimacy. Although he was outspoken in criticism of his friends, there was nevertheless underneath a persistent strain of loyalty, and his antagonism to those who had once transgressed his code was lasting.[21]

The total picture in America is not so easy to draw, nor is it so important. A foreigner settling in a new country is not always welcomed, particularly when necessities of life like laboratories are scarce. Especially may this be true of a dominating personality. Yet Titchener seemed always ready to respond to friendship. Angell had moved across the continent, but Sanford, he felt, welcomed him, and until Sanford's death there remained a warm cordiality between these two men. With Sanford he joined Hall on the staff of *The American Journal of Psychology* in 1895. In general, however, Titchener felt repulsed and isolated, and thus he let his colleagues alone, except in controversy. He was probably not constituted for team-work.

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[20] Two anecdotes of the nineties illustrate his rigid sense of decorum. Except for his being willing actually to carry the book-case, they might have happened at any time in his life.

In the early years, when Titchener lived with his family in a sort of dormitory building, he had an extra suite for a study and he let Pillsbury use the second room of it as his quarters. One day Pillsbury was about to carry a small book-case up to the Laboratory. Titchener objected and offered to carry it himself, on the ground that people would think Pillsbury was 'poor,' whereas his own status was beyond question. (Of course, Pillsbury carried the case.)

In those same years the President invited Titchener to dinner, but Titchener refused because the President had not called. The President argued (these matters of decorum have always led to argument with Titchener) that he did not have time to call on all the new men. Titchener then said that the President might at least send his coachman with his card. The President did and Titchener went to dinner!

[21] In these days he was beginning too the collection which later filled his house. He and Mrs. Titchener early agreed that the house should be a museum. In it was a place for anything that represented the intimate habits of other times or other peoples; a wool-wheel and a flax-wheel, a shepherd's crook, devil's masks and idols, pewter galore and old American hardware, hair wreaths, chest handles, the ancestral furniture and silver—I mention only what occurs to me. These articles stand for his anthropological interest in the broad; he was also especially interested in anthropology in the ordinary narrower use of the term. Moreover, the house was a vivarium as well as a museum; the interests of the days with Burdon-Sanderson showed themselves. There were cages of snakes, lizards, a legless lizard, chameleons, horned toads, Gila monsters, finches, parrots, and cockateels. The laboratory men helped to build the pond for the gold-fish. Then also for Mrs. Titchener he began and continued throughout his life a collection of precious and semi-precious stones, a collection necessarily limited, however, by a professor's income.

His relation to the American Psychological Association was typical. The twenty-six charter-members elected Titchener and Münsterberg at the meeting for organization at Clark University in the summer of 1892. Titchener was present at the first regular meeting the next winter, but he shortly resigned over a difference of opinion that had arisen about some action which he thought a matter of honor. He was host at the Ithaca meeting in 1897, but not a member. He joined again in 1910, but I do not think he ever went to a meeting after 1897. When the Association again met in Ithaca in 1925, he was still a member but did not attend, although he held court for a continuous stream of visitors at his house. The next year he allowed his membership to lapse.

Thus the lines between America, on the one hand, and Titchener, on the other, became sharply drawn. Clark University, because of Sanford and *The American Journal of Psychology*, remained within his world and was later definitely to affect it. The *Psychological Review* was still in the camp of the enemy; naturally Titchener stuck to *The American Journal of Psychology* as the mouthpiece for Cornell, following here a well established German tradition. Until 1917 he never published in any of the Psychological Review Publications except once briefly (1902) and thrice again briefly in controversy (1910, 1912). The record shows clearly that he avoided these journals and found less obvious places when *The American Journal of Psychology* seemed unavailable.

The controversy with Baldwin over reaction-times in the middle nineties was partly an ordinary scientific controversy but also a symptom of something beneath the surface. It is interesting to see now that it hinged on a fundamental national difference that was to become more obvious as American psychology developed. Titchener was saying: we must study the generalized human mind, even if it can be done only with elaborately trained observers; this was the German view. Baldwin was saying: we want to know about all sorts of minds, even if some of them are untrained in laboratory work. It is a similar opposition that developed later between the 'pure' scientific psychology of the generalized, normal, adult, human mind (Titchener) and functional psychology, the psychology of individual differences, mental tests, applied psychology, and lastly behaviorism.[22]

Thus it was in the late nineties that Titchener used James' phrase "structural psychology,"[23] in opposition to functional psychology. The latter had come to be applied to the Chicago product (John Dewey, A. W. Moore, and James R. Angell), although philosophically the American tradition, including Columbia and William James, is functional. In the ebb and flow of discussion and controversy the Cornell school thus came for a decade and even longer to be called 'structural.' Titchener himself, however, always disliked labels as pinning one down and impeding progress. He soon abandoned the word, and more recently 'introspectional' has been substituted for it without the least acquiescence from Titchener. The younger men, who in general like sharp dichotomies, might think of themselves as 'structuralists' or 'introspectionalists,' but not so Titchener. I never heard him refer to his school by any other word than "we."

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[22] These psychologies may seem to the reader unrelated. I think, however, that there is the same dominant motive behind them all, a motive that means that we can correctly speak of an American psychology (with Titchener left out).

[23] *Philos. Rev.*, 7, 1898, 449-465; 8, 1899, 290-299. But cf. K. M. Dallenbach, this JOURNAL, 26, 1915, 473-484, esp. 483.

We are now, however, passing into the first decade of the new century. Here Titchener's publication fell off for he was engaged in a great undertaking, the preparation of the four volumes of the *Experimental Psychology*, popularly known as 'Titchener's Laboratory Manuals.' Some one once said that this is the most erudite psychological work in the English language, and I am inclined to agree, not even excepting James (whose *Principles* Titchener called 'theory of knowledge' and not psychology!). What Titchener wanted to do was to establish psychology as a science by the exhibition of its scientific nature and by the introduction of laboratory 'drill courses' (as he always called them) into university instruction. Sanford's *Course* (1894-98) was not enough; the project needed to be prosecuted upon a larger scale. Titchener worked out all these experiments in the laboratory first. It was not sufficient for him to have the reports of research. He had to devise clean-cut procedures which would require but simple apparatus (for laboratory appropriations were small) and then to see whether students could get univocal results with them. If they did not, instructions could be changed, cautions could be added, procedure could be modified. A tremendous amount of careful, laborious work in the laboratory was necessary. The relation of the volumes to the psychological literature is more obvious. In this respect they are encyclopedic, and also astonishingly accurate. While there must be errors in typography and references, I have still to find one. When the *Quantitative Manuals* were almost completed, G. E. Müller's *Psychophysische Methodik* appeared in 1904, and Titchener had to revise all the way through. However, he never shirked responsibility of this sort and these *Manuals* were published the next year. The *Qualitative Manuals* had appeared in 1901.

It was about this time that Titchener's separatism showed itself in another fashion. Titchener was spiritually, as well as actually, outside of the American Psychological Association. It did not always follow in "the straight scientific path,"[24] which was of course experimental psychology in the narrower sense in which that phrase is still used. He wanted a group where there could be intimate discussion and constructive criticism of laboratory research then in progress. Later, perhaps even then, he dwelt upon the importance of the attendance at such meetings by graduate students, who thus would hear their seniors in friendly debate. So in 1904 he organized at Cornell a group which has ordinarily been called "the Experimentalists." Nothing could have been more informal in organization. Certain laboratories were to constitute the group, and Titchener always, even up to the time of his death, thought of a laboratory in Wundtian fashion as a man with his staff, his students, and his problems as the common property of all. The 'Experimentalists' have met every spring at different laboratories twenty-four times, omitting only the year of the War. They never had a president, a secretary, a record, a program, or, with a few exceptions, even a printed notice in the journals. There was only, every year, a "host," with Titchener of course as *deus ex machina*. The principle was ideal, especially in over-organized America. Many of us who have attended have felt that these meetings were the most stimulating of the year. As time went on, with Titchener's vigorous personality creating enemies as well as friends, the group became more limited. The bonds were between men and not laboratories; and only those who knew Titchener can realize how completely these meetings centered about him, and why they thus belong, not in a history of their own, but in his biography.[25]

With the *Manuals* out of the way, Titchener's interest in systematization came to the fore. It had never lapsed since the Leipzig days, but in so far as it found its way into print it was more often for the sake of removing an objection than from a pure interest in construction. Mach and Avenarius were necessary to prove psychology a science. 'Structural psychology' was intended as an antidote to 'functional psychology.' The doctrine of the sense-distance in the *Quantitative Manuals* was Delboeuf's and was meant by Titchener to overcome the 'quantity objection' to mental measurement. Now, however, Titchener, almost forty, was ready to cope with the problems of attention, feeling and thought. There were three years (1905-07) of preparation with little productivity. Then in February, 1908, Columbia University asked him to deliver a series of lectures, and he gave the *Lectures on the Elementary Psychology of Feeling and Attention,* and published them in the same year. It was feeling and attention that he held to be "elementary;" the lectures are not. Attention was then (as now, alas!) a difficult concept. It seemed to imply activity, and 'scientific introspection' shows only content in the mind. How was science to deal with it? Titchener sought to settle the problem of attention by declaring its status as an attribute of sensation. Feeling too was an uncomfortable item. Wundt's theory was not believed, and feeling was always becoming either sensation or, like attention, activity. Titchener sought to give it, as far as possible, independent elementary status, thus preserving for an elementaristic system an additional term with which to work.

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[24] As James said, referring to Titchener.

[25] That the hitherto unprinted record may be complete I give the list of meetings from the first as Titchener gave it at Cornell in 1923.

1904 Cornell
1905 Clark
1906 Yale
1907 Pennsylvania
1908 Harvard
1909 Princeton
1910 Hopkins
1911 Cornell
1912 Clark
1913 Wesleyan
1914 Columbia
1915 Yale
1916 Princeton
1917 Harvard
1918 [Omitted]
1919 Cornell
1920 Cornell
1921 Clark
1922 Wesleyan
1923 Cornell
1924 Clark
1925 Princeton
1926 Pennsylvania
1927 Harvard

Ten laboratories were invited to the first meeting.

The next year he was invited for a lecture series at the University of Illinois. Külpe had slipped from the path that led straight from Wundt, and Külpe's school of 'imageless thought' at Würzburg, while it brought thought into the laboratory as Wundt would not attempt to do, introduced into psychology a fourth element (sensation, image, feeling, thought) which Titchener could not, in the experiments of his graduate students, find. At Illinois Titchener gave the *Lectures on the Experimental Psychology of the Thought-Processes,* which were published the same year (1909). Here he analyzed and combated Külpe's school, seeking to reduce thought to the sensory and imaginal conscious attitudes. Perhaps the most important thing in this book is the context theory of meaning, which psychologizes the supposedly imageless thoughts in a way that explains why they seem to be imageless.

All this while the graduate students in the laboratory were very active, mostly upon problems related to Titchener's then primary interest. Titchener himself was also preparing the *Text-book of Psychology,* intended for instruction in elementary courses, but really a systematic work in brief compass. The first half of it appeared in 1909 and the whole in 1910. It is the only thorough account of Titchener's psychology that we have in single covers. It was printed seventeen years before his death and to the psychological world at large still represents Titchener, for there has come from him nothing else comparable.[26]

Titchener kept on thinking, reading, and working. The system of 1910 was old in 1915, and it has, perhaps several times, been again outgrown. The language of his laboratory changed, and a graduate three years absent had on returning to learn psychology again—not entirely, but some fundamental thing that shot through the entire structure and bobbed up at every point of a discussion. Naturally the Cornell group felt that it was far in advance of psychology in general and of the Titchener who was known outside. There is, however, a striking change in the objective record. The change is not so much in the number of titles in Titchener's bibliography as that there are no books except the *Beginner's Psychology* in 1915,[27] begun as a revision of the old *Primer*, but characteristically written entirely anew. On the surface then there is a difference between the first and the last halves of the thirty-five years in America. Can it be explained?

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[26] The *Text-book* is still used in classes, just as James' *Principles* stood for James the psychologist and was used in classes at the time of his death, eighteen years after its publication.

First, we must note that in 1909 Clark University, then a virgin university for research and graduate instruction only, called Titchener on Sanford's elevation to the presidency of Clark College. Cornell responded by appointing Titchener to a professorship in the Graduate School, thus relieving him of all undergraduate instruction. The laboratory was divided. In 1912, however, when Bentley went to Illinois, Titchener resumed command of both halves of the laboratory (so closely integrated that it had never been really divided), and he also returned to the elementary lectures by way of a special additional appointment.

The result of these changes was the creation of a life of academic retirement. Let me picture Titchener from 1913 to 1917 when I was his "underling."

In the first semester on Tuesday and Thursday at eleven he lectured to the undergraduates in the new lecture-room in Goldwin Smith Hall, the room with a psychological demonstrational laboratory and an office built off it, and with the pitch of the seats determined by Titchener's stature. The demonstration was set out the hour before, and Titchener arrived shortly after ten to inspect it. Later the staff gradually gathered in his office. When the time of the lecture arrived, he donned his gown, the assistant brushed his coat for fear of ashes from the ever-present cigar, the staff went out the door for apparatus and took front seats, and Titchener then appeared on the platform from the office-door. The whole rite was performed pleasantly and sometimes jokingly; yet it was scrupulously observed. After the lecture the staff gathered in Titchener's office for an hour for talk and at one o'clock dispersed for lunch. The Laboratory was across the campus in Morrill Hall and Titchener never went near it during the first semester. That he was able from his house nevertheless to determine some of the smallest details of its operation is a tribute to a personality that could impress its will, generally without resentment, upon others.

In the second semester the lectures ceased and the seminary began on Monday evenings in the Laboratory. Invitation to attend was an honor.

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[27] As a matter of record I remark that the original MS., with its corrections, is in my possession. Titchener composed rapidly on the typewriter and revised with pen. In drastic cases of course he rewrote and in all revision he was indefatigable.

The staff were always there as a a matter of course. The seminary brought Titchener to the Laboratory and there was the chance for the lucky graduate to persuade Titchener to look at his apparatus, but he did not always succeed. The seminary ran on, with prandial entertainment provided in rotation by its members, until late—often until after midnight when gas light had to be substituted for electricity.

In June Titchener came to the Laboratory for doctors' examinations, but I do not think that in seven years (perhaps seventeen) he was once in the Laboratory between June and February. Graduates saw him at the house and not often. On my dissertation I conferred with Titchener twice, once when the problem was planned, once eighteen months later when he had gone over the finished manuscript. He had a special assistant and no one else ever saw him in the mornings (except at the lectures in the first semester). Telephoning, for others than the assistant, was taboo except in great emergencies.

In those days Titchener played tennis daily in the summer. During the rest of the year he would on occasion go down town to shop, but otherwise lived almost entirely in the house. He had, I think, absolutely no social engagements except with the laboratory group and with the little orchestra of his family and students that he conducted at home on Sunday evenings. His old-time friends had been much older than himself and were dead or removed from Ithaca. Professors who had long been at Cornell had never seen Titchener. They could not even get into his lectures, for the room was full of sophomores and no one else was admitted.

In the last decade there were changes in this routine, but they do not greatly alter the picture. In 1918 Titchener began to lecture in the summer school for financial reasons. His tennis dropped off at the same time. Some (but not all) graduate students with doctoral research in hand, went to the house weekly for the most careful oversight and instruction.

In 1911 Titchener gave the Lowell lectures (never published), but thereafter he travelled only with great reluctance. He went to the 'Experimentalists' in the spring. At the time of the War he broke his rule to lecture, so that he might contribute to the Prince of Wales Fund for war widows and orphans. He lectured at Wesleyan in 1922 and at the opening of the new Princeton Laboratory in 1925, both in connection with attendance upon the meetings of the Experimentalists. He had planned this present fall to participate in the opening of the new laboratory at Wittenberg College, for he still felt that the founding of a psychological laboratory was the most important physical thing that could happen in psychology.

Now this life of retirement ought to have led to increased productivity. It did not on the record. Is the record at fault?

Perhaps it is in the beginning of the period. From about 1912 to 1917 Titchener was energetically engaged in writing a systematic psychology. His friends supposed then that it was to be a work of several volumes like Wundt's *Grundzüge*. In 1917 the first volume was done all but the last chapter. Had the first volume of a great work appeared in 1918, I suppose we should say that the preceding period was merely one of incubation; but it did not appear. In 1921-22, having become sole editor of *The American Journal of Psychology*, he printed therein two of the chapters.[28] About this time he seems to have decided not to attempt to continue the work beyond this volume of systematic prolegomena, and also to rewrite the volume in part without again printing these chapters. The book was advertised by the publishers. Titchener's friends urged him to complete it, but there was always some excuse. When an anonymous benefactor offered, in behalf of the book, to relieve him of teaching in the summer of 1927, he hesitated, agreed, but deferred acceptance until 1928.[29] He died without publishing.[30]

I can not presume to say anything of the growth of Titchener's psychology since 1917, but must leave that to some intimate colleague of these years. Already in 1917 there had been a shift in the doctrine of sensation and the attribute.[31] The doctrine of dimensions came later, as did also the stress on phenomenological observation as distinguished from introspection. There have been hints of the 'newest' psychology in theses, but none that are clear to the uninitiated. Only his manuscript or some laboratory intimate of the last few years, who has lived in the community built around the 'system,' can tell us.

Our question, however, is still unanswered. Was there a change about 1910? In objective productivity: yes. In the relation of this magnetic, dominating personality to others: no. In the vigor or quality of psychological thought: we may never know.

My own theory is just a guess and I give it only as such. It is probable that no one works without extraneous incentives—ambitions or irritants. Titchener's graduate appointment in 1910 had removed many irritants. Ambitions were being removed by circumstances. It was becoming plain that Titchener would find no one who would carry on his torch as he had been carrying Wundt's, and he always wanted an able successor-disciple. He thought of Münsterburg's post at Harvard as the greatest position in America; but, when it was offered him in 1917, he found that his picture of a laboratory could not be created at Harvard and he stayed at Cornell with no other place where he could possibly want to go. Except, of course, Oxford; but Oxford seemed no more ready to recognize psychology then than it was a quarter of a century earlier. There was a *Festschrift* for him after twenty-five years of service at Cornell,[32] and it is true that Titchener felt this an honor. He was, however, an alien and ineligible to the National Academy of Sciences. He was a psychologist in the "colonies" and never got his F. R. S. In 1926 he was elected a foreign member of the

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[28] Brentano and Wundt: Empirical and experimental psychology, this JOURNAL, 32, 1921, 108-120; Functional psychology and the psychology of act, *ibid.*, 32, 1921, 519-542; 33, 1922, 43-83. Titchener's thoroughness is indicated by the fact that he said that he spent an entire year on Husserl only to find out that "there is nothing in him." The result of this year is four printed pages; *ibid.*, 33, 1922, 55-58.

[29] The growth of the unsuspected cerebral tumour may explain the 'resistance' of the last few years, but the indication is that it was not of earlier origin.

[30] Many of us hope that the publication of what is finished can follow now.

[31] Sensation and system, this JOURNAL, 36, 1925, 258-267. Titchener denied that this article meant a change in his doctrine of attributes, and said that he had held the same view all along. The article certainly was a change from what the men of his Laboratory at that time thought had been his doctrine.

[32] *Studies in Psychology, op. cit.*

Polish Academy of Science and Letters, but by this time he had gotten beyond response to such events.[33] He had, by shutting himself off more and more, outgrown even the possibility of external stimulation. I can not assert that an external drive is necessary in a man like Titchener, but I think it is. The fact that the psychological urge in him, as compared with his first two decades in America, had continuously weakened in the last fifteen years is unescapable, compelling as it has always been. He let himself be drawn into other things, like numismatics. He was too young for this change to have resulted from age; one has to look for some other cause.

Titchener's interest in numismatics is significant as showing both his versatiltiy and his thoroughness. He had always, so he once wrote, "been interested, even from fairly early childhood, in Greek and Roman numismatics." In recent years he undertook to learn what he could of Mohammedan coins. He began by "buying up fag ends of lots offered for sale in New York and London," and amused himself by working out the inscriptions. He was very fond of puzzles and here were puzzles where an historical and linguistic knowledge could be acquired and brought to bear. He was borrowing books in 1922 from the American Numismatic Society, and thus came into contact with Mr. Howland Wood, its curator. A close 'numismatic friendship' developed, with a few visits between the two men and a voluminous correspondence.[34] Mohammedan coins were at the focus of this interest, and Titchener learned some Arabic in order to read them.[35] Later his attention was extended to include Chinese coins, but he never lost his original love for the Greek and Roman. Wood would send him coin-puzzles to decipher, or together they would work them out. In all of this work Titchener was the student, but an ideal student who admitted help only in the last emergency. Mr. Wood writes: "He has probably read nearly every book in our library on oriental coins, so thorough was his enthusiasm in pursuing to a finish whatever he undertook to do. . . . I have been increasingly surprised at his mastery in reading the inscriptions on coins after he really got started. An unread coin was a delightful problem to be mastered. . . . His knowledge, versatilty and memory filled me with wonder. His reasoning and deductive powers, given time, would have put him in the first ranks as a numismatist, especially as he was not afraid to ask questions and gratefully accept answers." All this in a field outside of psychology! It is true that it may in part account for the fact that the systematic psychology was never finished, but it shows clearly that there had been no diminution either in Titchener's intellectual vigor or in his capacity for friendships.[36]

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[33] One trouble was that he achieved distinction too soon. His honorary degrees were: D.Sc., Oxford, 1906; LL.D., Wisconsin, 1904; D.Litt., Clark, 1909; D.Sc., Harvard, 1909. After 1910 there would be nothing left to do with degrees except increase the quantity.

[34] Mr. Wood has very kindly shown me some of this correspondence.

[35] I have said nothing of Titchener's command of languages. He retained his training in Greek and Latin from the Oxford days, when he studied classics; he used to make up Latin verses for fun. He was most fluent in German, which he read incessantly and could speak well. He also read French constantly, and spoke it well in his younger days. He could read Italian, but used an Italian-German dictionary when he wished to be accurate. He was naturally a linguist. Burdon-Sanderson at Oxford once gave him an article in Dutch for report a week later. When Titchener said he did not know Dutch, Burdon-Sanderson said, "Learn it." He 'learned' it enough to report in a week. He knew some Sanskrit on account of his interest in philology. The numismatics took him into Arabic and even a little Chinese. One ought to add English to the list, for he was much more than ordinarily fluent in the mother-tongue.

[36] I have not touched as fully as I should like upon the personal side of the older Titchener. He had enemies in youth, derogators in maturity, and he was never old; but, in spite of his separatism, he had always a small group of psychological friends with whom he kept up a voluminous correspondence. Besides these men there was also a much larger group of young men who depended upon him professionally in the criticism of papers, for academic advice, and even in personal affairs. In many there was the mixture of fear and admiration that I have already mentioned. His total correspondence was very large, and he never before 1921 had stenographic assistance.

The best key to Titchener's life, I think, lies in the fact that he emulated Wundt—how consciously I do not know. We have already seen the superficial evidence of the Leipzig impress, but I think the influence goes deeper. Often when Titchener wrote about Wundt, he seemed to be writing about himself. Often when he defended Wundt in print, he used phrases which he has used in defense of himself.[37]

Titchener urged that there were three interests and corresponding periods in Wundt's life, concerned successively with the establishment of experimental psychology, with the creation of a scientific metaphysics, and with the development of *Völkerpsychologie;* and he was most emphatic in his insistence that those interests constitute, not three programs of work, but a single, closely integrated program.[38] The parallel in Titchener holds for the first two periods. He began with the further establishment of experimental psychology as a science (1892-1905); he went on to systematization, that made many say he was no longer an 'experimentalist' (1905-1927); he had always a keen interest in anthropology,[39] and a few of his friends had wondered if he would not turn to anthropological writing when he retired from his active professorship. Yet these interests were not successive, but continuous; he had never been without them all; the dates mean only the dominance of one in personal publication. Titchener thought of them as an integrated whole and defended the unity of the plan in Wundt. He was at pains to show that Wundt foresaw the entire plan of his life in 1862,[40] and just so might Titchener's tripartite psychology be traced from 1892.

In this article on Wundt, Titchener protested against the belief that "social psychology was Wundt's first and fondest love, and that all his life, up to about 1890, was spent in clearing intruders out of the way, that he might ultimately return to it." "I should not accept this legend," he wrote, "if it came with Wundt's own subscription; I should mistrust an old man's memory."[41] Strong words! Could Titchener have been anticipating the criticism that he was not at heart an experimentalist should he presently turn, in his writings, to anthropology? Or could he have been thinking of the criticism that his interest in system called in question the primary nature of his experimentalism?

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[37] Cf. therefore, Titchener's account of Wundt, immediately after Wundt's death, this JOURNAL, 32, 1921, 161-175, to which reference is made below. The suggestion that a thread of autobiography runs through this article of Titchener's must, of course, be taken with extreme caution.

[38] *Op. cit.*, 163-169.

[39] His home contained many curios.

[40] *Loc. cit.*, all, but esp. 163.

[41] *Op. cit.*, 169.

Then there is the discussion of Wundt's fluidity. James once said of Wundt: "Cut him up like a worm, and each fragment crawls; there is no *noeud vital* in his medulla oblongata, so that you can't kill him all at once."[42] Titchener dissented vigorously from this sort of criticism, which is not unlike complaints that were sometimes made of him. Wundt was, he said, "as honest as the day," but his work was always "at once systematic and provisional" and "he held his theories far more loosely than his readers ordinarily suppose;" "his greatest reverence was for fact."[43] Titchener based his assurance in this matter on Wundt's assertions to him in conversation; what intimate of Titchener's is there who has not heard him use almost identical phrases about himself?[44]

There is also what Titchener said of the necessity that Wundt should not be taken externally and superficially and "that students of Wundt must read his books in series, and can never hope to understand him fully from any single presentation of his thought."[45] How often Titchener used this same argument in reply to criticism!

Most of the other similarities between Titchener and Wundt we have already noted as lying in the more objective record. There remains only his editorial attitude. Titchener wanted a journal of his own, like Wundt's *Philosophische Studien.*[46] He never had it, although he practically secured it in 1921 when he became sole editor of *The American Journal of Psychology.* When he found in 1925 that it was not easily possible in America to do what used to be the rule in Germany, he resigned. However, he discovered that the continued *American Journal* and the *Psychological Review Publications* were to him what the *Archiv* and the *Zeitschrift* were to Wundt in 1902. He thought of a Cornell series, analogous, I suppose, to Wundt's *Psychologische Studien,* started under similar circumstances. Actually he finally arranged to participate in the founding of a new journal —a project still incompletely realized at the time of his death.

The evaluation of Titchener's psychology can be left to posterity, and such was his wish, often expressed. He said of

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[42] *Letters of William James,* 1920, I, 263.

[43] Titchener, *op. cit.,* 169-171. Titchener's note that "there is, however, no smoke without fire" (p. 170) gives the reason why Wundt did not acknowledge change publicly, and would seem similarly applicable to Titchener's own thought.

[44] Titchener has always criticized me for writing with "hardened concepts" and for showing, in controversy, my whole hand. He cited G. E. Müller as the ideal, but of course he sought to follow his own ideal. Especially when taken in relation to what he says of Wundt, I think this fact throws light on Titchener's attitude in controversy.

It was to Müller, rather than Wundt, that Titchener wrote. He once said that he always had Müller in mind as his audience, and he may have been trying to hoist Müller with his own petard as well as to follow Wundt.

[45] *Op. cit.,* 171 f.

[46] Although not in make-up, for he had very definite views about notes, reviews, and the other constituents of a general journal.

Wundt's plan of life: "As to the ultimate significance of that task, it would be the part of wisdom to keep silence; we stand too near to Wundt to see him in a just perspective."[47] Certainly Titchener was a remarkable personality and a great psychologist. That his influence has been so effective in spite of his personal isolation from psychologists and his intellectual isolation from the national trend is, I take it, a test of greatness. A century hence it will be possible to say just where his psychology belongs in the history of the science.

EDWIN G. BORING

CORNELL DOCTORATES IN PSYCHOLOGY

The psychologists who took the doctorate with Titchener are listed below.[48] As the Graduate School was not organized at Cornell until 1910, and as the records before that date are incomplete, the names of some may have been inadvertently omitted.

1894. Margaret Floy Washburn
1896. Alice Julia Hamlin
Walter Bowers Pillsbury
1898. Isaac Madison Bentley
Eleanor Acheson McCulloch Gamble
Stella Emily Sharp
1900. William Chandler Bagley
Guy Montrose Whipple
1901. Carrie Ransom Squire
1902. John Wallace Baird
Margaret Everitt Schallenberger
1905. Herman Campbell Stevens
1906. Samuel Perkins Hayes
1907. Elsie Murray
1908. Joseph Herschel Coffin
1909. Ludwig Reinhold Geissler
Taizo Nakashima
William Henry Pyle
1910. Helen Maud Clarke
Clarence Errol Ferree
Tamekichi Okabe
1912. Lucy May Day
Austin Southwick Edwards
Alma deVries Schaub
1913. Karl M. Dallenbach
William Silliman Foster
Christian Alban Ruckmich
1914. Edwin Garrigues Boring
Mabel Ensworth Goudge
1915. Josephine Nash Curtis
1916. Salem Shihadeh George
1917. Gilbert Joseph Rich
1918. Cora Louisa Friedline
Josephine Mixer Gleason
Paul Thomas Young
1919. Hubert Sheppard
1920. Homer Guy Bishop
Claire Comstock
Forrest Lee Dimmick
Louis Benjamin Hoisington
Robert Thomas Holland
Cheves West Perky
Alice Helen Sullivan
Michael Jacob Zigler
1921. Anna Kellman Whitchurch
1922. Mabel Florence Martin
1923. Grace Kinckle Adams
Wilbert Andrew Garrison
Albert Clayton Reid
1924. John Paul Nafe
Elizabeth Fallin Möller
Magnus George Scheck
1925. Samuel Feldman
1926. Frederick Lovell Bixby
1927. Joy Paul Guilford
Junichiro Horiguchi

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[47] *Op. cit.*, 176.

[48] I am indebted to Professor Dallenbach for this list.

A QUANTITATIVE INVESTIGATION OF THE PURKINJE AFTER-IMAGE[1]

By DEANE B. JUDD, Cornell University

INTRODUCTION

In 1823 Purkinje described a fleeting after-image following a brief stimulation of the visual organ by a light pulse.[2] Since that time many investigators have studied the after-images following momentary retinal illuminations and have referred with considerable inconsistency to one or another of the after-image phases as the Purkinje phase or after-image.[3] Inasmuch as many major contradictions are common in the results reported by different workers, this inconsistency is not surprising. It was not until the publication of a series of papers by Fröhlich (1921),[4] which followed the pioneering work of Dittler and Eisenmeier (1909),[5] that the after-image reported by Purkinje was specifically identified.[6]

Although many investigations have been made on the visual after-images since Purkinje's time, the work of Dittler and Eisenmeier is referred to as pioneering because it was the first to identify and to describe the complete series of after-images that are produced by a momentary retinal illumination. Their descriptions were verified by Fröhlich in such an exhaustive series of experiments that little doubt now exists regarding the accuracy of their findings for the normal observer. The series of phases in the image, according to Dittler and Eisenmeier, is as follows:

(*1*) The primary image (not an after-image).

(*2*) A very short, dark interval (negative phase).

(*3*) A positive after-image (bright),[7] of the same hue as the primary image (Phase *1*) though usually less bright (Hering after-image).[8]

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[1] From the Physical Laboratory of Cornell University.
This is a condensed report of a thesis offered to the Graduate Faculty of Cornell University in partial fulfillment of the requirements for the degree of doctor of philosophy. The complete thesis may be had on application to the Cornell University Library.

[2] J. Purkinje, *Beobachtungen u. Versuche zur Physiologie der Sinne*; *Beiträge zur Kenntnis des Sehens in Subjective Hinsicht*, I, 1823, 97.

[3] It must be understood at the outset that the relative increase in sensitivity to light of short wave-length which the eye exhibits in twilight vision, and which is sometimes referred to as the Purkinje effect, is connected to the subject of the present investigation only by the fact that the same man first described it.

[4] F. W. Fröhlich, Untersuchungen über periodische Nachbilder, *Zsch. f. Sinnesphysiol.*, 52, 1921, 60-88; Über den Einfluss der Hell und Dunkeladaptation auf den Verlauf der periodischen Nachbilder, *ibid.*, 53, 1922, 79-107; Über die Abhängigkeit der periodischen Nachbilder von der Dauer der Belichtung, *ibid.*, 108-121.

[5] R. Dittler and J. Eisenmeier, Ueber das erste positive Nachbild nach kurzdauernder Reizung des Sehorgans mittelst bewegter Lichtquelle, *Arch. f. d. ges. Physiol.*, 126, 1909, 610.

[6] Carl von Hess, in a long series of publications, has reported results which are so contradictory to the findings of other investigators that they suggest that he has made an error in phase identification. Cf. Fröhlich, *op. cit.*, 53, 1922, 80.

[7] The after-image following a light pulse is called positive if it appears brighter than the surrounding field, and negative if less bright. The use of the terms 'positive' and 'negative' to refer to brightness only, and not to the hue of the image—as is more usual in psychology—is necessitated by the existence of an after-image that is positive in brightness and complementary in hue to the primary image, *e.g.* the Purkinje phase. This terminology was used by H. G. Hamaker in 1899 (Ueber Nachbilder nach momentaner Helligkeit, *Zsch. f. Psychol.*, 21, 1899, 1-44), and has since been accepted by most workers in this field.

[8] This first positive after-image is called the Hering after-image by Fröhlich because Ewald Hering described the method which led Dittler and Eisenmeier to their definite discovery and identification of this phase. Cf. Ewald Hering, Eine Methode zur Beobachtung und Zeitbestimmung des erstes positiven Nachbildes kleiner bewegte Objecte, *Arch. f. d. ges. Physiol.*, 126, 1909, 604.

(*4*) A short, dark interval, darker than the surroundings, a negative phase. It is, however, longer than the second phase (Phase *2*).

(*5*) A second positive after-image, which appears most plainly in the hue complementary to that of the primary image (Purkinje after-image).[9] It is definitely less bright than Phase (*3*).[10]

(*6*) A long, dark interval, a negative image.

(*7*) A third positive after-image of long duration; it is of the same hue as the primary image, but unsaturated. It is less bright than Phase (*5*).

(*8*) The final phase, a long, lingering dark image, negative. This final negative phase does not appear immediately after the disappearance of (*7*) but only after a (usually long) blank interval.[11]

Thus there exist 8 separate phases in the image following a momentary retinal illumination, 7 of which may be called after-image phases. It must not be assumed, however, that these 8 phases can always be observed in every experiment, for such is not the case. The conditions which make it easy to observe the first part of this after-image series, make it difficult to observe the last, and contrariwise.

The experimental conditions upon which the characteristics (such as development time, duration, intensity, and quality) of the after-image series depend, are: (*a*) duration of light stimulus; (*b*) intensity of light stimulus; (*c*) spectral distribution of energy in light stimulus; (*d*) characteristics of the surrounding field; (*e*) characteristics of the field on which the after-image series is projected; (*f*) condition of adaptation of the *O*'s eyes; (*g*) retinal region stimulated; and (*h*) the competency of the *O*s. The complexity of the problem thus becomes evident. The effect of these 8 variable conditions must be determined before the problem is solved.

A great advance was made by Fröhlich when he specified these conditions and worked out the effects of varying (*a*), (*b*), (*f*), and (*g*). He also varied (*c*) — the spectral distribution of energy in the light stimulus — but because these variations were not quantified his results upon this point have only a qualitative significance.

PROBLEM

Because of the extreme complexity of the after-image series and of the conditions which affect it, we limited ourselves in the present study to the description of the Purkinje phase (Phase *5*) and to the investigation of the conditions of its arousal.

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[9] Although the hue of this phase is complementary to the hue of the primary image (when that image possesses a hue), it is still called a positive after-image phase because it is brighter than the surrounding field. (Cf. footnote 7.) Thus, a primary image consisting of a white circle with a black line across it would be followed by a phase (*5*) consisting of a light circle with a black line across it. If, however, the circular field of the primary image were yellowish white with a black line across it, the phase (*5*) would appear bluish white with a black line across it.

[10] Designated as the Purkinje phase because most workers in referring to any flash image as the Purkinje phase have reported characteristics similar to the above. It has also variously been referred to as 'recurrent vision,' the 'recurrent image,' and 'Bidwell's ghost.'

[11] The interval is referred to as 'blank' because in it the after-image is indistinguishable from the dark surrounding field. It must not be confused with the short negative phases (*2*, *4*, and *6*), which although they do not possess hue, are definitely darker than the dark surrounding field.

Of all the phases of the after-image sequence, the Purkinje is the most interesting, not only because less is known about it, but also because of its apparent paradoxical nature—being positive in brightness and negative in hue. Our work, therefore, bears upon the Purkinje phase. The other 7 phases are mentioned incidentally only for purposes of orientation and elaboration.

APPARATUS AND PROCEDURE

Before pointing out the means by which the present work might be expected to fill some of the gaps in Fröhlich's results, it may be well to note that, since Purkinje's description more than a century ago, the attack on this problem has always been by one of two general methods. (1) Either a stationary light pulse has been used, after which the images were described as they appeared successively in time; or (2) a moving light spot or slit has been used—the observing eye being held stationary—after which, at a given time, the images appear simultaneously spread out in space. The first method, used by Brücke,[12] Helmholtz,[13] Aubert,[14] Exner,[15] Young,[16] Davis,[17] Charpentier,[18] Hess,[19] Snellen,[20] Bosscha,[21] and Hamaker,[22] is best adapted to the study of the last part of the after-image series. The second method, used by Purkinje,[23] Bidwell,[24] Von Kries,[25] Hess,[26] Hamaker,[27]

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[12] E. Brücke, Versuche über subjective Farben, *Poggend. Ann. d. Physik u. Chem.*, 84, 1851, 418.

[13] H. von Helmholtz, *Physiologischen Optik*, I, 358 ff.; also *Handbuch der Physiologischen Optik*, II, 1911, 204.

[14] H. Aubert, *Handbuch der gesammte Augenheilkundes*, edited by Graefe and Saemisch.

[15] S. Exner, Ueber den Erregungsvorgang im Sehnervenapparate, *Sitz. d. K. Akad. Wiss.*, 65, 1892, 59.

[16] C. A. Young, Note on recurrent vision, *Phil. Mag.*, 43, 1872, 343.

[17] A. S. Davis, On recurrent vision, *Phil. Mag.*, 44, 1872, 526.

[18] A. Charpentier, *Oscillations Retiniennes*, *C. R.*, 113, 1891, 147.

[19] Carl von Hess, Bemerkungen zu dem Aufsatz von Bosscha: Primäre, sekundäre und tertiäre Nachbilder nach momentanen Lichteindrücken, *Arch. f. Ophth.*, 40, 1894, 259; 44, 1897, 445. Also *Arch. f. d. ges. Physiol.*, 49, 1891, 190; 95, 1903, 1.

[20] H. Snellen, *Notes on Vision and Retinal Perception*, 1896.

[21] H. P. Bosscha, Primäre, sekundäre und tertiäre Netzhautbilder nach momentanen Lichteindrücken, *Arch. f. Ophth.*, 40, 1899, 22.

[22] H. G. Hamaker, Ueber Nachbilder nach momentaner Helligkeit, *Zsch. f. Psychol.*, 21, 1899, 1-44.

[23] J. Purkinje, *op. cit.*

[24] S. Bidwell, On the recurrent images following visual impressions, *Proc. Roy. Soc.*, 56, 132, 1894.

[25] J. von Kries, Ueber die Funktion der Netzhautstäbchen, *Zsch. f. Psychol.*, 9, 1896, 81; Ueber die Wirkung kurzdauernder Lichtreize auf das Sehorgan, *ibid.*, 12, 1896, 81; Ueber die im Netzhautzentrum fehlende Nachbilderscheinung und über die diesen Gegenstand betreffenden Arbeiten von Hess, *Arch. f. Ophth.*, 42, 1896, 90.

[26] Carl von Hess, Untersuchungen über den Erregungsvorgang im Sehorgan bei kurz und langdauernder Reizung, *Arch. f. d. ges. Physiol.*, 101, 1904, 226; Untersuchung zur Lehre von der Wechselwirkung der Sehfeldstellen, *ibid.*, 179, 1920, 50.

[27] H. G. Hamaker, *op. cit.*

Samojloff,[28] McDougall,[29] Dittler and Eisenmeier,[30] Hering,[31] and Fröhlich,[32] is best adapted to the study of the first part of the series, *i.e.* of the rapidly appearing and disappearing phases (Phases *1-5*).

Apparatus. The apparatus used in the present experiment was constructed according to the principles of the first method so as to give control of a stationary light pulse. As this apparatus is described in detail in the January number of this JOURNAL,[33] a brief statement of its several advantages will here suffice. The experimental set-up enabled us: (1) To obtain readily, in recognized units of retinal illumination (photons),[34] a measure of the brightness of the stimulus. Fröhlich is the only experimenter who measured the brightness of his light. He, however, used arbitrary proportional units which have slight significance outside his own work. (2) To vary continuously and through a wide range: (a) the duration of the light pulse (0.005-2.0 sec); (b) the brightness of the light pulse (0.04-20,000 photons); and (c) the brightness of the surrounding field (0.004-20.0 photons). (a) and (b) have been varied experimentally by Fröhlich, but only with the moving-slit method. The brightness of the surrounding field, (c), has almost invariably been held constant at a very low value (darkness). (3) To control and vary the spatial pattern of the stationary stimulus, and thus to study the detail given in every one of the different after-image phases. A study of this kind is practically impossible with the moving-slit method. (4) To use a large number of *O*s.

Observers. Most of the experimenters have emphasized the necessity of long practice for observation in this field of research, and they have consequently acted as their own *O*s. Now and again a second *O* has been called in for a casual qualitative report, but the instances are few in number. Observation of the after-image series was so easy with our apparatus that it was found possible to use 8 *O*s, and to obtain quantitative results from all of them. Of the 8 *O*s, 1 is a psychologist, 1 a biologist, 3 are mathematicians, and 3 physicists.

Procedure. Two methods of procedure were used in the present study. In the first, Method A, the *O*s were required to describe all the images that they experienced. The stimulus was presented at the option of the *O*s until they were satisfied that they could give an accurate and detailed description of their images. Although the reports of all the after-image phases were recorded, the variation of the stimulus-conditions was governed by the reports of the Purkinje phase.

In the second method, Method B, the brightness of the stimulus was determined which was required just to evoke a sharply defined Purkinje image. The *O*s were required to report 'yes' or 'no' accordingly as they did or did not see an after-image of a line—whose width in the stimulus-field subtended a visual angle of 5′—in the Purkinje phase. The intensity of the light was varied and the liminal value necessary to produce the after-image of the line in the Purkinje phase was determined for every *O*, and this value was taken as the measure of sensitivity for the Purkinje phase under the different stimulus-conditions. The *O*s were instructed to concentrate their attention upon the disappearance of the detail rather than upon the disappearance of the phase itself, as we found that such judgments were less difficult and less fatiguing.

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[28] A. Samojloff, Zur Kenntnis der nachlaufenden Bilder, *Zsch. f. Psychol.*, 20, 1899, 118.

[29] W. McDougall, The sensations excited by a single momentary stimulation of the eye, *Brit. J. Psychol.*, 1, 1904, 78.

[30] R. Dittler and J. Eisenmeier, *op. cit.*

[31] E. Hering, Eine Methode zur Beobachtung und Zeitbestimmung des erstes positiven Nachbildes kleiner bewegte Objecte, *Arch. f. d. ges. Physiol.*, 126, 1909, 604.

[32] F. W. Fröhlich, *opp. citt.*

[33] D. B. Judd, Apparatus for the control of stationary light stimuli, this JOURNAL, 38, 1927, 107-112.

[34] Diffuse light of one candle per square meter of radiating surface gives one photon of retinal illumination with a pupillary opening of one square millimeter.

RESULTS: EXPERIMENTAL

I. Identification of the after-image phases. Using the method of complete description (Method A), the after-image phases forming the series reported by Dittler and Eisenmeier, and corroborated by Fröhlich were first identified.

TABLE I

Spectral Distribution of Energy for the Light Stimuli

Wave length in mμ	Transmissions				Emission
	Red	Orange	Green	Blue	Black body Radiator at 2790° K.
380	0.0	0.02	0.00	0.080	0.07
390	0.0	0.02	0.00	0.080	0.09
400	0.0	0.02	0.01	0.082	0.11
410	0.0	0.02	0.01	0.084	0.13
420	0.0	0.02	0.02	0.086	0.16
430	0.0	0.02	0.04	0.089	0.18
440	0.0	0.02	0.07	0.115	0.22
450	0.0	0.02	0.10	0.131	0.26
460	0.0	0.02	0.17	0.141	0.29
470	0.0	0.02	0.25	0.142	0.33
480	0.0	0.02	0.34	0.129	0.37
490	0.0	0.02	0.46	0.106	0.42
500	0.0	0.02	0.51	0.077	0.47
510	0.0	0.02	0.52	0.052	0.53
520	0.0	0.03	0.53	0.036	0.58
530	0.0	0.11	0.53	0.025	0.63
540	0.0	0.20	0.52	0.018	0.70
550	0.0	0.30	0.50	0.013	0.76
560	0.005	0.38	0.44	0.010	0.82
570	0.01	0.49	0.36	0.009	0.88
580	0.02	0.61	0.30	0.008	0.94
590	0.03	0.62	0.25	0.008	1.00
600	0.07	0.61	0.19	0.007	1.06
610	0.17	0.61	0.13	0.006	1.12
620	0.28	0.62	0.07	0.005	1.18
630	0.41	0.65	0.05	0.004	1.25
640	0.51	0.68	0.04	0.002	1.31
650	0.61	0.69	0.03	0.002	1.38
660	0.66	0.70	0.02	0.002	1.44
670	0.68	0.70	0.01	0.001	1.50
680	0.69	0.71	0.01	0.001	1.56
690	0.70	0.71	0.00	0.000	1.63
700	0.70	0.72	0.00	0.000	1.68
710	0.71	0.72	0.0	0.0	1.73
720	0.71	0.72	0.0	0.0	1.79

Since the most characteristic attribute of the three positive after-image phases is their hue, whether it be the same as the primary image or complementary, a stimulus yielding a colored primary image was employed. This stimulus consisted of the image of a lantern slide over whose four quadrants four different gelatine filters were placed, which will be hereafter designated as Red, Orange, Green, Blue. The transmissions of these four filters found by the spectrophotometer are given in Table I.

TABLE II
Color Characteristics of Stimuli

Identification of Light Beam	Dominant Wave length mμ	Purity	Transmission for 2790°K.
Black-body radiator at 2790° K.	586	0.61	1.000
Bb beam through Red filter	627	1.00	0.090
Bb beam through Orange filter	593	0.93	0.425
Bb beam through Green filter	559	0.79	0.326
Bb beam through Blue filter	480	0.39	0.016

The color temperature of the source lamp was determined by color matching with a Bureau of Standards lamp whose color temperature had been previously determined as a function of voltage at the Bureau of Standards, Washington, D. C. At a voltmeter reading of 110 (the voltage at which the source lamp was maintained throughout the bulk of the observations) this color temperature was found to be 2785 ± 5° K. This determination was further checked by measuring in watts per mean horizontal candle the power consumed by the source lamp at the same voltage. The color temperature corresponding to this efficiency (0.859 watts per mean horizontal candle) was obtained by interpolating between values previously determined by Forsythe.[35] The color temperature so yielded was 2800° K. with an uncertainty of about 12° arising not so much from experimental error in measuring the light emission as from possible errors in passing from the mean horizontal candle power, which was measured directly, to spherical candle power, the units used by Forsythe. The value of the color temperature finally taken was 2790° K. This accuracy in source specification was necessary because (as will appear later) a change in color temperature of source of 50°, although scarcely perceptible as a color difference in the primary image, produces a change in sensitivity for the Purkinje image that is easily measurable. The emission of a black body radiator at 2790° was found approximately in proportional units from a curve embodying calculations by Priest,[36] and is included in Table I.

The color characteristics produced by a beam from a black-body radiator at 2790° K., together with the color characteristics produced by filtering that beam through the gelatines (Red, Orange, Green and Blue) were computed by a procedure essentially like that recommended by the Committee on Colorimetry for 1920-21.[37] The results of this calculation appear in Table II.

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[35] W. E. Forsythe, *Phys. Rev.*, 18, 1921, 147.

[36] L. T. Troland, Report of committee on colorimetry for 1920-21, *J. Opt. Soc. Amer.*, 6, 1922, 561.

[37] L. T. Troland, *op. cit.*, 580-585.

The transmissions were computed from the color excitation sums by means of the chromatic visibility coefficients which refer to the O. S. A. excitations that were used as the basis of the color computations.[38]

When this four-color slide was used the *Os* descriptions of their after-images varied uniformly with a change of the intensity of the light. There was a very close agreement among the reports of the 5 *Os* who served in this part of the experiment. Less than 1% of the readings showed individual differences. The experiments were commenced after 10 min. darkness adaptation. The fixation-point was adjusted so that it was at the exact center of the exposure field. The duration of the stimulus was 0.14 sec.—close to the optimum for the Purkinje phase.

The following phases were reported by all the *Os*. (1) The primary image which corresponds to Dittler and Eisenmeier's phase (*1*). (2) A flash-image which corresponds to their (*3*) or (*5*), *i.e.* to the Hering image or to the Purkinje image, or to both. (3) A diffuse positive image, colored like the primary but of poor saturation which corresponds to their third positive image, to phase (*7*). (4) A final negative phase, light or dark gray and without hue, which corresponds to their final negative phase (*8*).[39]

As the brightness of the stimulus was increased from the value which just elicited the 'flash-image,' so called because of its duration (estimated to be between 1/5–1/2 sec. in length), the following phases, which we shall call 'stages' to distinguish them from the four phases just mentioned, were reported:

(a) The lowest brightness, less than 10 photons,[40] gave a flash-image that was gray for all four quadrants of the slide. This is plainly the Purkinje image. Fröhlich similarly reports that, with increasing intensity of illumination, the Purkinje image is the first fleeting image to appear,[41] and that a colorless Purkinje image can be aroused after 10 min. dark adaptation with a stimulus too weak to elicit a colored flash-image.[42]

(b) When the brightness of the stimulus is increased four times, all the quadrants are seen in the flash-image as unsaturated violet. This stage was not noted by Fröhlich. In point of fact, he denies its existence, for he states that the Purkinje image is always complementary in hue to the primary image. Other workers are not so positive,[43] and there seems moreover to be ample evidence of the existence of this violet flash-image that is independent of the hue of the primary image.[44]

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[38] D. B. Judd, Chromatic visibility coefficients by the method of least squares, *J. Opt. Soc. Amer.*, 10, 1925, 646.

[39] The final negative phase never appeared immediately after the third positive image, but always after a noticeable blank interval. This description corresponds to that given by Dittler and Eisenmeier. Our *Os* were many times uncertain whether the final negative image appeared at all; but whenever it did, it was always preceded by this blank interval.

[40] It is impossible to be more specific regarding the stimulus brightness corresponding to this stage because—as will appear later—this value varies a hundred-fold, depending upon the wave length of the stimulus, and upon the individual difference of the *O*.

[41] *Op. cit.*, *Zsch. f. Sinnesphysiol.*, 52, 1921, 73.

[42] *Op. cit.*, *ibid.*, 53, 1922, 90.

[43] Dittler and Eisenmeier (*loc. cit.*) merely say that the Purkinje image appears most plainly in the hue complementary to that of the primary image; and Hamaker (*loc. cit.*) simply says that it is complementary in most cases.

Two of the 8 *O*s failed to report this stage. As the intensity of the stimulus was increased they passed directly from stage (a) to stage (c). Thus 2 of our *O*s agree with Fröhlich, and 6 definitely contradict him. Since stage (b) is continuous with the image that is complementary to the primary image, and since the majority of our *O*s report it, we think it reasonable to assume that it corresponds to the Purkinje image. That all our *O*s did not report it is due we think to their individual differences. If this be true, the importance of using more than one *O* becomes evident.

(c) When the brightness is again increased fourfold, the flash-image becomes complementary in hue to the primary image. This is without question the Purkinje phase.

(d) When the brightness is again quadrupled, the flash-image is described as the same in hue as the primary image, but tinged with violet of low saturation.[45] This is probably a superposition of the Hering phase (Phase *3*) upon the Purkinje phase (Phase *5*). Fröhlich, in an attempt to account for the contradictory observations reported in the earlier work, showed that the dark phase (Phase *4*), intervening between the Hering and Purkinje phases, would disappear at a proper selection of the stimulus-intensity and thus give the impression that the two phases were one.[46] Fröhlich does not specify the stimulus-brightness that produces this effect so a recheck of his results could not be made. Our *O*s definitely reported this stage as a unitary experience so, if Fröhlich is correct in his explanation, it will appear that the stationary light pulse method used in the present investigation is too ill-suited for phase identification to bring out this fact.

(e) If the brightness of the stimulus is further increased—doubled or tripled—the flash-image is reported as like the primary image in hue. This is plainly the Hering phase. The Purkinje phase fails to appear. The Hering phase becomes more prominent, at the expense of the Purkinje phase, as the brightness of the stimulus is increased.

(f) In some cases, near the upper limit of the scale of stimulus-brightness afforded by the apparatus, the flash-image was described as double in nature; the first part being of the hue of the primary image; the second part being complementary.[47] The first part of this double-flash is, we believe, the Hering phase (Phase *3*), and the second part is the Purkinje (Phase *5*).

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[44] Bidwell (*loc. cit.*) reports that the hue of the 'ghost' is generally violet; Hamaker (*loc. cit.*) reports that the flash-image following a blue primary image is always violet.

[45] One *O* consistently reported a yellow-green or olive flash-image following a blue primary image.

[46] *Op. cit.*, *Zsch. f. Sinnesphysiol.*, 52, 1921, 73.

II. Spectral effectiveness. From the *Os*' reports of stages (b), (c), and (d) we determined the intensity of stimulus that best brought out the complementary Purkinje image.

The brightness of the stimulus was first calculated in ml., from the brightness of the unfiltered stimulus beam and the integral transmissions of the filters (see Table II), and then changed to photons (see Table IV) by mulitiplying the ml. by the constant 48, which is the conversion factor that corresponds to a pupil diameter of 4.4 mm. We did not measure the pupil diameters of our *Os* but took the average value of the pupil diameter obtained by Reeves,[48] and by Holladay[49] for adaptation to darkness and fixation 11 in. distant.

Fig. 1 shows that light of short wave lengths is more effective in exciting the complementary Purkinje image than light of long wave lengths.

This fact has been observed by several workers, but Fröhlich is the only one who has given quantitative results.[50] It is impossible, however, to compare his results with ours because he neglected to give the spectral transmission of his filters and the brightness of his light. He used daylight as his source of illumination which is, as is well known, subject to the varying conditions of the weather. Fröhlich, nevertheless, points out that light of the shorter wave lengths excites the Purkinje image much more readily than does that of the longer. His results are, therefore, in essential agreement with those presented in Fig. 1.

The effectiveness of the different wave lengths, as shown in Fig. 1, is the reciprocal of the optimum brightness for the complementary stage (c). If this brightness is exceeded in the stimulus, the brightness of the image may or may not increase—probably would increase, but that point has not been settled by our work. But it is known that when the optimum brightness in the stimulus is exceeded, the complementary character of the flash-image is not so marked; and when it is greatly exceeded, the flash-image becomes almost of the same hue as the primary image. The optimum brightness is not measurable with a high degree of precision, but its reliability is comparable to that of the liminal brightness. The probable error of the points plotted in Fig. 1 (the average of 5 *Os*) is: Red—28%; Orange—17%; Green—200%; Blue—16%. The probable error for Green is high because Green, alone of all the colors used, had a complementary which is a fairly close match for the violet-white stage. When *O* reports a violet flash-image, there is no way of telling whether the violet is due to the violet-white stage (b), to the complementary stage (c), or to the beginning of stage (d).

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[47] The *Os* uniformly reported this double image for the Orange and Green quadrants, which were much brighter than the other two quadrants of the slide. Only occasionally did an *O* report this double image for the Red or Blue; and when he did he was always uncertain about it. It is possible that still higher intensities of stimulus would bring out the double images for all the colors.

[48] Prentice Reeves, Rate of pupillary dilation and contraction, *Psychol. Rev.*, 25, 1918, 330.

[49] L. L. Holladay, The fundamentals of glare and visibility, *J. Opt. Soc. Amer.*, 12, 1926 310.

[50] *Op. cit.*, *Zsch. f. Psychol.*, 52, 1921, 74.

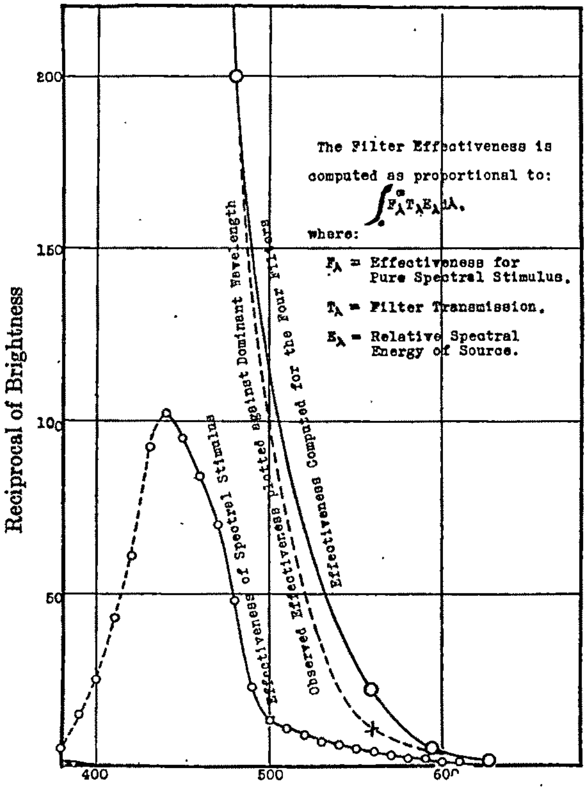

FIG. 1. Showing correlation between wave length and brightness of stimulus that evokes the best complementary Purkinje image.

The effectiveness of the four filters (see dotted curve through X, Fig. 1) is plotted against the dominant wave length. The dominant wave length represents the *color* effect of light of the entire group of wave lengths present in the beam by specifying the wave length of the pure spectral light which, when mixed with white light, produces a color-match. If we wish to compare the filtered beams in brightness, these brightnesses may be set proportional to:

$$\int_0^\infty V_\lambda T_\lambda E_\lambda d\lambda$$

where: V_λ = visibility of light of wave length λ, or the reciprocal of the intensity of light of wave length λ required to match a standard brightness.

T_λ = transmission of the filter for wave length λ.

E_λ = relative energy emitted by the source of light for wave length λ.

This integral can be evaluated (in fact, was evaluated in the calculation of the integral transmissions of the filters) because V_λ is known.

TABLE III

Effectiveness of Spectral Stimulus to Evoke Complementary Stage

Wave length in mμ	Effectiveness[51]	Wave length in mμ	Effectiveness
380	5 v	510	11
390	25 v	520	9
400	25 v	530	8
		540	7
410	43 v	550	6
420	61 v		
430	92 v	560	4
440	102	570	3
450	95	580	2
		590	2
460	84	600	1
470	70		
480	48	610	1
490	23	620	0
500	13	630-720	0

If we wish to compute the relative effectiveness of the filters in producing the complementary stage of the Purkinje image, this effectiveness may be set proportional to:

$$\int_0^\infty F_\lambda T_\lambda E_\lambda d\lambda$$

where: F_λ = effectiveness of spectral stimulus of wave length λ; or the reciprocal of the intensity of light of wave length λ which best evokes the complementary stage of the Purkinje image.

T_λ and E_λ are defined as above.

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[51] Just as V_λ may be built up from the color excitations by applying chromatic visibility coefficients (Troland, *op. cit.*, 549, 551; Judd, *op. cit.*): R—0.45; G—0.54; B—0.01; so may F be built up from the color excitations by applying chromatic effectiveness coefficients: R—0.00; G—0.06; B—0.94. From the very high value for the last coefficient (B) may be seen just how close Bidwell's theoretical suggestion (Bidwell, *op. cit.*, 142) that the 'ghost' is due to action of the violet fibers, alone, is borne out quantitatively. As has been mentioned, Bidwell ascribed the 'ghost' to the violet fibers presumably because his experimental conditions led in great part to the production of the whitish-violet stage (stage b). The above confirmation somewhat paradoxically is derived from measurements on the complementary stage (stage c). The suggestion that the Purkinje phenomenon is due to the action of the same retinal mechanism to which is sometimes ascribed the blue, or violet, sensation, merely because the solution for the spectral effectiveness may be thrown into closely analogous form, must not be taken too seriously. The solution given above is not the only one that will satisfy the data which consists of determinations on four points only. Furthermore, the solution was built up by a cut-and-try adjustment of the color excitation sums referring to the filtered light. It is conceivable that the extraordinary resemblance to the blue or violet excitation is due as much to the method of computation as to the observations themselves.

This integral cannot be evaluated because F_λ as a function of λ has not been evaluated. However, the reverse process is possible. By experiment the effectiveness of the filtered beams has become determined. We, therefore, inquire what values of F_λ will yield by computation the same effectiveness. With only four filters whose value for integral effectiveness must be fitted, it is quite likely that a number of solutions are possible for F_λ. By cut-and-try methods, one of these solutions has been worked out, and is included in Table III.

A plot of the spectral effectiveness is included on Fig. 1. The solid portion of the curve may be regarded as fairly well established by our results. The dotted portion (corresponding to the wave lengths 380-430 mμ, marked 'v' in Table III) is roughly proportional to the visibility. We nevertheless feel that this part of the curve is approximately correct because a stimulus that is not visible is not likely to evoke the complementary stage of the Purkinje image. As far as the data taken in the present work are concerned these values are undetermined because of the use of a source lamp of radiating characteristics like a black body at 2790° K. The energy emitted at wave lengths shorter than 430 mμ is so small a proportion of the total that no information can be yielded about the spectral effectiveness in this region.

TABLE IV

Dominant Wave length in mμ	Optimum Brightness of Stimulus	Observed Effectiveness*	Computed Effectiveness	% Error	P. E. of Observed Effectiveness
480	16	200	201	0.5	.16
559	312	10.3	22.2	120	2.00
593	780	4.0	5.2	18	.17
627	1720	1.8	1.1	39	.28

*Unit effectiveness corresponds to 3200 photons. To calculate the observed effectiveness divide 3200 by the optimum brightness in photons.

As the last two columns of Table IV show, the values of F_λ (given in Table III) satisfy the observed data on filter effectiveness to the degree of accuracy indicated by the probable errors. Although it is possible that an extension of the cut-and-try method would yield another solution for F_λ which would fit the data more closely, further work was not thought advisable because of the large probable error of the experimental results. It must be emphasized that the readings on which the observed effectivenesses are based were not intended to lead to spectral effectiveness, but were taken only for the purpose of phase identification. The evaluation of spectral effectiveness is therefore incidental in our work; it remains for other investigators to obtain absolute results regarding the spectral effectiveness for the complementary stage of the Purkinje image.

III. Distinctness of the images in the different phases. One of the most striking characteristics of the flash-image is the distinctness of its detail. It is so difficult to measure this by the moving slit method that no one seems to have attempted it; but the stationary light pulse method is admirably adapted for this determination.

Four sets of concentric circles (alternating black and white) were used in this part of the work, one set on each of the four quadrants of a slide. The circumferences of the circles in quadrant 2 were twice the width of those in quadrant 1, in quadrant 3 twice the width of those in quadrant 2, and in quadrant 4 twice the width of those in quadrant 3. The circumferences of the circles in quadrant 1 gave a retinal image of 0.007 mm. in width—about twice the distance that two dots must be separated to be distinguished as two[52]—so we assume that the circles in quadrant 1 were separated about 2 j.n.d.'s. The retinal separation of the circles in quadrant 2 would then correspond to 4 j.n.d's, in quadrant 3 to 8, and in quadrant 4 to 16 j.n.d.'s.

The *O* remained in darkness for 10 min. before the experiments were commenced. He was then instructed to report the number of quadrants on which he distinguished the circles in the primary image, in the flash-image, and in the third positive image. He was allowed to look at the stimulus as often and as long as he liked, until he was satisfied that he could give a correct report.

The fixation point was placed at the center of the stimulus. The retinal image of the stimulus then extended approximately 13° from the foveal pit. The brightness of the stimulus was held constant at 33.5 ml., and the duration was varied among the three intervals, 0.124, 0.031, and 0.008 sec. Five brightnesses of the surrounding-field, including darkness, were used, but the results with this variable were so diverse and so inconclusive that a description of them is not warranted. In general, however, it was found that the use of a moderately bright surrounding-field increased the distinctness of both the primary and the flash-images.

The results show that the flash-image is about ¾ as distinct as the primary image. Four of the 6 *O*s saw just as many of the quadrants covered with circles, on some trials, in the flash-image as in the primary image. One *O*, at 0.124 sec. exposure and with a surrounding-field illumination of 0.03 and 0.10 ml., reported a sharpness of the flash-image which enabled him to discriminate as circles the Purkinje image of quadrant 1. We say, therefore, that this Purkinje image had a distinctness corresponding to 2 j.n.d.'s.

Only a few measurements were obtained for the third positive phase. These, however, indicate that the distinctness of this phase corresponds to 8 j.n.d.'s. This phase is much more diffuse than the flash-image, and in general appeared only as a gray blotch.

No detail was ever reported by the *O*s in the final negative phase.

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[52] H. von Helmholtz, *Physiological Optics*, Eng. trans., II, 1924, 37.

Five of the 6 *Os* serving in this part of the experiment reported a very interesting phenomenon which should be mentioned at this point. With a light surrounding-field a negative flash-image appeared between the positive flash-image (Phase *5*) and the third positive image (Phase *7*). This image had a distinctness corresponding to 4 j.n.d.'s. It is termed negative because the black lines of the circles appeared white and the white appeared black. This report was given again and again, and check experiments were made with a slide showing a large black cross on a yellowish white field. As the bluish-white Purkinje image (Phase *5*) of the field faded into the dark phase (Phase *6*), the cross again flashed up momentarily and appeared distinctly brighter than the surrounding field. Then the usual third positive phase (Phase *7*) appeared in which the image was that of a black cross on a light gray field. Though this intermediate phase has never before been reported we did not carry the investigation further.[53]

IV. Sensitivity, a function of stimulus-duration. When the brightness of the stimulus is increased above the lower limit of the range adequate to the flash-image, a gray phase (Purkinje Phase *5*) separates from the third positive phase (Phase *7*). It is difficult to determine the exact point at which this occurs for, as Fröhlich points out, the beginning of the third positive phase first becomes a little lighter and then it gradually separates from the dim remainder. This remainder however, is much more diffuse than the Purkinje phase. Hence the *Os* were instructed in the next series of experiments (Method B) to concentrate their attention upon the appearance of a detailed image. The stimulus-brightnesses obtained in this way were approximately twice as high as those obtained by concentrating the attention upon the appearance of a flash-image.

A light surrounding-field (0.005 ml.) was used instead of a dark field because: (1) a condition of adaptive equilibrium can be reached in 10 min. while with the dark field this state is reached only after 1 hr.; and (2) equilibrium condition once reached is stable, while with the dark field the bright stimuli presented to evoke the Purkinje image seriously disturb the adaptation level, at times toward light adaptation and at other times toward dark adaptation.[54] The slide used in the liminal determinations showed a series of fine, black, crosslines (width 0.5 mm.) on a yellowish-white background (radiating characteristics of black body at 2790° K.) on which was also superposed a heavy irregular, black cross (width of line, 7 mm.). The heavy cross was used because its image was always visible in the third positive phase (*7*) while the fine crosslines never were. This arrangement made easy the distinction of the two phases, (*5*) and (*7*).

Fig. 2 (a typical curve of our results) shows the variation of liminal brightness with duration of stimulus. It is high for short durations, falls as the duration is increased, reaches a minimum at about 0.16 sec., and then rises again at the larger durations. Each point plotted is the mean of four liminal settings, and has a probable error of 25%. The curve, for every *O*, is accurate enough to evaluate consistently the time interval within which a sharply defined Purkinje phase readily appears. The time in sec., corresponding to the two places where the curve crosses the 120 photon mark, has been recorded on Fig. 2 as a measure of this time interval for each of the 6 *O*s. The lowest stimulus-brightness which excites a sharply-defined Purkinje phase is also given in photons for every *O*, as is also the time corresponding to this minimum brightness (the optimum time). The *O*s agree closely in the values for the optimum time,

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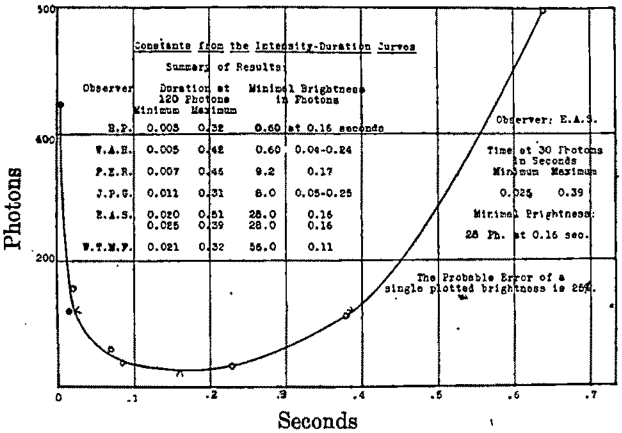
[53] In one of Von Hess' early papers (*op. cit.*, *Arch. f. Ophth.*, 40, 1894, 259) a negative image of very short duration was reported, but it was subsequently identified as one of the usual negative phases in which the dark parts of the primary image did not change to bright.

[54] The effect of flashes of light well above the limen, on the course of adaptation to darkness has been studied. Cf. H. v. Helmholtz, *op. cit.*, ii, 1924, 323.



Constants from the Intensity-Duration Curves

Summary of Results:

Observer	Duration at 120 Photons Minimum	Maximum	Minimal Brightness in Photons	
B.P.	0.003	0.32	0.60	at 0.16 seconds
W.A.H.	0.005	0.42	0.60	0.04-0.24
P.E.R.	0.007	0.46	9.2	0.17
J.P.G.	0.011	0.31	8.0	0.05-0.25
E.A.S.	0.020 0.025	0.51 0.39	28.0 28.0	0.16 0.16
W.T.M.F.	0.021	0.32	56.0	0.11

Observer: E.A.S.

Time at 30 Photons in Seconds Minimum	Maximum
0.025	0.39

Minimal Brightness:

28 Ph. at 0.16 sec.

The Probable Error of a single plotted brightness is 25%.

FIG. 2. Showing correlation between duration of stimulus and photons of intensity required to evoke just noticeable detail in the Purkinje image.

but differ greatly (100 to 1) in the brightness values required at this duration. It seems reasonable to conclude, however, that the average *O* will see a sharply-defined Purkinje phase if the duration of stimulus is between 0.02–0.30 sec., and the brightness of the stimulus is between 200–800 photons. The conclusion, however, probably holds only for stimuli that possess radiating characteristics close to those of a black body at 2790° K.[55]

The uncertainty of the determinations is too great to warrant the casting of our results in the form of an equation; still, the curves always rose with short durations and indicate that, within the probable error, the product of the duration and the stimulus brightness is a constant. This suggests that a retinal element must absorb a certain quantum of light energy before it gives rise to a sharply defined Purkinje phase.

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[55] The color of the stimulus was not varied in this experiment. Color has been found to be of great importance in this work.

The subsequent rise in liminal brightness for increasing durations of stimulus quite plainly corresponds to factors of a kind entirely different from the cause of this initial decrease. At these durations (0.2–0.5 sec.) the failure of *O* to experience a detailed flash-image was due (1) to the rise of the final negative phase which blotted out the flash-image, and (2) to the fact that the primary image was so dazzling that it showed no detail, in which case the Purkinje phase would, of course, show none.

It must be admitted that the causes of the changes represented by the summary of results in Fig. 2 is somewhat obscure. It is likely that the chief value of this material lies in the fact that it defines the conditions of intensity and duration of stimulus under which the sharply-defined Purkinje phase may be expected to appear.

V. Sensitivity, a function of dark adaptation. If an attempt is made to determine with a dark surrounding-field, the liminal brightness just required to evoke a detailed Purkinje image, the results show that during the first 10 min., approximately, the liminal values decline and appear slowly to be approaching a constant. As the work is carried further, and the stimulus intensities reduced still more to determine another limen, the *O*s frequently observe that though the intensity of the stimulus has been decreased the Purkinje image seems much plainer than before. The next limen is always much lower—usually about 1/10 the previous value. It is evident therefore that there is a discontinuity in the rate of adaptation.[56]

The brightness at which this occurs was measured for 4 *O*s.

The same slide was used as in *Part IV*; the duration of stimulus was held constant at 0.11 sec. The surrounding-field was dark; the fixation point was placed at the center of the stimulus and was adjusted before each determination to a brightness that did not distract attention from the Purkinje image. *O*, after looking out of the window for 5 min., commenced liminal determinations (Method B), approaching first from the side of higher stimulus intensities, then, in the next determination, from the side of lower intensities.

During the first 10 min., the Purkinje phase was blue-violet where the stimulus corresponded to the radiating characteristics of a black body at 2790° K. After the sudden increase in sensitivity, this Purkinje phase at close to liminal brightnesses was described as 'colorless,' 'slate gray,' or 'gray.'[57] The lines which were black in the primary image always appeared black in the Purkinje phase also. It must be emphasized that detail still appeared in the Purkinje image when color of a definite hue had become almost indistinguishable.

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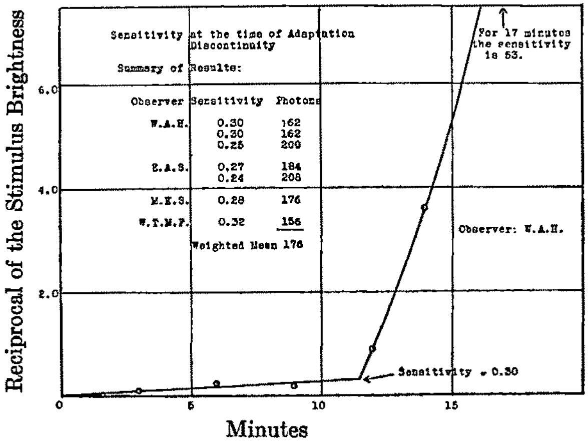
[56] This sudden change in the rate of adaptation is indicated in the work of Fröhlich (*op. cit.*, 53, 1922, 79-107) though it is not emphasized by him. It is gratifying to report that our work verifies his in this respect.

The results of a typical experiment of this sort are plotted in Fig. 3. The liminal brightnesses corresponding to a given number of minutes following light adaptation are shown reciprocally after the manner of Fröhlich,[58] and Piper.[59] The reciprocals refer to brightness of the slide image in ml. The sensitivity at the discontinuity in rate of change of sensitivity is just below 0.30, which corresponds to approximately 3.4 ml., or 164 photons.



Sensitivity at the time of Adaptation Discontinuity

Summary of Results:

Observer	Sensitivity	Photons
W.A.H.	0.30	162
	0.30	162
	0.25	200
E.A.S.	0.27	184
	0.24	208
M.E.S.	0.28	176
W.T.M.P.	0.32	156
	Weighted Mean	176

FIG. 3. Correlation between the time after daylight adaptation at which the limen was determined and the reciprocal of the stimulus intensity required to evoke just noticeable detail in the Purkinje image.

The weighted mean of all determinations made by the 4*O*s is 176 photons.[60] We conclude therefore, that, at a liminal stimulus of brightness close to 180 photons, sensitivity for the sharply-defined Purkinje phase increases suddenly. Troland gives 100 photons as the stimulus-brightness necessary to insure, after steady stimulation of 10 min. the complete adaptation of the retinal rods to insensitiveness.[61] We assume, then, that 180 photons corresponds approximately to the brightness of a momentary stimulus just low enough to permit functioning of the rods. It is then but an easy step to say, as is suggested by Fröhlich in this connection, that the discontinuity in rate of change of sensitivity marks an appearance of the rods in a rôle in which they were previously absent. The experiment does not, however, serve to identify the new rôle.

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[57] Fröhlich also determined an adaptation curve for the complementary Purkinje image (*i.e.* a higher intensity in the stimulus will change the gray image to the complementary image, or stage c, see Part I, this section). As mentioned before in this connection, 6 of the 8 *O*s reported a stage (*b*) of the Purkinje phase intermediate between the gray stage (*a*) and the complementary stage (*c*), that is, an intermediate whitish-violet stage. For these *O*s an adaptation curve for the complementary stage would logically have to be preceded by a curve for the whitish-violet stage.

[58] Fröhlich, *op. cit.*, 53, 1922, 91.

[59] H. Piper, Ueber Dunkeladaptation, *Zsch. f. Psychol.*, 31, 1903, 161.

[60] Some determinations were given greater weight than others because the corresponding curves showed a more definite break.

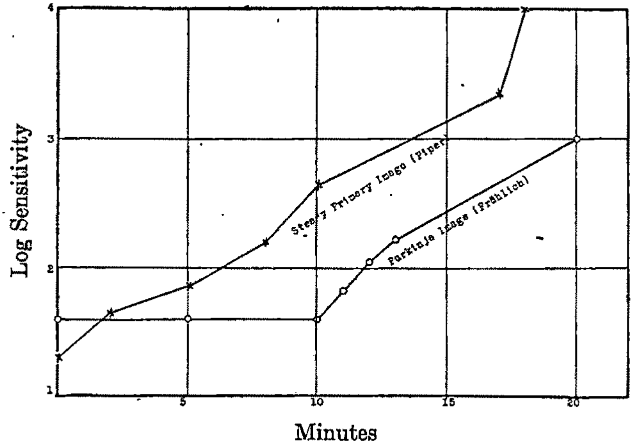

FIG. 4. Correlation between minutes of dark adaptation and the logarithm of sensitivity.

Fröhlich obtained experimental results showing the same discontinuity in rate of change of sensitivity by setting upon the appearance of a separate Purkinje phase instead of (as in the present work) the appearance of a sharply-defined Purkinje image. He interpreted his findings, however, as supporting the known course of dark adaptation for steady vision. He says that the study of the Purkinje phase allows the sensitivity of the visual organ to be studied in a new way, supplementary to the method of Piper,[62] and that the two methods yield the result that adaptation to darkness commences only after 10 min. The discontinuity in the slope of the sensitivity curve for the Purkinje phase after 10 min. adaptation to darkness is not commented upon by him.

If the discontinuity in adaptation rate is caused by the rods, Fröhlich's contradictory conclusion, from essentially similar data, must be incorrect. The curves plotted by Piper show that the sensitivity stays *very low* during the first 10 min. of dark adaptation compared to the heights attained after 20 min. Reference to the table of sensitivities from which the curves are plotted shows that adaptation *is* going on during the first 10 min., and, in some cases, *faster in the first 10 min. than at any other time.*

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[61] Troland, *op. cit.*, 555. [62] Piper, *op. cit.*

To make this point evident, the sensitivities obtained by Piper from steady vision have been compared, in Fig. 4, with those obtained by Fröhlich from settings on the Purkinje phase. The logarithm of the sensitivity[63] is plotted against the duration of adaptation to darkness, taking as typical of Piper's data a run in which the *O* was Piper, himself, and as typical of Fröhlich's data on the Purkinje image, a run shown in curve form in Fröhlich's own paper.[64] From Fig. 4 it is evident that the two methods of studying adaptation to darkness measure different processes. The curve of Piper's results shows that the adaptation to darkness, of steady vision, proceeds at a fairly constant rate from the very beginning; the curve representing Fröhlich's results shows plainly the increased rate of change in sensitivity for the Purkinje image commencing at 10 min.

The conclusion suggested by the present work that at a stimulus-brightness of about 180 photons the rods function in the production of the Purkinje image is therefore substantiated by the Fröhlich data in spite of the fact that he does not so interpret it.

VI. Individual differences. The fact that in the rapid after-image phases, of which the Purkinje phase is one, we are dealing with phenomena that are less stable and more variable than ordinary vision, deserves special emphasis. The only reason any degree of agreement has been obtained among the different investigators seems to lie largely in the fact that the most striking individual differences are quantitative in character. The failure of the workers to attempt a quantitative specification of stimuli has allowed these individual differences to go unnoticed.

There seems, however, considerable justification for the statement that the same processes occur in most individuals. The stimulus-intensities and durations required to evoke identical processes in two different and supposedly normal, individuals are exactly the same, and are often markedly different. (See summary of results in Fig. 2; the minimal brightness is seen to vary almost in a 100 to 1 ratio between observers.)

There has, however, been some evidence of actual absence of normal processes in certain otherwise normal *O*s. Thus 2 *O*s (H. P. and W. A. H.) failed to see the whitish-violet stage of the Purkinje image described by the other 6 *O*s. It may be pointed out that the same 2 *O*s also gave by far the lowest minimal brightness (Fig. 2) for the appearance of a sharply defined Purkinje image.[65]

A study of the effect of dark adaptation upon the stages of the flash-image was made with 1 *O* only (W. T. M. F.). This study was undertaken because this *O*, in contrast to all the other *O*s, failed to report a Purkinje image under certain stimulus conditions. It was found that he saw the Hering phase during the first 10 min. of dark adaptation; and then, when adaptation was carried further, he saw the Purkinje image which was normal to all the *O*s at the stimulus intensity and duration used. In observations taken with the other *O*s, increased dark adaptation functioned as the analogue of increased stimulus intensity; for this *O*, however, light adaptation definitely favored the Hering phase to such a degree that the smallest stimulus brightness that would elicit any flash-image at all, failed to yield the Purkinje phase. It may be possible to connect this failure of the Purkinje phase with the high liminal brightness required by this *O* for eliciting the Purkinje phase after adaptation to darkness, but, what is of immediate significance is the establishment of the fact that individual differences in quantitative results do occur.[66]

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[63] The course of dark adaptation, as shown by plotting the logarithm of sensitivity, has already been discussed. Cf. H. v. Helmholtz, *op. cit.*, ii, 1924, 321.

[64] Fröhlich, *op. cit.*, 53, 1922, 91.

[65] It may be mentioned that the bio-luminescence hypothesis (discussed in the section to follow) would require the disappearance of the whitish-violet stage of the Purkinje image for all *O*s exhibiting an extremely high sensitivity to the sharply-defined Purkinje phase. According to this view, then, absence of the whitish-violet stage does not mean the absence of a process but corresponds to a quantitative distinction, namely: an extraordinarily high cone 'fatiguability' attendant on the high cone sensitivity required for discrimination of detail at low brightness levels.

RESULTS: THEORETICAL

From Bidwell,[67] who accounted for the Purkinje images (the 'ghost' or 'recurrent image') as due to the action of the violet-sensitive fibers of the retina, up to Fröhlich,[68] who compared the entire after-image series to other periodic reactions of the nervous system and considered them as periodic reflexes of perception, many explanations of the phenomena have been proposed. While a few authors have remained discreetly silent concerning theories, still particularly since the work of Von Kries, there has appeared in the literature much argument and speculation as to whether the Purkinje phase was primarily a rod phenomenon (as Von Kries maintained), or a cone phenomenon (as the appearance of colored Purkinje images seemed to indicate).

As noted above, however, there existed much confusion in the identification of the separate phases of the after-image series. Under these conditions it is not surprising that theories concerning the cause of a single phase should be inadequate. Since practically all doubt in phase identification has recently been removed, the chance of arriving at a consistent explanation of the Purkinje image is now much better and a reconsideration of theory seems to be warranted.

It must be pointed out, at the outset, that the principal stumbling block to the theorists has been the fact that although the Purkinje phase was undoubtedly colored under many experimental conditions, and, therefore, seemed to depend on the action of the cones, still, the image often failed to appear in the fovea centralis and always failed to appear for a primary stimulus of pure spectral red. These latter two established facts point to a direct dependence on rod action, rods being not only absent in the fovea centralis but also insensitive to spectral red. An assumption, perhaps as simple as any, which is capable of reconciling this apparently contradictory nature of the Purkinje phase is that it results from a complex process depending both on the cones and on the rods. The present immediate purpose is to apply this assumption to our experimental results.

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[66] Three *O*s (J. P. G.; E. A. S.; W. A. H.) independently reported that the Purkinje image with dark surrounding field seemed to jump closer to the eye than the primary image (*i.e.* seemed to leave the plane of the screen on which the slide-image was focussed and to appear perhaps 4 in. out in the space between the screen and the observing eye).

The same *O*s reported a flash image in some cases which consisted of two colors, one nearer to the eye than the other, as if (W. A. H.) "one were looking at one color through a semi-transparent film having a different color;" or, as if (E. A. S. following an orange primary image) "the green-blue Purkinje phase was suddenly overlain with a violet sheen."

One *O* (W. A. H.) reported consistently the appearance of the Purkinje image followed by the Hering image. This was for the blue primary image only. He reported that the blue phase was too quick to be the third positive phase which remained as a diffuse gray patch for several seconds after the disappearance of the blue flash.

[67] Bidwell, *op. cit.*, 142.

[68] Fröhlich, *opp. citt.*

I. Identification of the after-image phases. Three stages in the Purkinje phase are identified as intensity of the stimulus is increased: (a) the gray stage, independent of the hue of the primary image; (b) the whitish-violet stage, also independent of the hue of the primary image; and (c) the complementary stage, in which the Purkinje phase is complementary in hue to the primary image.

These three stages, (a), (b), and (c) may be set in exact correspondence to the stages (a′), (b′), and (c′), respectively, which would be caused from a stimulation of the retina by an hypothetical secondary stimulus of whitish-violet light of increasing intensity following the original stimulus, thus: (a′) the gray stage is due to stimulation of the retina by the whitish-violet light (*i.e.* accurately, light of all wave lengths with the shorter wave lengths predominating in intensity), but the intensity of this light is below the levels of cone sensitivity; (b′) the whitish-violet stage corresponds to all the cases (except c′) where the secondary light is of high enough intensity to affect the cones; (c′) the complementary stage corresponds to stimulus intensities and durations large enough to fatigue the color-sense ascribed to the cones so that the secondary light, which by unfatigued cones would appear whitish-violet, now appears of a hue complementary to that of the primary image, the whitish-violet character having been rendered undetectable because of the instantaneous (since there is not time for precursor action to replenish sensitive substance) fatigue of the cones.

To account for this hypothetical secondary radiation it is necessary to assume that some retinal structure gives off light about 1/5 sec. after the original stimulation. It has been long, if not widely known, that phenomena exist which are easily explained by the assumption of bio-luminescence of retinal structures. The blue-arcs of the retina[69] have been shown by Ladd-Franklin[70] to be due to bio-luminescence of active nerve fibers.

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[69] W. R. Amberson, Secondary excitation in the retina, *Amer. J. Physiol.*, 69, 1924, 354-370.

[70] Christine Ladd-Franklin, The blue arcs of the retina, 'Seeing your own nerve currents,' *J. Opt. Soc. Amer.*, 12, 1926, 494.

If we are to assume that the positive character of the Purkinje phase depends on the actual emission of physical light by excited nerve tissue, it seems natural to fasten upon the nerve structure closest to the receptor layer of the retina as the most probable source of the whitish-violet secondary radiation.

II. Spectral effectiveness. The effectiveness of a spectral stimulus for producing the complementary stage of the Purkinje phase is approximately determined as a function of wave length. If bio-luminescence of a retinal structure does occur, the effectiveness would depend on the condition of the receptors (rods and cones) before the primary stimulation (production of the nerve activity which causes bio-luminescence) and would also depend on cone fatigue due to primary stimulation (response to the secondary radiation caused by bio-luminescence). The relation of spectral effectiveness to wave length gives information (according to this hypothesis) about the combined effects of: (1) the production of bio-luminescence; and (2) the instantaneous 'fatiguability' of the retinal cones. Without more specific knowledge about the exact retinal structures concerned in bio-luminescence, it seems unwise to attempt a resolution of the combined effect (which is, itself, none too accurately established) into its components.

III. Distinctness of the images in the different phases. The distinctness of the Purkinje image is shown to be surprisingly high—certainly no lower than ¾ that of the primary image. If the bio-luminescing structure were far out in the retinal layers away from the receptor layer, a sharp Purkinje image could not arise. The indication is, therefore, that the bio-luminescing structure is close to the light-sensitive layer of the retina.

IV. Sensitivity, a function of stimulus-duration. The conclusions indicated by the fact that liminal brightness for the production of a detailed Purkinje image rises both for an increasing to high values of stimulus-duration and for a decreasing to low values must be referred specifically to bio-luminescence during or following the occurrence of nervous activity in a fiber, instead of indefinitely to the process producing the Purkinje phase, as follows:

(1) Bio-luminescence is aroused optimally by a stimulus of 0.16 sec. duration. (2) It is possible that bio-luminescence requires a definite minimal energy of light (or, perhaps, of nerve activity) to release it.

These results do not, of course, constitute a test at all crucial for any detail of the hypothesis.

V. Sensitivity, a function of dark adaptation. It is noted that detail appears in the Purkinje image at about the stimulus-brightness which marks the transition from the gray stage (Stage a) to the whitish-violet stage (Stage b). This appearance of detail coincident with the perception of hue substantiates the conclusion drawn from *Part I* that the intensities of the bio-luminescent light which produce the gray stage in the Purkinje image correspond to light intensities below the cone level of sensitivity, and that the intensities of bio-luminescent light producing the whitish-violet stage (Stage b) correspond to light intensities just above this level.

Further, a conclusion is suggested from the discontinuity established in rate of change of sensitivity, that the rods of the retina after about 10 min. adaptation to darkness enter into a new rôle in the production of the Purkinje image. The bio-luminescence assumption now under consideration makes it possible tentatively to name this new rôle. According to the bio-luminescence hypothesis the rods may act in two ways: (1) to produce the nerve activity which causes bio-luminescence; and, (2) to respond again under the influence of the light produced secondarily from their own previous activity. For stimulus-brightnesses corresponding to the flat part of the sensitivity curve (*i.e.* brightnesses greater than 180 photons) the rods are bleached to insensitiveness by the first light pulse, the bio-luminescence due to which is, therefore, perceived only by the cones. For stimulus-brightnesses corresponding to the steep part of the sensitivity curve (*i.e.* brightnesses less than 180 photons) a part of the sensitive substance in the rods remains after the action of the primary light by means of which substance (presumably the visual purple) the secondary light is perceived. If we make this designation of function for the rods, then, not only is the discontinuity in rate of change of sensitivity accounted for, but also, with fair plausibility, the stimulus brightness (180 photons, or about twice the brightness required to bleach the rods to insensitiveness by steady stimulation) at which it occurs.

The assumption of a complex process involving both rods and cones for the production of the Purkinje phase leads, then, to a workable hypothesis, many of the attributes of which have been defined by applying the experimental results of the present work. This hypothesis involves a series of processes which may be summarized as follows:

(1) By the usual direct action of light on the sensitive retinal layer, a photo-chemical reaction takes place in the rods and cones.

(2) Nerve activity results from this chemical change in both cones and rods which leads to two effects:

(*a*) the perception of the usual primary image; and (*b*) the bio-luminescence of the nerve fibers (or surrounding structures) close to the rods and cones. Since the rods, if not light-adapted, are always much more sensitive to light than are the cones, the nerve activity due to them must be much greater than that due to the cones. Hence, most, if not all, the bio-luminescence causing the usual Purkinje image results from the tremendous pulse of nerve activity following instantaneous bleaching-out of visual purple in the rods.

(3) The secondary radiation produced by the bio-luminescence of the nerve fibers is almost non-selective, the shorter wave lengths being slightly predominating.

This light is perceived:

(*a*) by both rods and cones as the whitish-violet stage (b) or, less likely, the complementary stage (c);

(*b*) by the rods alone, if the bio-luminescence is so weak that it is below the level of cone sensitivity, as the gray stage (a); or

(*c*) by the cones alone, if the primary radiation is intense enough to bleach the rods to insensitiveness, as the completementary stage (c) or, less likely, whitish-violet stage (b).

This hypothetical series of retinal events suffices to explain the results of the present investigation; it is interesting to view a few cases previously reported in which this hypothesis serves to harmonize the contradictions.

As mentioned before, some investigators maintain that the Purkinje phase disappears[71] at and near the retinal region of most distinct vision. Following Fröhlich's excellent treatment of the fact of appearance of the Purkinje phase in the fovea under some conditions and its non-appearance under others, we must conclude: (1) that the sensitivity of the Purkinje phase is much less within the extreme central region (fovea centralis, approximately 1° in diam.) than within any other retinal region; (2) that the sensitivity increases rapidly in the region between 1°–7° in diam. (the macula lutea and the so-called para-foveal region); and (3) that it remains substantially at this high value for the remainder of the retina.

We would expect, according to our bio-luminescence hypothesis, that three retinal regions would be differentiated on the basis of sensitivity.

(*1*) The periphery (all of the retina except a central area of 3°–4° in diam.) should be the region of highest sensitivity because rods are present in large numbers. The sensitivity should decrease slightly toward the fovea because of the decrease in the number of rods compared to the number of cones in the para-foveal region.[72]

(*2*) A semi central zone, comprising the remaining 3°–4° (with the exception of the foveal pit, itself, which is roughly 1° in diameter), should yield a lower sensitivity because of two factors: (a) the proportion of the number of rods to the number of cones[73] continues to decrease—as in (*1*)—and would lead to the prediction of a continued gradual decrease in sensitivity; and (b) the absorption of light of short wave length by the macular pigment which overlies much of this zone would lead to the prediction of a still lower sensitivity.

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[71] V. Kries, (*op. cit.*). Cf. also Samojloff, (*loc. cit.*) who gives the area of the retina insensitive to the Purkinje phase as between three and four degrees. Fröhlich, (Ueber den Einfluss der Hell und Dunkeladaptation usw., *loc. cit.*) gives the area insensitive under certain stimulus conditions for the Purkinje image an extent varying from 4°-7° in diam.

[72] Quain, Anatomy, III, 1897, 47.

[73] The evidence for the presence of rods in the semi-central zone is entirely indirect (based on the Purkinje effect). See Helmholtz, *op. cit.*, 2, 1924, 363. See also Troland, *op. cit.*, 555. There seems to be no direct anatomical evidence of rods within the 3° area. If it should be assumed that this region is rod-free, sensitivity for the Purkinje phase should drop to a very low value on that account alone.

(*3*) The foveal pit, should yield a much lower sensitivity than any other region, because here in the normal *O* there are no rods and no overlying nerve tissue. There are, then, two ways by which a Purkinje phase could become visible in this region. (a) By a light stimulus falling upon the foveal pit, causing the cones there to give rise to nerve activity sufficiently intense to evoke bio-luminescence. This method would presuppose that the cone fiber just within the *membrana limitans externa*[74] is the bio-luminescing structure since so little nerve tissue overlies the foveal pit. (b) By a light stimulus falling upon the retina in the vicinity of the fovea causing bio-luminescence of sufficient intensity to affect not only the retinal elements external to the fovea centralis by whose action it was caused, but also, by scattered light, the cones of the foveal pit which are very close to them. It is seen that the sensitivities arising from either chain of events (a) or (b), would be lower than that characterizing the semi-central zone.

These three retinal regions differentiated on the basis of sensitivity to the Purkinje phase by the bio-luminescence hypothesis are clearly in very close correspondence to the findings of Fröhlich.[75]

There are two very striking experiments reported by Bidwell,[76] a satisfactory explanation for all details of which has not yet been offered.

(*1*) Following a moving spectrum, each separate region of which gave a complementary colored 'ghost,' appeared a 'ghost' (*i.e.* Purkinje phase) which was *all violet.*[77] From this observation Bidwell suggests that the 'ghost,' is due to the action of the violet fibers (the violet of the fundamental red, green and violet of the Young-Helmholtz theory of color-vision) of the retina only. He ascribed the complementary phases, previously observed, to errors in color judgment since no basis for color judgment, except the saturated primary image of one hue, was available in the field of vision. It has been established since by many investigators that the complementary character of the Purkinje phase is real and not based upon errors of color judgment.[78] Bidwell's suggestion, must, therefore, be rejected. Since, however, as is common to all investigators before Fröhlich, no specification of stimulus-brightness is made, we think it reasonable to assume that the brightness of the spectrum used by Bidwell was so low that the cones momentarily affected by it were not fatigued. The subsequent hypothetical bio-luminescent light would, therefore, be perceived in its true whitish-violet character. The intensity of the image indicated by the diagram of the 'ghost' spectrum following the primary image of the spectrum is strongly suggestive of rod visibility (as the bio-luminescence hypothesis requires) because it shows a shift in maximum intensity with respect to the primary image toward the short wave lengths.

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[74] Quain, *loc. cit.*, 53.

[75] The structural distinction between the fovea centralis and the semi-central zone of the retina seems to give a tool by means of which we should be able to determine with considerable precision the retinal element producing bio-luminescence. Thus, if the fovea, alone, be stimulated by an intense light pulse, a Purkinje phase seen at the fixation point would suggest that method (a) was the true method by which foveal Purkinje phases are produced. If this Purkinje phase were surrounded by a brighter ring more than one degree in diameter, we should conclude that the bio-luminescence had also occurred later in the chain of events than the cone fiber (*i.e.* had occurred in the early neural structures which do not overlie the foveal pit, but do overlie the semi-central zone). This experiment is difficult because of at least the following three factors: (1) Light scattered by the optical media of the eye may be sufficiently strong to arouse a Purkinje image in the semi-central zone in the ordinary way; (2) steady fixation is essential to the success of the experiment, and; if something suggesting a halo were reported, the question of accommodation error and possible failure to control fixation could always be raised; and (3) the Hering phase does not suffer a marked change in sensitivity as the center of the retina is approached and would, therefore, always be much more prominent than the Purkinje phase toward which observation should be directed.

With a moving stimulus, the last difficulty is not present because phase identification is much easier with that method. If we take a slit of light falling vertically across the fovea but a little to one side of the center, and observe the Purkinje phase to be not a straight slit like the primary phase, but showing rather a bending out around the border of the fovea, it might be concluded that method (B) correctly placed the seat of bio-luminescence. If the slit is stationary, the above-mentioned difficulties would be encountered; if moving, then an observed bending out as the fovea was approached could also be interpreted as a longer development time of the Purkinje phase for the semi-central zone. This experiment has, in fact, been tried out by Fröhlich (*loc. cit.*, 53, 79-107); a bending-out ("Ausbuchtung") has been observed by him; and he did place the above alternative explanation on his results.

For these reasons, then, although experiment does not gainsay the bio-luminescence hypothesis, the structural difference between the fovea centralis and the semi-central zone cannot be made use of easily to locate the seat of bio-luminescence, providing, of course, bio-luminescence is actually present.

[76] Bidwell, *op. cit.*, 137, 139.

(*2*) It was further reported that a yellow primary image from a slit illuminated by a mixture of spectral red (which gave no 'ghost') with spectral green (which gave a violet 'ghost') was followed by a blue 'ghost,' exactly as was a yellow primary image evoked by a spectral yellow. According to the bio-luminescence hypothesis, the reason spectral red is ineffective (see Fig. 1) is that it does not affect the rods from which the nerve activity causing the bio-luminescence arises. Spectral red is, however, effective in fatiguing the cones, and will, therefore, play its part in rendering the Purkinje phase complementary in hue to the primary image. The bio-luminescence producing the 'ghost' following the yellow primary image is due entirely to the green component which alone affects the rods; the fatigue of the cones is due to both the red and the green components.

The bio-luminescence hypothesis, therefore, accounts for some specific observations heretofore remaining unexplained. Further, all reliable conclusions concerning the Purkinje phase may be put into terms of the hypothesis. It has, throughout the course of the present work, exhibited all the characteristics of a successful hypothesis. Whether the chain of retinal events making up the bio-luminescence hypothesis or some modification of this chain of events really exists remains to be decided by subsequent experimental work.

SUMMARY

(1) As the brightness of the stimulus is increased the Purkinje image passes through three characteristic stages: (*a*) gray, (*b*) whitish-violet, and (*c*) complementary to the primary image.

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[77] Bidwell says that the spectrum was moved parallel to itself across the field of vision, which might imply that a given retinal element was affected in turn by light of all wave lengths before the 'ghost' appeared from its action. Subsequent discussion of the results and a diagram of the 'ghost' spectrum make it seem likely that the spectrum was really moved perpendicular to its dimension of greatest length, or in such a way that a given retinal element was affected by light of a single wave-length and, then, without further stimulation exhibited the 'ghost.' If the former statement really represents the experimental procedure, the bio-luminescence hypothesis would require a whitish-violet 'ghost' regardless of the intensity of the stimulus, since each cone would be fatigued successively by light of all wave-lengths, the resulting fatigue effect being, of course, non-selective so that the true whitish-violet character of the bio-luminescence radiation would become evident.

[78] The Purkinje phases of striking complementary character reported in our work, following a stimulus each quadrant of which was differently colored, shows definitely that the complementary character does not arise from lack of a basis for color judgment.

(2) Light of short wave length is more effective than that of long wave length in evoking the complementary Purkinje phase. The effectiveness of spectral light in evoking the complementary stage has been evaluated as a function of wave length.

(3) Under favorable conditions the Purkinje image is not less than ¾ as distinct as the primary image.

(4) The brightness of the stimulus evoking a distinct Purkinje phase is lowest when the stimulus has a duration of 0.16 sec. If the duration of the stimulus is greater than 0.02 and less than 0.30 sec., a distinct Purkinje phase may be aroused by a stimulus whose brightness lies between 200 and 800 photons.

(5) The lowest brightness of stimulus required to evoke a distinct Purkinje phase varies with the observer from 0.60 to 60 photons for a stimulus having the radiating characteristics of a black body at 2790° K.

(6) The semi-quantitative findings of Fröhlich concerning the effect of adaptation to darkness on the Purkinje phase are corroborated. The brightness of stimulus required just to evoke a distinct Purkinje phase is measured in photons as a function of dark adaptation.

(7) Our results suggest that some retinal structure close to the light-sensitive layer gives off physical light about 1/5 sec. after the beginning of the stimulus.

(8) The assumption that a retinal structure exhibits this property of bio-luminescence serves to explain certain apparent contradictions in former results.

'FLUCTUATIONS OF ATTENTION' WITH WEAK VISUAL STIMULI[1]

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I. Introduction

The problem of 'fluctuation of attention' has been settled and unsettled at least four times in as many generations of experimental psychology.[2] Whether the observed changes in minimal sensory experiences should be attributed to periodic lapses of attention or to extra-attentional factors has been the chief question. For those to whom it means fluctuation the attentional changes have been of two kinds. If we take the two-level theory of attention as our basis,[3] the disappearance of a minimal quality might mean either a change of content of the upper level (a loss of clearness due to a redistribution of vividness in the 'mental content') or it might mean a periodic loss of the degree of clearness of the upper level regardless of change of content—a periodic loss of efficiency of attention, to put it in functional terms.

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[1] From the Psychological Laboratory of Cornell University.

[2] It is a pleasure to make acknowledgment to Professor K. M. Dallenbach who, besides serving as an observer, gave me constant help and counsel on matters of technique and procedure.

[3] E. B. Titchener, *A Textbook of Psychology*, 1910, 276.

The chief questions upon which the outcome has depended are two. (1) Is the phenomenon found in all sense departments? If it is a matter of attention the answer to this question must be in the affirmative. (2) Are the conditions for fluctuation peripheral or central? In order to be a matter of attention it has been assumed that the conditions must be central. For those who hold the first view of fluctuations of attention the conditions may be central but local; for those to whom there is a periodic loss of attentional ability, the conditions must have a general central basis. Other questions have interested the latter group, namely, Why are the fluctuations rhythmical? What determines their rate? These questions have led to attempts to correlate fluctuations with various bodily rhythms.

The history of the problem presents a typical picture of development in experimental psychology. On the side of theory we often find, especially during the early period, the expression "fluctuations of *the* attention"—ghosts of a pre-experimental period; and all the way through we find psychological theory mixed with or opposed to physiological theory. On the side of experiment we find one crucial test after another as laboratory technique has improved. We present the following historical outline, keeping to the chronological order for the greater part; but also having some regard for the development of certain phases of the general problem. Special effort has been made to state the problem as each investigator saw it, to point out his salient results and interpretations, and to refrain from criticism of theory except upon points which cannot be later included in the discussion of our own results.

HISTORY

It was Urbantschitsch,[4] a specialist in aural disorders, who made the first specific study (1875) of the fluctuation of minimal sensations. He had found, in testing the hearing of his patients by means of the ticking of a watch, that the sound would come and go without a change in the distance of the watch from the ear. This phenomenon was not purely physical because when he asked several individuals to report simultaneously the appearance and disappearance of the sound their reports did not coincide. Similar results were obtained with a faint stream of water and with an electrically driven tuning fork. When he examined the ear to find a possible source of the phenomenon there he discovered that patients who had perforated ear-drums and also one patient whose stirrup bone was disconnected had the same experience. The fluctuations could even be perceived when the sound vibrations were transmitted to the inner ear by bone-conduction. He drew the conclusion, without making any specific tests of it, that the basis for the fluctuations is in the "perceptive ability" of the auditory nerve.

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[4] V. Urbantschitsch, Ueber eine Eigenthümlichkeit der Schallempfindungen geringster Intensität, *Centralbl. f. d. med. Wiss.*, 1875, 625–628.

Nicolai Lange[5] (1888) brought the phenomenon into psychological prominence with his experiments on sensory attention, in Wundt's laboratory. He dismissed the fatigue theory of Urbantschitsch with the arguments (1) that there is no evidence that sensory nerves tire so quickly from minimal stimuli, (2) that the nerve would have to recover while being stimulated, and (3) that the effect ought to be noticed with intense stimuli. He then sought to show that the fluctuations have a central origin, in the periodic imaginal reinforcement of the sensation. Using as stimuli the Masson disc, the ticking of a watch, and electrical contacts on the skin, he found slightly different periods of fluctuation, measured from one point of maximal intensity to the next, in the three sense departments. When he asked the *O*s to report upon the fluctuations of the gray ring and the sound presented together, the two did not fluctuate independently at their own rate but formed a new rhythm, gray alternating with sound at definite intervals. This he offers as an argument for a central theory along with the fact that images of the three sensations fluctuate in rates which correlate exactly with those of the original sensations. Lange concluded that these phenomena are fluctuations of active apperception which produces a periodic assimilation of image with sensation.

Münsterberg[6] (1889) followed with a critical and experimental study. At the outset he objects (1) that Lange's *O*s could not report accurately the point of maximal intensity; (2) that Lange's results were too regular and fell too nicely into his theory; (3) that the combined visual and auditory fluctuation is an entirely different phenomenon; (4) that Lange's theory places the source of the fluctuations in the realm of the transcendental. Münsterberg set himself the following problems: to determine whether fluctuation is "a matter of the activity of consciousness and not a matter of changes in conscious content;" and "in how far is it a central matter, and in how far the process is peripherally conditioned."[7] Using 1 *O* besides himself, and the Masson disk as stimulus, he obtained the following results. If the eyes move in order to keep fixated upon a stimulus which is moved, either quickly or slowly, the periods of fluctuation as measured from one moment of disappearance to the next—are noticeably longer, or in some cases the stimulus fails to disappear. Voluntary winks every one or even two seconds prevent a disappearance, although a similar cutting off of the stimulus by placing a gray card over it every one or two seconds even shortens the fluctuation periods. He also found that short periods occurred along with rapid breathing and long periods with slow breathing and that often a disappearance coincided with an inspiration. He concluded that the conditons of fluctuation lie chiefly in the muscles of accommodation and fixation. His theory is that, as the muscles of accommodation become fatigued and relax, dispersion of the contours of the gray ring results and the ring merges into the background. The change in conscious content is followed by a new effort to accommodate and the ring reappears. In the case of fixation, as fatigue sets in, the muscles shift the fixation so that the ring is brought upon a more sensitive part of the retina outside the yellow spot where the gray gains relatively more in brightness than does the white background and hence disappears. Breathing, through widespread muscular contraction and nervous connections, exerts an influence upon accommodation.

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[5] N. Lange, Beiträge zur Theorie der Aufmerksamkeit und der activen Apperception, *Phil. Stud.*, 4, 1888, 390–422.

[6] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, 2, 1889, 69–124.

[7] *Ibid.*, 81.

Pace[8] (1893), using the Masson disc, sought to test the results of Lange and Münsterberg. As a test of the 'accommodation' theory he treated one eye with atropin, thus paralysing the muscles of accommodation, and found fluctuations for that eye differing but little from those for the normal eye. He also pointed out that the fluctuations bear some relation to the limen; that as the experimental series progresses the limen is lowered because some of the fainter rings then appear. As the stimulus value rises above the limen during a series, the periods of visibility become longer and the periods of invisibility become shorter and the total period longer. He concluded that eye movements—as in Münsterberg's experiments—may change the fluctuation time by lowering the limen. He suggests that the fluctuations depend upon a reciprocal action of center and periphery.

Eckener[9] (1893), using as stimuli a watch, a Wagner hammer, and a stream of fine sand, attempted to determine whether the afferent nerves carry a constant impulse to the center so long as the physical stimulus is constant. He found two kinds of fluctuations: (1) those of an objective type that are brief, sudden, and complete; and (2) those of a subjective type that are longer, more gradual, and accompanied by an inner effort to bring back the perception. The former he attributes to a failure of the sense organ; the latter to a central cause. He also found that when the sound stimulus was cut off after the *O* had reported a disappearance, the latter was immediately aware of a change in conscious content. He takes this to mean that the afferent nerve is conducting a constant impulse to the center and that failure to perceive therefore has a central origin.

Lehmann[10] (1894) studied the relation between fluctuations and the breathing rhythms. Against Münsterberg he argues that the skin exhibits no muscles of accommodation and the ear shows no fatigue. Using as stimuli a white square on a black background, the hiss from a Bunsen burner and copper electrodes on the skin, he found that only the periods for tactual fluctuations agree in length with the breathing period. Analysis of the other fluctuations, however, showed that most reappearances come a short time after the beginning of expiration and many also occur during an inspiration. He concludes that the most unfavorable times for reappearance are (1) the respiration pause, when the blood pressure is least in the brain, and (2) the maximal inspiration, when neural energy is drained into channels innervating the muscles of breathing. Correlation with tactual fluctuations is best because there are no muscles of accommodation to mask the effect of breathing. The minimal sensation may therefore disappear from fatigue or loss of accommodation, and its return is dependent upon the intensity of the stimulus and upon favorable moments during the breathing cycle.

Marbe[11] (1895), working independently of the Leipzig laboratory, attacked the problem from a slightly different angle. Against Lange's theory he offers the argument that a proof of central origin does not mean that the fluctuations are phenomena of attention, nor does fluctuation of sensation and image in the same periods mean that the one is the cause of the other; they may be traced to a common cause. He maintains that if Münsterberg's theory is true, the stimulus would have to be so weak as to be perceptible only when accommodation and fixation are good, and that only a dark stimulus on a light ground would fluctuate. He disproves Münsterberg's first requirement by showing that the gray ring may be varied within relatively wide limits and still give fluctuations. The second requirement he finds not to be true, for a light spot on a dark ground will fluctuate. In varying the gray ring from a point where it was perceptible none of the time up to the point where it was visible all of the time—during a one-minute observation—the following facts came to light: (1) the ring appears first in the periphery and then in direct vision; (2) the periods of visibility lengthen until they equal the periods of invisibility; (3) fluctuations cease last in the periphery as the intermissions become shorter; and (4) fluctuations occur more rapidly near the limen. He draws the conclusion that these fluctuations have the same conditions as those liminal sensations.

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[8] E. A. Pace, Zur Frage der Schwankungen der Aufmerksamkeit nach Versuchen mit Masson'schen Scheibe, *Phil. Stud.*, 8, 1893, 388-401.

[9] H. Eckener, Untersuchungen über die Schwankungen der Auffassung minimal Sinnesreiz, *Phil. Stud.*, 8, 1893, 343-365.

[10] A. Lehmann, Über die Beziehung zwischen Athmung und Aufmerksamkeit, *Phil. Stud.*, 9, 1894, 66-91.

[11] K. Marbe, Schwankungen der Gesichtempfindungen, *Phil. Stud.*, 10, 1895, 620 ff.

The controversy regarding central as against peripheral conditions was carried across the Atlantic when Hylan[12] (1896) undertook a study of fluctuations in the cutaneous field. He used stimuli which gave cold and pressure. When cold and pressure were given together he found that the two could fluctuate independently—a result contrary to that of Lange. As in the experiments of Marbe, he found that an increase in the stimulus produced changes in the periods of fluctuation. As the temperature was lowered the pauses (absence of cold) became shorter; but as the pressure was increased the pauses became longer.

There occurred at this point a many-sided debate over the special question as to whether fluctuation of sound is a fact. It began with Heinrich[13] (1898), who found no fluctuations when pure tones, from a Galton whistle, organ pipes or glass tubes, were used. Cook [14] (1900), in the Cornell laboratory, tested the question with tones from a Politzer acoumeter, a tuning fork, and a Stern variator and found fluctuations with four observers. After further objection from Heinrich that the tones were not pure, Titchener[15] (1901) reported that no fluctuations were found with a gas harmonica and 2 *Os*. Dunlap[16] (1904) tested Heinrich's results by means of tones from a singing flame and diapason tones and found distinct fluctuations with 5 *Os*. He came to a conclusion opposite to that of Heinrich: namely, to the conclusion that fluctuations are favored by pure and simple tones and that complex tones fluctuate because we select out one dominant. Hammer[17] (1905) came in with the suggestion that fluctuating sounds are physical phenomena. Most investigators had used watches or similar instruments in which there is adequate opportunity for periodic changes in physical intensity of sound and his own observations—apparently with no other aids than the human ear—show such changes. Using a ticking electromagnet and 5 *Os* he found no fluctuations. Seashore[18] (1905) in a critique of Hammer reports that with a Runne-chronometer and 55 unpractised *Os* he found fluctuations in every case. The discussion ended about where it began, with Heinrich[19] (1907) and with a restatement of his position: that pure tones do not fluctuate whereas noises do, and that some *Os* report a fluctuation of the noise component independent of the pure tone, which remains constant. He submits the evidence from physiology that the ear-drum had been found to make adjustment for noises but not for pure tone. But even for noises the adjustments are noted only when there is a special effort made to hear. Several investigators had reported fluctuations of sounds during this period without entering into the controversy. Among them were Bonser[20] (1903), Wiersma[21] (1901) and Seashore and Kent[22] (1905).

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[12] J. B. Hylan, Fluctuations of the attention, *Psychol. Rev.*, 3, 1896, 56 ff.

[13] W. Heinrich, De la constance de perception des tons purs à la limite d'audibilité, *Bull. internat. l'Acad. des. sci. de Cracovie*, 1900, 37 ff.

[14] H. O. Cook, Fluctuation of the attention to musical tones, this JOURNAL, 11, 1900, 119 ff.

[15] E. B. Titchener, Fluctuations of the attention to musical tones, this JOURNAL, 12, 1901, 595.

[16] K. Dunlap, Some peculiarities of fluctuating and of inaudible sounds, *Psychol. Rev.*, 11, 1904, 308–317.

[17] B. Hammer, Zur experimentellen Kritik der Theorie der Aufmerksamkeits-schwankungen, *Zsch. f. Psychol.* 37, 1905, 363–376.

[18] C. E. Seashore, Die Aufmerksamkeitsschwankungen, *Zsch. f. Psychol.*, 39, 1905, 448 ff.

[19] W. Heinrich, Über die Intensitätsänderungen schwacher Geräusche, *Zsch. f. Sinnesphysiol.*, 41, 1907, 57 f.

Another paper, important for the controversy, but apprently unnoticed by those who were engaged in it, was presented by Jackson[23] (1906). He used a telephone in which the current was interrupted by means of a König fork of 50 v.d. In order to test the constancy of physical energy in the tone, the amplitude of the vibrations was measured by means of a beam of light reflected from the diaphragm of the telephone and seen magnified in a telescope with a micrometer eye-piece. No change in amplitude which could account for the auditory fluctuations in 6 *O*s could be found. The author states, however, that fluctuations were hard to get; requiring from 5 to 6 days' practice with some *O*s, because the range of intensities which gives fluctuations of sound is extremely limited. *May this not be the reason for the failure of some investigators to get auditory fluctuations at all?* Possibly the proper intensity was not found.

A similar controversy arose over the fluctuations of touch. Wiersma[24] (1901) found that pressures from areal stimuli on the back of the hand will fluctuate, and that, furthermore, these fluctuations obey the same laws as those in vision and audition. Marbe had shown that fluctuations occur over a relatively wide range of stimulus intensity in vision. Wiersma (with Heymans as observer) showed that the same holds for pressures. Ferree[25] (1906) found no fluctuations of tactual sensations either from cork wafers on the arm or from electrical stimulation on the tongue. Geissler[26] (1907), using weights similar to those of Wiersma, found no fluctuations with 4 *O*s, but with 2 other *O*s fluctuations occurred at first. Using a new set of weights much lighter than Wiersma's, the liminal areal pressure would disappear after 2–3 min. and not return, or continue as a vague, indefinite, and unpleasant irritation.

Another phase of the history took place under Pillsbury's direction in the Michigan laboratory. This is the phase which has to do with the correlation of fluctuations with the various bodily rhythms. These investigations began with the assumption that the weight of the evidence up to that time favors a central theory. Slaughter[27] (1900) began the investigations, expressing dissatisfaction with Lehmann's too "general and indefinite" theory and with the inadequate physiological theories of attention and apperception. Using the Masson disc and recording fluctuations over a period of several minutes, he found 3 rhythms in the 'attention wave.' Besides the coming and going of the gray ring as previously reported, he noted a secondary rhythm 10–15 sec. in length and a third rhythm of

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[20] F. G. Bonser, A study of the relations between mental activity and the circulation of the blood, *Psychol. Rev.*, 10, 1903, 133 ff.

[21] E. Wiersma, Untersuchungen über die sogenannten Aufmerksamkeitsschwankungen, *Zsch. f. Psychol.*, 26, 1901, 168–200.

[22] C. E. Seashore and G. H. Kent, Periodicity and progressive change in continuous mental work, *Psychol. Monog.* 6, 1905, no. 28, 47 ff.

[23] G. L. Jackson, The telephone and attention waves, *J. Phil., Psychol., etc.*, 3, 1906, 602–604.

[24] *Op. cit.*, 187.

[25] C. E. Ferree, An experimental examination of the phenomena usually attributed to fluctuation of attention, this JOURNAL, 17, 1906, 81–120.

[26] L. R. Geissler, Fluctuations of attention to cutaneous stimuli, this JOURNAL, 18, 1907, 309.

[27] J. W. Slaughter, The fluctuations of the attention in some of their psychological relations, this JOURNAL, 12, 1901, 313–332.

60–80 sec. These rhythms show themselves as an increase and decrease in the rapidity of fluctuation and in the ratio of visibility to invisibility. Rather general correlations with bodily rhythms are shown in the following facts. (1) Pressure exerted by the *O* upon a dynamograph fell during the invisibility period and rose during the visibility period. (2) The periods of fluctuation shortened during exertion and the visibility periods became relatively longer. (3) One *O* showed very short, regular fluctuations and also rapid breathing rate. (4) Another *O* showed abnormally long periods and also long Traube-Hering waves. More direct correlations were found in records where there was an increase of blood pressure during visibility. Two *O*s showed 83% and 91% correspondence of this sort. Slaughter concludes that the fluctuations may be correlated with 3 bodily rhythms: breathing; Traube-Hering waves; and other longer, still unknown, waves. His theory traces the correlation to nervous connections, not to increased blood-supply in the brain, because the blood-pressure in the brain stands in inverse relation to that in the limbs, and because the breathing and pulse rhythms appear in the record for blood-pressure. The nervous connection is made, he assumes, when the motor discharges for breathing or changes in blood-pressure reinforce the sensory centers. In the same way the motor discharges during voluntary exertion facilitate the sensory centers as shown in the dynamographic and fluctuational records.

Taylor[28] (1900) attempted to study the effect of secondary stimuli upon the fluctuations. In some cases the 'attention wave' was lengthened and in others it was shortened so that no general conclusions could be drawn. Bonser[29] (1903), working independently of the Michigan group, found a confirming correlation with the Traube-Hering waves. He states in conclusion that "the minimal sound occurs soon after the period of vaso-constriction; it increases until the maximum is reached just before the beginning of vasodilation and is lost near the middle of the curve's ascent." Pillsbury[30] (1903), in seeking a measure for fatigue, secured fluctuation-records at three periods during the day. Results show characteristic changes in length of fluctuation and the ratio of visibility to invisibility for the individual *O* throughout the day. Galloway[31] (1904) found that secondary stimuli that affect the fluctuations affect the Traube-Hering waves in a similar manner and that the Traube-Hering waves show the same diurnal variations as Pillsbury had found for the same *O*s.

This chapter also finds its close in the laboratory where it started. After standing for two decades it was taken up again in 1924 by Griffits and Gordon.[32] Preliminary experiments show a slight tendency for the appearance of a memory-image or a liminal light to appear during the trough or upward slope of the Traube-Hering wave. But strange to say the authors found the same tendency when the stimulus itself was made to fluctuate. No correlation was made between the length of the 'attention wave' and that of the Traube-Hering wave either in individual differences or with different stimuli. One is forced to the conclusion that if there is any correlation at all between the two waves, the question as to which is the 'cause' of the other is still open.

The fluctuations in vision, which no one disputes, have retained a more unbroken interest up to our own time. A number of theories have been advanced and have been tested along with the 'accommodation' theory of

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[28] R. W. Taylor, The effect of certain stimuli upon the attention wave, this JOURNAL, 12, 1901, 213 ff.

[29] Bonser, *op. cit.*, 120 ff.

[30] W. B. Pillsbury, Attention waves as a means of measuring fatigue, this JOURNAL, 14, 1903, 277 ff.

[31] C. E. Galloway, The effect of stimuli upon the length of the Traube-Hering waves, this JOURNAL, 15, 1904, 512 ff.

[32] C. H. Griffits and E. I. Gordon, The relation between the Traube-Hering and attention rhythms, *J. Exper. Psychol.*, 7, 1924, 117 ff.

Münsterberg. Slaughter tested this theory by using an *O* who had had a crystalline lens removed and found fluctuations nearly normal in period.[33] Ferree[34] (1906) found fluctuations with two aphakial *O*s. McDougall[35] (1903) has shown in a study of retinal rivalry and of ambiguous figures and of negative after-images, however, that attempts to accommodate or to converge the eyes result in a reinforcement of the sensation for the eye, either under normal conditions or even when it has been atropinized. Heinrich and Chwistek[36] (1907) have shown from the observation of changes in the crystalline lens, made with an ophthalmometer during fluctuations of a small black dot on white, that disappearance of the dot coincides with a change in the lens. These changes are not fatigue effects because they occur from the very first moments of fixation. A point placed beyond the range of accommodation, with myopic *O*s still gives fluctuations. They attribute the disappearance of the dot, which may be as large as .2 cm. in diam.—the larger it is the longer is it visible during fluctuations—to a loss of *Sehschärfe*. In reply to this contention, Ferree[37] (1913) objects (1) that the fluctuation of a point stimulus is a special case never before considered under the subject of fluctuation of attention; (2) that their method of observation of changes in the lens was unreliable, chiefly because eye-movements may produce the same changes in the reflected images seen in the ophthalmometer; (3) that all the facts which they present require no 'accommodation' theory but can be equally well explained on the basis of a theory based upon adaptation and eye-movement; and (4) that it is difficult to see how changes in accommodation can be effective beyond the range of accommodation. He shows by the use of four aphakial *O*s that there are fluctuations of a point stimulus without changes in accommodation and that these fluctuations obey the same laws which he found to hold for stimuli of larger areas in an earlier study, a study which we shall presently review. We may conclude, in the light of the evidence on both sides, that accommodation has its effects upon visual fluctuations but that there would be fluctuations independent of these effects.

Pace[38] (1902) is the first to offer, in a second paper on the subject, a new peripheral theory. He suggests that retinal adaptation has something to do with disappearance of a visual sensation. Adaptation was studied as usual through its after-effect, the negative after-image. He found that if the stimulus is cut off at the moment of disappearance a distinct negative image appears; but if the stimulus is not cut off until the moment of reappearance, the after-image is weak if present at all. His theory is as follows. (1) The light disappears by fatigue of the retina; (2) attention is aroused by this change in mental content; (3) this causes accommodation to lapse; (4) the retina has a chance to recover; and (5) the light reappears as accommodation is restored. Statement (3) is based upon the fact offered by Heinrich that when attention is distracted from the visual object accommodation lapses.[39]

Hammer[40] (1905) adopts adaptation as a means of disappearance, but wishes to substitute shifts in fixation to account for reappearance. He maintains that accommodation alone cannot be responsible for the recovery from adaptation because fluctuations occur and must have the same effect. Using adjacent rectangles of gray, giving five different steps or degrees, he finds two indications of adaptation: (1) that during the course of the experiment the visibility time decreases; (2) that a negative after-image can be observed especially with the greater stimulus differences. The only evidence adduced to show the effects of re-fixation is that voluntary shifts in fixation will bring back the stimulus difference.

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[33] Slaughter, *op. cit.*, 331.

[34] Ferree, *op. cit.*, 94.

[35] W. McDougall, Physiological factors of the attention process, *Mind*, 12, 1903, 473 ff.

[36] W. Heinrich and L. Chwistek, Über das periodische Verschwinden kleiner Punkte, *Zsch. f. Sinnesphysiol.*, 41, 1907, 59-72.

[37] C. E. Ferree, Fluctuation of liminal visual stimuli of point area, this JOURNAL, 24, 1913, 378–409.

[38] E. A. Pace, Fluctuations of attention and after-images, *Phil. Stud.*, 20, 1902, 232–245.

[39] W. Heinrich, Die Aufmerksamkeit und die Funktion der Sinnesorgane, *Zsch. f. Psychol.*, 9, 1896, 362 ff.

[40] Hammer, *op. cit.*, 363 ff.

During 1906 Ferree[41] made two studies in the Cornell laboratory intended to show that visual fluctuations are a phenomenon of retinal adaptation rendered intermittent by eye-movements. The evidence comes from various sources. (1) A stimulus which is physiologically constant—produced by electrical stimulation of the retina—does not give fluctuations. That the stimulus was liminal, however, is doubtful for it took 30 sec. for complete adaptation. (2) The same conditions of stimulation have the same effect upon adaptation as upon fluctuation. Square stimuli within the range of 2 x 2 cm. to 22 x 22 cm. will fluctuate; within this range of areas adaptation also was intermittent. Hues which require a longer time for complete adaptation also show a longer visibility-time during fluctuations. Certain combinations of stimuli and backgrounds which require longer adaptation-time also remain visible more of the time during fluctuations. (3) Eye movements break up adaptation because most *O*s show an excess of eye-movements in the horizontal direction as detected by the movement of a projected after-image and for these *O*s vertical strips fluctuate more than do horizontal. In a second study[42] (1908), Ferree proceeds on the assumption that the fluctuation of negative after-images is the same kind of phenomenon as the fluctuation of sensations. That may be admitted if we overlook the continuous action of a physical stimulus in the case of sensations. He presents the addition to the 'eye-movement' theory that the physiological stimulus is rendered intermittent by streaming phenomena on the retina which are facilitated by eye-movements. The evidence for this theory comes from his study of fluctuating after-images and lies in the following facts. (1) If the stimulus is too small no fluctuations occur; if too large it fluctuates in parts. (2) Good fixation decreases fluctuation; poor fixation facilitates it. (3) Narrow strips fluctuate more than do squares of the same area; they also favor poor fixation. (4) Vertical strips fluctuate more than horizontal; most eye-movements are in the horizontal direction. (5) Voluntary eye-movements (12 cm. to the right every 3 sec.) are followed by a disappearance. (6) Streaming phenomena were observed and also the fact that they are accelerated by eye-movements. (7) In a still later report the streaming phenomena were observed to wipe out parts or all of a negative after-image in the form of a vertical strip.[43] Ferree states his theory as follows.[44] "(1) The intermittence of minimal visual sensation is a phenomenon of adaptation. (2) Adaptation is rendered intermittent chiefly through the influence of eye-movement. (3) Eye-movement interferes with adaptation in three ways: (a) it decreases the total time of stimulation; (b) it gives opportunity for restoration; and (c) more immediately, it determines or influences the washing or streaming over the retina of some material capable of directly affecting the visual processes."

This theory was apparently undisputed until Liddell[45] (1919) in the Michigan laboratory undertook to photograph the eye during fluctuations in order to get simultaneous records. Among 7 *O*s he found two distinct types. (1) Those with frequent eye-movements during fluctuations, either evenly distributed over the periods of visibility and of invisibility or found mainly during the periods of invisibility. (2) No eye-movements, or very few and at random. In the first group, rarely did an eye-movement coincide with a reappearance or disappearance, and more than one movement could occur without producing an immediate reappearance or a disappearance, and this in spite of the fact that the *Os* were convinced that eye-movements, especially the 'searching movements,' brought back the spot of light. The author concludes that "fluctuations of attention are not causally dependent upon eye-movements."

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[41] C. E. Ferree, *op. cit.*, 17, 1906, 81–117.

[42] C. E. Ferree, The intermittence of mimimal visual sensations, this JOURNAL, 19, 1908, 58–130.

[43] C. E. Ferree, The streaming phenomenon, this JOURNAL, 19, 1908, 484 ff.

[44] C. E. Ferree, *op. cit.*, 19, 1908, 129.

[45] H. S. Liddell, Eye-movement during the fluctuation of attention, this JOURNAL, 30, 1919, 241–252.

The possibility of a correlation between changes in size of the pupil was not considered by anyone until Troland[46] (1921) suggested it in a study of adaptation. He found that there were rhythmical variations in size amounting to 2 mm. in diam. and that with stimuli of "moderate intensity" a disappearance was very often immediately preceded by a contraction of the iris. With an artificial pupil there were almost no disappearances of supraliminal lights. The relation of pupillary changes to fluctuations with a liminal stimulus, however, will still have to be determined.

We offer no more general conclusions than the few we have ventured so far. One general criticism might be made. It has been the tendency in most cases to look for one and only one condition of the fluctuations. Such a result would of course be a simple solution. But the whole problem on the physiological side appeals to us as very complicated. The problem, let us also say, appears double, or even triple, and this has not always been recognized. There is a psychological question, which is a systematic one; and there is a physiological problem, which lies, for psychology, in the realms of theory. The systematic question is whether the fluctuations are fundamentally a matter of change in clearness or vividness. It involves the whole matter of the relation of intensity to clearness. The fluctuations may be phenomena of attention regardless of where the physiological conditions may be found; the final test is one of psychological observation. The physiological problem is to ferret out in the organism the necessary and sufficient conditions for the experiential facts. The correlation of the physical and physiological conditions with the various phases of the phenomena of fluctuation constitutes the psychophysical problem. We shall begin our study with this last problem, testing for some of the peripheral and central conditions and for the dependence of fluctuation upon stimulus conditions, and then use as much phenomenological description as we have toward a solution of the systematic problem.

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[46] L. T. Troland, The colors produced by equilibrium photopic adaptation, *J. Exper. Psychol.*, 4, 1921, 362–390. Professor Troland's paper was found after this study was completed, hence the relation between pupillary changes and fluctuations was not examined.

II. Fluctuations Regarded as a Function of Stimulus-Intensity

One striking thing in the history of the problem is the dependence—first pointed out by Marbe—of the fluctuations upon the intensity of the stimulus. It is also noteworthy, and almost inconceivable, that the phenomenon has been studied without knowledge of the limens of the *O*s and the relation of the stimuli to the limen. Nowhere do we find a specific liminal value given and even Marbe assumed that the stimulus-value which gave most rapid fluctuations was the liminal stimulus without finding that value by any of the customary means of determining limens. We therefore proceeded first of all to find the liminal stimuli for our *O*s.

Observers. The *O*s who took part in the preliminary experiments were: Dr. L. B. Hoisington (Ho), assistant professor of psychology; Dr. C. A. Dickinson (Di), now professor of psychology, University of Maine; Mr. George Kreezer (K), assistant in psychology, and Mr. M. K. Walsh (W), fellow in psychology. From these *O*s we have results which bear chiefly on the phenomenal aspect of the problem.

The *O*s who served in the main experiements—one hour at the same time of day, six days a week, throughout the summer of 1926— were: Dr. K. M. Dallenbach (D); Dr. Harry Helson (H), now assistant professor of psychology, University of Kansas; Mr. D. G. Griffin (G), assistant in psychology; and Mrs. Florence Hazard (Ha), scholar in psychology.

Determination of the limens. Since we were to work under progressive dark adaptation the liminal value had to be determined for every 5 min. period during the observational hour. We found, however, that if *O* did nothing for the first 10 min. in the dark room, the limen was fairly constant from that time on, especially after 15 min., as is generally known.[47] *O* always went into the dark room after being about the laboratory for some minutes. For about 3 min. after entering the dark room he was subjected to the indirect, diffuse illumination from two electric lights; then the room was entirely darkened. The starting point for dark adaptation was thus about the same from day to day.

The stimulus used here, and in the experiments on fluctuation which follow, was a circular white patch of light 42 mm. in diam. and 170 cm. distant from the eye of the *O*. It was presented in parafoveal vision, since *O* fixated a red dot 2.5 mm. in diam. about 40 mm. above it. The stimulus-light was provided by the Nagel adaptometer[48] in which it is possible to vary the light intensity one part in eight million and to read the stimulus value from a scale. The light source in the adaptometer, three 50 w. lamps, was on alternating current the voltage of which was measured at every hour and found to vary not more than 1 v. in either direction from a normal of 112 v. in any experimental hour, and not more than 3 v. from day to day. The fixation light was furnished by a 2.5 v. flashlight bulb behind a red glass filter and a ground glass, and reflected in a mirror so as to appear to *O* to be just above the stimulus light. The current for the fixation light was about 1 v. and was kept constant by means of a rheostat.

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[47] There is one remarkable exception in the case of Ha. Her sensitivity increased at the same rate up to 40 min. of dark adaptation. This indicates a visual peculiarity which the same observer exhibited throughout the experiments on fluctuations.

[48] For a description of the adaptometer, see W. Lohmann, *Zsch. f. Sinnesphysiol.*, 41, 1907, 304 ff.

Tests with a sensitive galvanometer show that changes in the current never exceeded a half milliampere during an observation. It was found important to keep the fixation light constant because of Heymans' law of inhibition; a slight change in intensity would cause the stimulus-light to appear or disappear. The fixation point was regarded as necessary to keep the factor of local adaptation under control, also the factor of greater peripheral retinal sensitivity. Red was used because under dark adaptation it has the greatest attraction for the fovea.[49]

The method of securing a liminal value was a combination of the method of limits and the method of average error, a method used by Nagel.[50] The stimulus was shielded from one eye of *O*, but the fixation light was exposed to both. The limens were found for each eye separately, because the later work was to involve monocular vision.

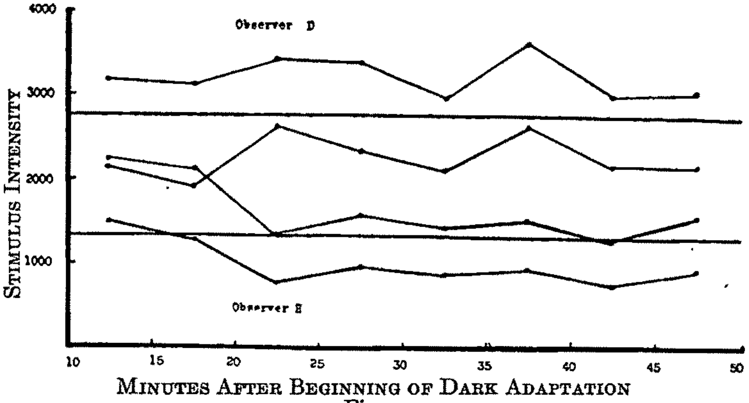

Fig. 1

Three days were given to each eye after two days of practice. The *O*, with his head held in one position by means of a head-rest, varied the stimulus, increasing its intensity until it became visible. He then stopped and reported it 'in.' He then turned a little beyond this position; then reversed the direction, decreasing the intensity until it disappeared, when he would report it 'out,' and so on. *E* took the readings from the adaptometer scale. The readings were begun 11 min. after the beginning of dark adaptation, continued for 3 min., then 2 minutes' rest, and so on throughout the hour. About 8 readings, both 'in' and 'out,' could be made in every 5-min. period with 2-min. rests between.

The curves (see Figs. 1 and 2) show the limits, 'out' and 'in,' for every 5-min. period and the average liminal value for the hour during six days. From them we selected the stimuli to be used in the experiments on fluctuation, using the average liminal value for the whole hour, also the average values of the upper and lower limits as representative stimuli above and below the limen. For 2 *Os* (D and Ha) the limens for the two eyes were almost identical. For the other 2 *O*s (H and G) there was an ocular difference in one case almost equal to the mean variation for either eye. For that reason we chose 4 stimuli for these two *O*s, but later work showed that the differences were probably due to chance and so we present a single liminal value for them also.

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[49] For a discussion of fixation in the dark, see Tigerstedt's *Handbuch der physiologischen Methodik*, III, 2 Abth., 1914, 17–25.

[50] For Nagel's discussion, see Tigerstedt's *Handbuch*, 21 f.

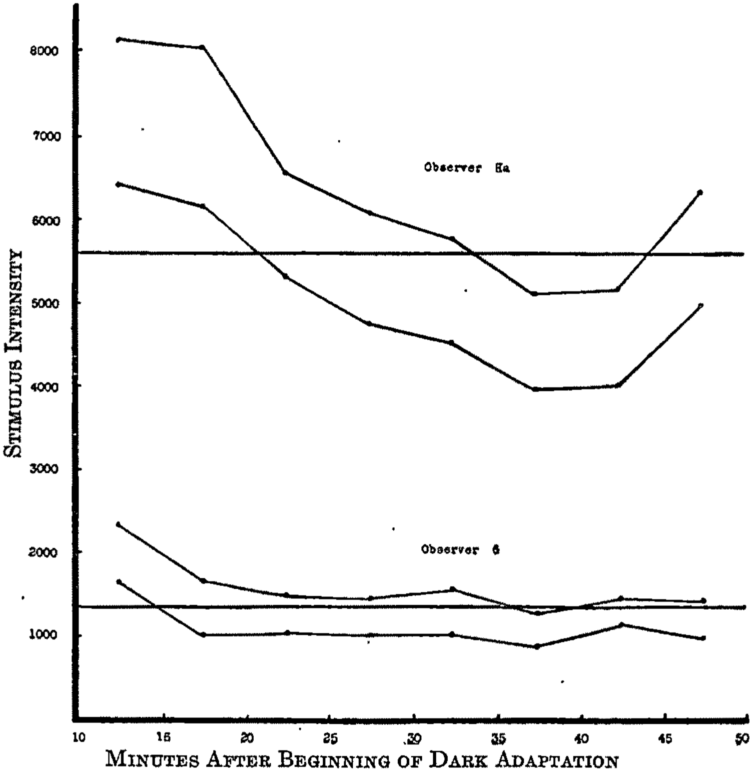

Fig. 2

Whether the limens remained the same throughout the 6 weeks of experiments we do not know. It is reasonable to assume that if improvement in sensitivity or in ability to observe did occur, it took place more rapidly during the first 8 days when we were establishing the limens. We did test for the limen of one *O* (D) later in the term and found that his limen was unchanged.[51]

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[51] It is interesting to compare these results with those of Seashore and Kent (*op. cit.*), which they secured in a similar manner with continuous observations lasting 2 hrs. The 'hour' and 'minute' waves which they found may be found also in our data, as a rising and falling of the limen, apart from the initial drop due to dark adaptation. The waves are masked in the curves presented because the latter represent averages for different days. There are, however, other than periodic factors producing a rise and fall in sensitivity; *e.g.* the tendency for the field of vision about the fixation-point and the stimulus to fill up with mists, hazes, and colors. This condition is always accompanied by a higher limen.

Fluctuations with selected stimuli. The arrangements for this part of the experiment were exactly the same as in the determination of the limens except that a biting-board was added to the head-rest to give greater stability to the head. *O* was asked to report fluctuations—by pressing a key at the moment of disappearance and lifting the key at the moment of appearance—for 3 min., and then he was allowed to rest 2 min., as before. He was encouraged to take a passive rather than an active attitude toward the stimulus light and to keep fixated upon the red point. The stimulus was set at one of the selected values and left there for the whole hour.

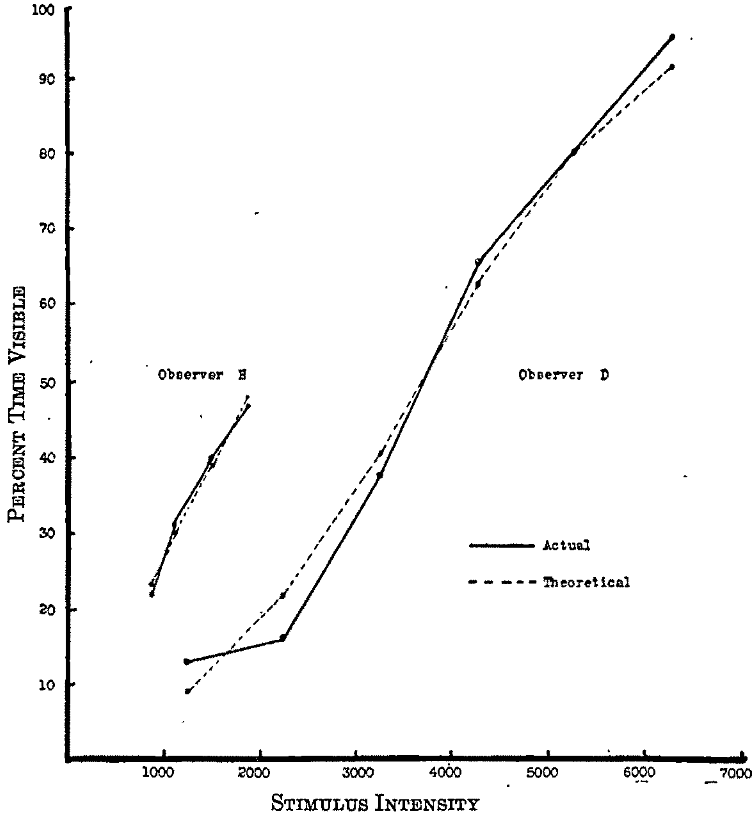

Fig. 3

Procedure. The recording apparatus was in a room next to the dark room so that *E* and *O* could be in communication at any time. During the 2-min. rest, *O* gave his verbal reports into a dictaphone while *E* prepared the apparatus for the next series. No records were accepted for later computations until after 2 days of practice in recording fluctuations. Control experiments were interposed from time to time in which *E*, by pressing a key, could cover the stimulus by means of a shutter under electromagnetic control. This served as a check on whether *O* was reporting changes immediately, and in one case it led to an interesting phenomenon which will be mentioned later. Four days were given to every one of the selected stimuli, but other kinds of series—to be described later—were alternated with the simple fluctuation series so that we may say that every stimulus value was presented at least twice, once to each eye, at every 5-min. period during the experimental hour. The kymographic records were timed in sec. by means of a Jacquet chronograph.

Results: Relation to the phi-gamma hypothesis. Our first problem is to see what fraction of the time every stimulus is visible during the 3 min. of fluctuation. The curves in Fig. 3 show the matter graphically for 2 *O*s. The curve for G coincides almost exactly with that for H, so nearly that it cannot be shown on the same diagram. This is significant because the limens for H and G are almost the same, as may be seen from Figs. 1 and 2. The case of Ha is again peculiar. All stimuli gave visibility approximately 70% of the time. Since her adaptation never reached a point of equilibrium during the hour, as is true for the other *O*s, we might not expect her fluctuations to vary in the more precise manner of the others. We found only too late that the stimuli which were chosen were too near together to be representative. In a few cases where this fact was obvious before calculation of the results, we added one or two more, above or below those chosen as the case required. In only one case were we able to extend the range nearly to the limits in both directions. This was in the case of the most practised observer (D). With the data we have, however, we shall attempt to test Marbe's theory that fluctuations and the limen are fundamentally the same phenomenon. Two questions will constitute our test. (*1*) Does the curve for percent visibility against stimulus-intensity follow the phi-gamma function?[52] (*2*) Does the 'fluctuation' limen, *i.e.* the point of 50% visibility, coincide with the limen found by regular methods?

(*1*) In answer to the first question we call attention to the curve for D which has all the appearances of the phi-gamma function. The dotted line represents the best-fitting theoretical curve. If we compute the goodness of fit of this curve to the theoretical curve, according to the method of Pearson,[53] and

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[52] E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280 ff.; also F. M. Urban, *Application of statistical methods to the problems of psychophysics*, 1908, 106 ff.

[53] K. Pearson, *Tables for statisticians and biometricians*, 1914, xxxi. This is only a rough measure of normality of distribution of the data in this case. Thomson's method (see G. H. Thomson, Criterion of goodness of fit of psychophysical data, *Biometrika*, 12, 1919, 216), which has been devised specifically for curves of this type, could not be used because we do not know from these continuous observations the number of judgments.

also of the segments of the curves for H and G, the Pearson P is equal to .266, .976, and .545 for D, H, and G, respectively.[54] All these coefficients are good and in the best case P indicates almost perfect fit.[55]

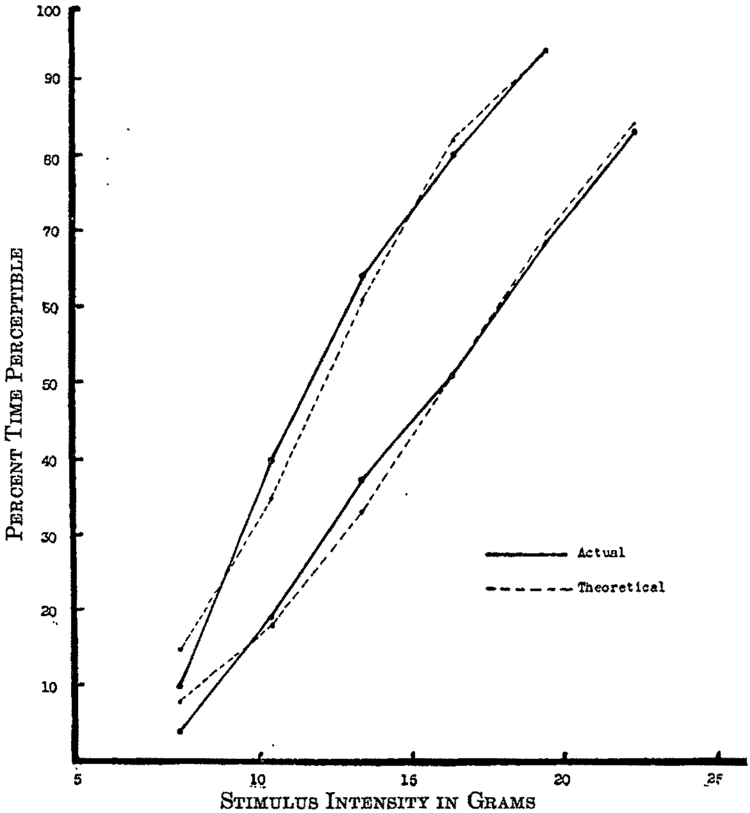

Fig. 4

If we take the liberty of using Marbe's data[56] and treating them as we have our own, we find curves for 2 *O*s which also have the familiar ogive form but which are nonsymmetrical and which fit very poorly the theoretical curves. From data presented by Wiersma[57] we also find curves which appear to follow the phi-gamma hypothesis. These data come from tactual and auditory stimulation as well as from visual. The Pearson P for the goodness of fit of the curves from visual stimuli are .03 in one case and .35 in another; for the curves from tactual stimuli, .59 and .71. The curves from auditory stimuli appear from inspection to fit even better, but since unequal stimulus-steps were used the data could not conveniently be treated by statistical methods.[58] We present the curves for Wiersma's data from tactual stimulation, with the theoretical curves, in Fig. 4. Marbe and Wiersma were dealing with the difference limen. The former, it will be remembered, was using the Masson disk and the latter differences of tint made Hering grays. We may therefore conclude that fluctuations of a liminal difference obey the same laws as fluctuations of a light at the stimulus limen.

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[54] Ha's curve is an exception. We have already seen that she is unique in her rate of dark adaptation (see Fig. 1). Control experiments show that we are dealing with a different phenomenon in her case, for when the weaker stimuli were actually cut off she continued to report fluctuations in the same rather regular manner for as long as a half-minute. It is hard to say what was being reported here, whether it was one of the patches of mist or haze, or the positive and negative phases of the after-image of the stimulus, or whether it was a memory image. If it were a memory image we might call it evidence for Lange's theory of imaginal reinforcement; but we cannot be sure that it was.

[55] Cf. E. Culler, Studies in psychometric theory (*J. Exper. Psychol.*, 9, 1926, 286), for interpretation of the Pearson P coefficient.

[56] Marbe, *op. cit.*, 631. [57] Wiersma, *op. cit.*, 180 ff.

We may predict that the phi-gamma function for fluctuations of sound will have a greater precision (h) due to the absence of sensory adaptation. This is apparently true in the work of Jackson,[59] who found that the range of intensities which give auditory fluctuations is very narrow and hard to determine. We may also venture to account for Heinrich's failure to find fluctuations for pure tones on this same basis. He states that for noises the ear makes muscular adjustments whereas for pure tones it does not. With pure tones, then, still another factor for fluctuations has been removed and the range of intensities which will give fluctuations is even more narrow and hard to find. More work should be done in which the composition of the tones is known and can be correlated with the precision of the psychometric function.

(*2*) The answer to our second question undeniably points to the fact that local adaptation plays a rôle in fluctuation. The limen for fluctuation is in every case greater than the limen found by the method of limits. Using the data obtained in finding the limens we can find the percentage of times each of the chosen stimuli was perceptible and treat the data by the method of constant stimuli.[60] It is obvious that there was also some local adaptation during the tests for the limen, because the stimulus was continually acting upon the same region of the retina. The effect of adaptation should be less than that during fluctuations, however, because the intensity of the stimulus was constantly being changed and *O* was able to shift fixation and to give the eye momentary rests between settings of the stimulus light. We have an opportunity to estimate the liminal value which is independent of local adaptation from data obtained in later experiments. In these experiments the same stimulus light was suddenly exposed to a new region of the same retina lying the same distance from the fovea. Taking the cases where *O* reacted to the light (*i.e.* reported it 'present' with his key) within one second (the average reaction time was about 1.00 ± 0.33 sec.) after exposure, as against the total number of exposures, we have the percentage of times every stimulus-value was perceptible, and from these data we can compute the limens for momentary stimuli, or the limens unaffected by local adaptation. Table I shows how the three limens compare.

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[58] If our guess is correct the fluctuation limen for sounds ought to coincide with the limen found with momentary stimuli. This would be a crucial test and will be made later.

[59] Jackson, *op. cit.*, 602.

[60] Boring, *op. cit.*, 282.

Other interesting results, which have been reported before by various investigators but which no one has ever reported or attempted to interpret collectively, have to do with the length of the visibility period, the invisibility period, and the total fluctuation-period as functions of the stimulus-intensity. The curves for D are given in Fig. 5. They show (1) the rate of decrease in the average invisibility period with increased intensity, (2) the decrease in the average visibility period with increased intensity, and (3) the average total period as a function of intensity. It appears that we have here rather simple, continuous functions. Similar curves for the period of invisibility may be found in Wiersma's article[61] for auditory and tactual stimuli as well as visual. We have made curves for the relation between the total period and the stimulus-intensity from Marbe's data[62] and present it in Fig. 6 as a verification of D's curve.[63] All three curves demonstrate the fact that fluctuations are most rapid at a mid-point near the fluctuation limen and decrease in rapidity on both sides. All curves are incomplete, however, so that it is difficult to say to what type of function they belong. They may be parabolic, but a better guess would be that they are inverted normal distribution curves. If that were true, by plotting the frequency of fluctuation, *i.e.* the number of total periods per time-unit, against varied stimulus-intensity we should get a normal probability curve. Further work will be needed to decide the matter.

TABLE I
Limens

O	Momentary exposure	Constant exposure	Fluctuation
D	2379	2751	3670
H	882	1312	1932
G	1185	1357	2150

What does it mean that the fluctuations occur most rapidly at a mid-point and more slowly as we go away from this stimulus-value? It is surely hard to explain on the basis of a rhythm of attention. If we remember that we are dealing with the limen, however, it is more easily accounted for. We have shown that the fluctuations fit nicely into the phi-gamma hypothesis. This means that for any given stimulus there are in the organism a set of factors favoring perceptibility and a set of factors against perceptibility. The same stimulus, say at the limen, is perceived at one moment because a 'chance' disposition of the factors on the whole favors it; it is not perceived at another moment because the 'chance' disposition is on the whole against it. A fluctuating light or sound would then give us a continuous picture of these 'chance' dispositions, perceptible when the balance of factors is favorable, and imperceptible when the balance of factors is unfavorable. With a stimulus at the limen the factors are balanced so that there is an equal chance of perceptibility or imperceptibility. With stimuli above or below the limen there is a preponderance of one or the other set of factors.

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[61] Wiersma, *op. cit.*, 187 ff.

[62] Marbe, *op. cit.*, 632.

[63] The curves have been smoothed, but they retain their general form.

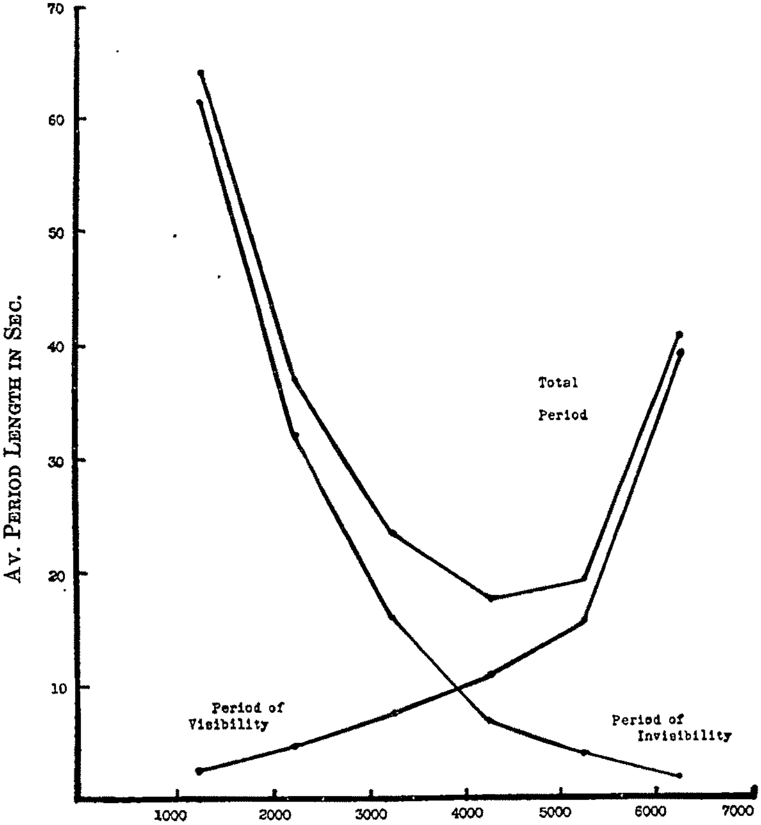
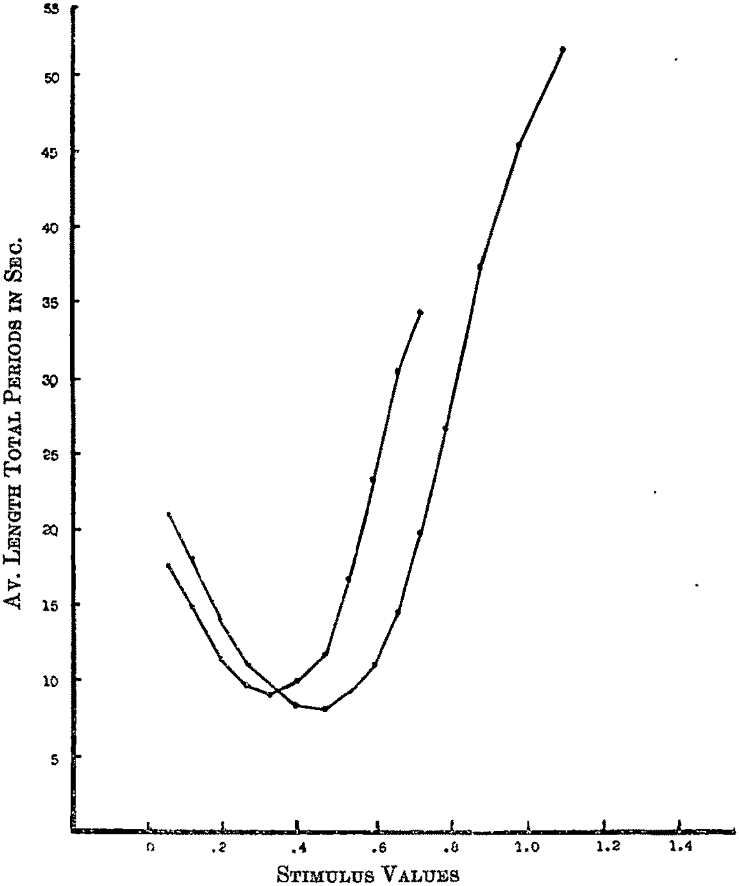


Fig. 5

It is conceivable that precisely at the limen, where the balance of factors is best (or worst, as one wishes), a slight change, a shift of but one factor on one side or the other, tips the balance in the opposite direction. Hence changes occur most rapidly at this point. As we go away from the limen on either side it takes a greater change to turn the balance in the opposite direction. Another suggested interpretation, which probably amounts to the same thing as the first, is that with subliminal stimuli the invisibility period is prolonged by mere inertia; in a similar manner for supraliminal stimuli the period of visibility is prolonged; the thing in mind tends to remain longer than its



Stimulus Values
Fig. 6

corresponding physical intensity will warrant; the thing out of mind has difficulty breaking in.

From the results thus far obtained we may draw the following conclusions. (1) Fluctuations of minimal sensations are a phenomenon of the stimulus limen. (2) The relative amount of the visibility is a direct function of the stimulus-intensity and seems to follow the phi-gamma function. (3) The average period of visibility varies directly with the stimulus-intensity. (4) The average invisibility period varies indirectly as some function of the stimulus-intensity. (5) The fluctuations are most rapid at a mid-point which is near the limen for fluctuations and become less frequent as the stimulus is made more or less intense.

Thus it appears that when we have found the factors which favor fluctuation we shall also have found the factors which control the limen. This allows, of course, for the factor of local adaptation, in the case of vision, which Marbe overlooked. Our next problem will therefore be to test for some of these factors, especially those that are still under debate.

If we view the fluctuations in the above light, the historical results do not seem so conflicting. It is not likely that the fluctuations are due to any one factor, nor that their conditions are exclusively central or peripheral. Indeed, we can find a place for all the acceptable results in such an interpretation as we have offered. Accommodation of the eye undoubtedly presents favorable and unfavorable moments for visibility. Breathing may have an occasional effect upon sensitivity, as many have shown and no one has disputed. Changes in blood-pressure are in doubt as factors at the present moment; but there is no *a priori* reason why they could not produce the occasion for heightened or lowered sensitivity. Local adaptation has been shown to be present with abundant evidence. In order to be effective after the first disappearance, however, there must be some factor which interrupts adaptation and allows for recovery. Eye-movement has been suggested as this factor, with a mass of indirect evidence for it from Ferree.[64] Although Liddell's direct study of this particular problem has led to negative results,[65] we believe that his work should be repeated, taking account of certain technical deficiencies in his work and treating the data more analytically. That is our next task.

III. FLUCTUATIONS AND EYE-MOVEMENTS

A repetition of Liddell's experiments seems necessary first of all because many verbal reports from our *O*s express the conviction that eye-movements do facilitate reappearance of the stimulus-light. The following reports are typical:

G. "There were times when I discovered that I brought the stimulus in by dropping my eyes But this occurs only in a small proportion of the cases."

D. "After the initial appearance of the stimulus and its disappearance, it remained out with but one exception in the first experiment and two exceptions in the second. In all of the three exceptions I can report eye-movements." "I felt my eyes slip from the fixation-point. I noticed during these slips that the stimulus reappeared." "I also observed that I frequently winked. Following the winks I often noticed that the stimulus came into clear vision." "I think that the upward movements are more efficacious [in bringing back the stimulus-light] than the downward movements. The movements are not symmetrically made; they are more or less dartings-out from the fixation-point and immediate returns to it." "I noticed incipient movements at the appearance and disappearance, usually at the reappearance of the stimulus."

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[64] Ferree, this JOURNAL, 17, 1906, 81 ff.; 19, 1908, 58 ff.

[65] Liddell, *op. cit.*, 252.

W. "Generally, shortly after its disappearance, I could feel my eyes give a sort of jerk, and as they gave that sort of jerk, the stimulus reappeared."

K. "At times the stimulus would disappear very rapidly and come back immediately. At such times I was aware of a flicker of the eyes as if it moved." "I would feel a sudden movement of my eye and it would suddenly reappear at full intensity."

H. "I noticed that the stimulus staid out for a long time until I would move my eyes. That would bring it back in, then it would suddenly disappear until I moved my eyes again." "Eye-movements played a considerable part. I have three criteria of eye-movement, first the feeling of the eye moving—it gives one a strain and almost a dizzy feeling under these conditions; secondly, the movement of the fixation-point; and thirdly, the sudden popping in (in very clear pattern) of the stimulus-light." "Movement of the eyes does not make it go out, it remains visible, in fact it may become very much clearer. When it is out, however, eye-movements tend to bring it in—with a jerk, suddenly, and in that objective fashion. If the eyes are kept absolutely fixed, I believe that it comes in very gradually, increasing in intensity and clearness."

Liddell also mentions that some of his *O*s were convinced that eye-movements play a rôle in fluctuations. The acceptability of these verbal reports of eye-movement depends upon whether the observation is reliable. When *O* reports an eye-movement, has one occurred or is *O* deceived? Observations which we made during the experiments on eye-movements seem to indicate that *O* scarcely ever reports an eye-movement when none has occurred. We ran a series of experiments in which *O* was to watch fluctuations as usual—with and without a fixation point—but to report with his key the moment he felt an eye-movement. A simultaneous photographic record of these reports with eye-movements (the technique to be described later) was secured.

The records show two distinct types of eye-movements; namely, tremors and re-fixations. Tremors appear in the records as rather small, sharp waves in the eye-line (see Plate I, W_1 and Plate II, D_5) and are probably tonic movements, some of them obviously made during a pulse-beat and others at random, perhaps a concomitant of rigid fixation or attempts to fixate.[66] Re-fixations appear as sudden breaks in the line (see

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[66] Professor Dodge has attributed these tremors to head-movements, chiefly due to the pulse (cf. R. Dodge, An experimental study of visual fixation, *Psychol. Monog.*, 8, 1907, no. 35), and to movements of convergence. It seems that these movements were genuine eye-movements in our cases, for, as we shall see, our *Os*' reports of eye-movements often coincide with them. That they are tonic adjustments of the eyes is suggested by the fact that they often occur immediately after a return of the stimulus-light or its sudden exposure, as may be seen in Plate I H_3 and Plate II D_5.

Plate II, H_1) and are usually greater in extent than the tremors. Table II gives numerical results showing what percent of each of the two types of movement *O* is able to report and, what is more important here, the percent of his reports which were actually preceded immediately by eye-movements. The first row, for every *O*, refers to the eye-movements reported when there was a fixation point, as in the previous experiments on fluctuation, and the second row is for those movements without a fixation-point except for the stimulus itself—a white light 1 cm. sq.—when visible.

TABLE II

Observers' report of eye-movement[67]

O	Tremors			Re-fixations			Reports		
	No.	% reported	P.E.	No.	% reported	P.E.	No.	% with movement	P.E.
D	69	52.2	4.1	117	1.7	0.9	40	95.0	2.4
	14	42.9	13.2	79	12.6	2.6	16	72.7	7.5
H	76	34.2	3.7	55	1.8	1.3	28	96.4	2.5
	71	54.9	4.0	50	4.0	1.9	41	100.0	——
G	29	65.5	6.0	69	18.8	3.2	41	71.8	4.7
	90	40.0	3.5	116	12.7	2.2	55	92.7	2.4

It is plain from the table that tremors are more easily detected than re-fixations. On the average, *O* can report half of the tremors which appear in our photographic records, whereas perhaps only 1/10 of the re-fixations are detected. It is important for us here that when *O* reports an eye-movement he may be right over 90% of the time. Let it be added that *O*'s reaction time to these eye-movements was very small, probably of the order of .2 second. Not every eye-movement, therefore, found in the vicinity of his signal was taken as belonging to that signal, but only those which were unmistakably connected with it.

But how are we to account for Liddell's negative results? The records which he published show no significant correlation; nor do our own, when given merely a cursory examination. But an intensive analytical study of our records has justified itself. A statement that "where the eye-movements do occur, they occur at random," without a statistical study to show that they *do* occur at random is not enough. Furthermore, Liddell concludes[68] that "fluctuations of attention are not causally dependent upon eye-movements; for some subjects exhibit eye-movements . . . during the invisibility phase without causing the *immediate* recurrence of the visibility phase."[69] Now if an eye-movement is to be held responsible for a return of the visibility phase, we must allow time for two things: (1) the time between the eye-movement and the emergence of the faint light, and (2) the reaction of *O* to the appearance of the light. We happen to know that a part or all of this time is relatively long. As one *O* (D) put it, "It is not so simple as merely reporting the presence or absence of an object. Every report implies a judgment as to whether the stimulus is or is not present; whether it is or is not confused with the retinal lights into which it frequently melts and with which it frequently fuses."

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[67] The probable errors were found according to Yule's formula. See H. A. Edgerton, Table of standard errors and probable errors of percentages for varying number of cases, *J. Applied Psychol.*, 10, 1926, 378.

[68] Liddell, *op. cit.*, 252.

In later experiments, in which the liminal light was suddenly exposed to a new spot on the same retina, the same distance from the fovea, after having disappeared in its original position, *O* did not always see the light immediately. For 4 *O*s the average reaction times to these liminal stimuli were, 0.80 ± .24, 1.04 ± .16, 1.15 ± .60, and 2.37 ± 1.80 sec. Thus if an eye-movement is to have an even chance to provide the favorable occasion for a reappearance, one should allow at least one second, and to be certain that it is not a factor, considerably more time.

Technique. The technique of photographing eye-movements follows the Dodge method.[70] The light source for corneal reflection was a small electric arc, and later a 100 w. incandescent lamp with concentrated filament. It was enclosed in a light-proof box which was provided with a round opening 2.5 cm. in diam. over which was placed a condensing lens at a distance of one focal length from the light source. This made an almost parallel bundle of light which was projected out through a tube about 50 cm. long and in diameter a little larger than the opening. Within the tube were placed, at approximately 15 cm. intervals, cardboard washers with openings about 2 cm. in diam. These, with the tube itself, helped to keep the bundle of light parallel. At the outer end of the tube was placed a filter of pot-blue glass plus a minus-red filter to reduce the physiological intensity of the light.

Let us remark here that the presence of this light in the *O*'s visual field is a questionable procedure during fluctuations. It is strong enough to be a constant distractor until after much practice. In the preliminary experiments it was found that the arc light was unsteady. At times some *O*s noted distinct fluctuations in its intensity, for example, W. "The light which was thrown into my eye fluctuated a number of times in violation to the fluctuations of the stimulus. At a number of times both the stimulus and the light were completely obliterated and I got no visual sensation whatever except the dark gray. At other times the light appeared to become dim without completely disappearing. There were a number of fluctuations of the stimulus without any noticeable fluctuations of the light itself, so that the variations were not in a one-to-one correlation, but only at certain times did they appear to be bound together." Such changes—even imperceptible changes—doubtless have a facilitating or inhibiting effect upon the stimulus-light according to Heymans' law. We should thus have a new type of fluctuation superposed upon the old. If eye-movements were effective at all in producing the usual fluctuations, their effect would be considerably masked by these new fluctuations. We later changed to the incandescent light source, which, if it physically fluctuated at all, would be accompanied by a corresponding fluctuation of the stimulus-light because they were on the same local electric circuit. We thus had eye-movement records from both kinds of light source. Both apparently correlate equally well with fluctuations, although the number of records from the arc light are too few for us to be sure on this point. The disadvantage of the incandescent light is that it is not so nearly a point-source and gives a less sharp eye-line on the film than does the arc-light.

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[69] Italics mine. [70] Dodge, *op. cit.*

Besides the unsteadiness of the blue light, found in W's report, in the case of the arc, there is the second disturbing tendency for the whole field to 'go blind,' as D reports it: "at times it seems as if the whole field of vision goes blind; that is, it seems as if nothing is being seen with the left eye." Attempts were made entirely to eliminate any visible photographic light by the use of filters which transmit only ultraviolet wave-lengths. With the combination of an ultraviolet filter and a minus-red gelatin all visible rays were eliminated, but the intensity was so weak photographically as to make no impression on the film. With the ultraviolet filter alone the visibility was reduced to a negligible minimum; but no readable impression could be obtained upon the film. It may be that one could succeed by using quartz lenses throughout in place of glass, but we did not try this procedure. In the face of all these difficulties, therefore, any correlation found between fluctuations and eye-movements will be very significant.

The beam of blue light was directed from the rear and to the left of the *O* into a mirror which was directly below the camera lens. The latter was about 18 cm. from the cornea of *O*'s left eye. The mirror was so adjusted that the reflected beam fell upon *O*'s left eye completely covering the cornea, where a tiny bright spot was produced. It is this brilliant spot that is photographed in the Dodge method.

The camera was 55 inches long from the lens to the film. In place of Dodge's falling plate device a panoramic camera was attached for carrying the film. The film, which was 5 feet by 6 inches, was wound up on a drum at the rate of 16 mm. per sec., the slowest speed attainable with the spring-motor of the panoramic camera. The film was always run in the vertical direction so that horizontal eye-movements, which are the most numerous, show in the records as breaks or waves in the eye-line. Extremely few movements are exclusively vertical, so that practically all movements will show in the records.

In order to get a simultaneous record of fluctuations beside the eye-line we made use of a small flash-light in circuit with *O*'s reaction-key. This signal light was furnished by a 3.5 v. concentrated filament bulb mounted in the end of a brass tube of variable length. In the other end of the tube, about 20 cm. distant, was a short-focus lens for producing an image of the light on the film. The tube was mounted inside the camera-box to one side of the path of light from the eye, with its lens about 22 cm. from the film. When *O* pressed his key to indicate a disappearance the light traced a straight line on the film beside the eye-line.

In order to insure that the two images were even with each other, they were brought together upon a focusing screen before each series of fluctuations. *O*'s head was held fast by a very rigid biting-board and head-rest. During the series *E* could see the two images from the back of the film. If they should not stay abreast the film could be stopped momentarily and the two dots produced at this point from over-exposure indicated their relative positions.

Procedure. *O*'s right eye was covered throughout the experiments to avoid any stimulation and subsequent retinal rivalry. In every experimental hour *O* adjusted his position in the head-rest and biting-board and the camera was focused. In preliminary trials *O* made several adjustments of the stimulus-light in order to find an intensity which would fluctuate quite freely. Once this value was established it was used throughout all later experiments. The stimulus was a white patch of light from the Nagel adaptometer 1 cm. square, to agree with that used by Liddell, without a fixation point. *O* was accustomed to the sound of the camera during preliminary trials. This sound was constant and scarcely noticed by *O*. As we shall show later, distracting stimuli—other than visual—during fluctuations are no more serious than in the average psychological experiment. Each series lasted 97 sec. Before every one the camera was readjusted so as to get the eye spot and signal light together and in focus. As soon as *O* began reporting fluctuations the camera was started. After every series *O* had a short rest during which he gave verbal reports. Six series as a rule were secured in each experimental hour. We have on the whole at least 18 min. of fluctuations and eye-movement records from every one of the 4 *O*s, using the Liddell conditions as nearly as we could reproduce them.

Results. The special questions which we hoped to answer from the eye-movement records were the following. (1) Are there more eye-movements in the interval just before a reappearance than at other times during fluctuations? (2) Do eye-movements during the invisibility phase increase in frequency up to the point of reappearance? (3) Is the eye more or less quiet just before a disappearance? (4) Is there a direct correlation between the number of fluctuations and the number of eye-movements during a single series?

In answer to the first three questions we have counted the number of eye-movements during three representative phases: the 2.5 sec. just before a disappearance; the same time just before and just after a reappearance.[71] The periods were divided into ½ sec. intervals in order to note any trends in frequency of eye-movements during each phase. We chose 2.5 sec. as the limit because eye-movements during that time just preceding disappearance or reappearance may have some influence upon those events. The time was not made greater than 2.5 sec. because the average periods of visibility and invisibility are of course relatively short. Many invisibility periods are shorter than 2.5 sec.; none shorter than 1 sec. was included in the count. Visibility periods were not included in the count if less than 2.5 sec.; even in those less than 5.0 sec. in length the periods just after one reappearance overlap those just before the next disappearance. Fortunately the visibility periods were relatively longer than the invisibility periods, as is almost invariably the case when *O* is allowed to select his own stimulus; he selects one above his limen of fluctuation.

We counted separately and collectively, three kinds of movements, tremors, re-fixations, and winks. Winks were included because the records show that the eye makes a lateral movement at the same time. We make no claim of accuracy in counting the number of tremors, but every perceptible one—a movement out and back—in the eye-line was counted, depending upon the sharpness of focus of the eye-line and the depth of impression on the film. The re-fixations and winks are usually unmistakable in the records.

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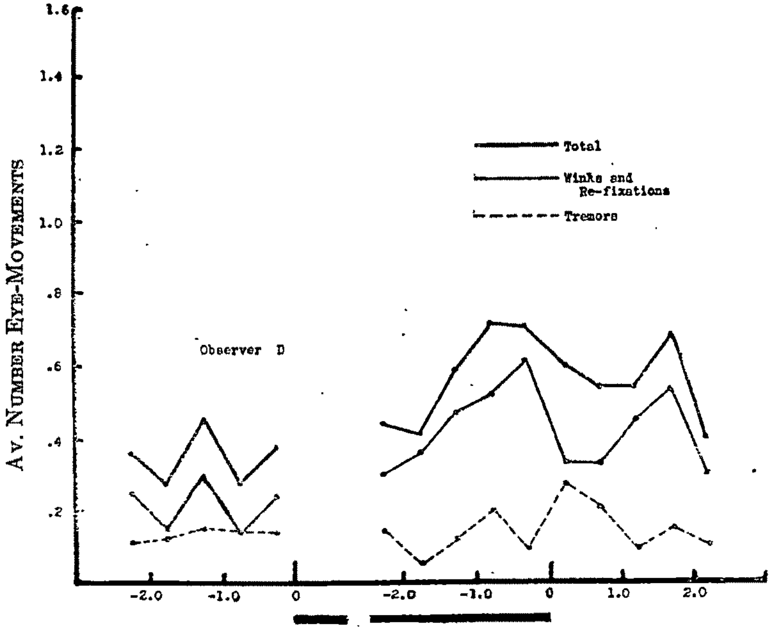
[71] We are indebted to Dr. E. Culler for suggestions on the treatment of the data on eye-movements.

The curves in Figs. 7, 8, 9, and 10, represent in clearest form the results. The average number of movements is based upon 30-80 cases of disappearance or reappearance. The curves may be interpreted as follows: (1) There is usually a minimum of activity of the eye in the 2.5 sec. interval before a disappearance, especially in the number of winks and re-fixations. Furthermore, the total number of movements per ½-sec. tends to become progressively smaller as the moment of disappearance is approached. This no doubt furnished more favorable conditions for local adaptation. (2) The phase just before a reappearance shows the greatest amount of oculomotor activity, especially in the number of winks and re-fixations. (3) Of all the ½-sec. intervals studied, the second one before a reappearance shows the greatest number of winks and re-fixations, and the first and third intervals usually come next.



PHASE OF INVISIBILITY
Fig. 7

There is a second high point in the curve about 2 sec. after reappearance for which we are unable to find any significance. This hyperactivity apparently subsides before the next disappearance. It may be a continuation of the marked instability of the eye exhibited just after the reappearance. This instability is extreme in the case of 2 *O*s (H and Ha), but shows itself mostly in the form of tremors. It is due to the necessary readjustments which the eye makes to the returned stimulus-light. Fixation is partially or entirely lost during the invisibility phase due to wandering or 'searching' movements when there is no fixation-point. Even where fixation is more stable, as in the case where there is a fixation-point, the reappearance of the additional light in the field apparently has its motor consequences in the eye. (See Plate I, H_3; and Plate II, D_5). The fact first mentioned, that the most frequent period for winks and refixations comes during the 1.5-sec. interval just before the return of the stimulus-light, is all the more significant when we recall that this coincides with the reaction times of the observers to these liminal lights.

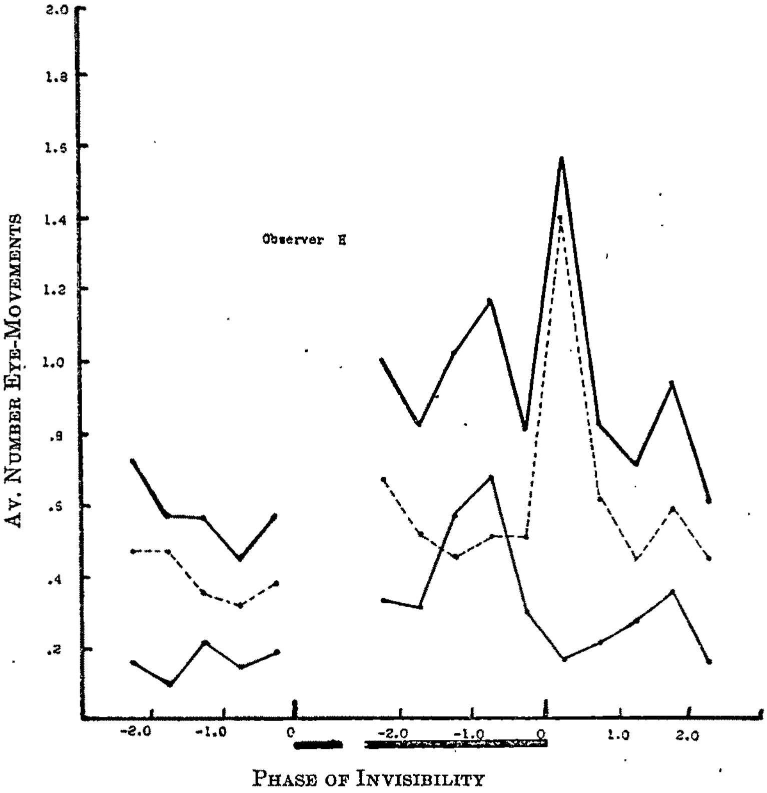
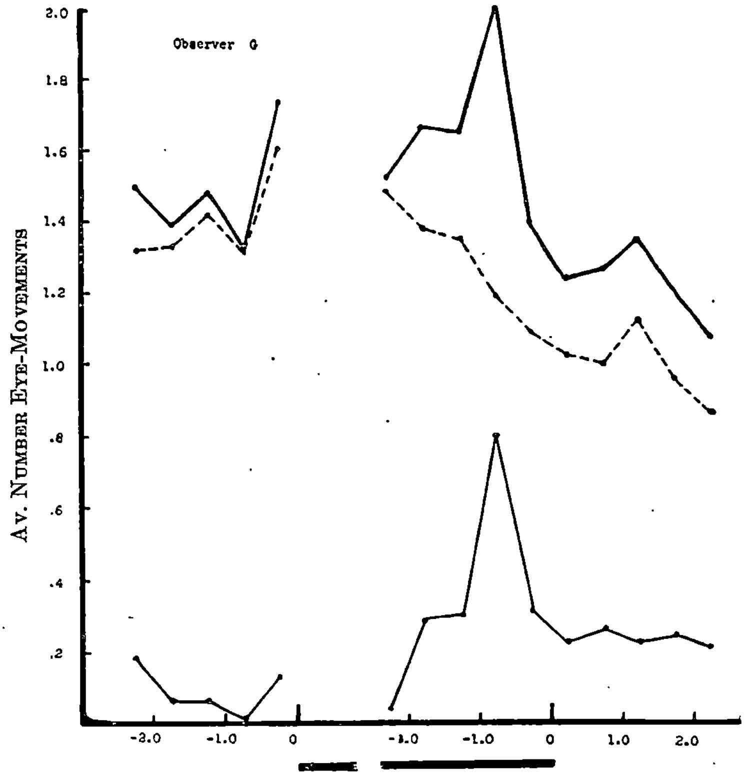


Fig. 8

Examples of good correlations between eye-movements and fluctuations will be found in Plate I, G_1, G_2, D_3, D_4, and H_3. The records are to be read from the top down. The straight white lines represent invisibility periods.

On the negative side, where eye-movements are rare or entirely eliminated, fluctuations should decrease in frequency or cease altogether. There is some evidence of this sort in one case.



Phase of Invisibility

Fig. 9

D had, in the previous experiments, a number of series of fluctuations in which the stimulus-light would disappear and remain out for the rest of the period of 3 min. Coupled with such a series were verbal reports of which the following is typical. "During the experiment I made an extraordinary effort to keep fixation constant. I can report that the eyes did not slip from the fixation-point a single time. . . . At no time, as the signals during the experiment show, did the stimulus appear." Fortunately a similar series occurred during the photographing of eye-movements so that we are able to test D's report of good fixation. Plate I, D_2 shows a part of the record. There was a fixation-point, as in similar earlier series, and the stimulus was exposed in parafoveal vision. There are a few slight re-fixations, but no distinct tremors. It is interesting to compare this record with the next two—W_1 and G_1—in which there are no re-fixations, but numerous tremors. In both these cases there were fluctuations.

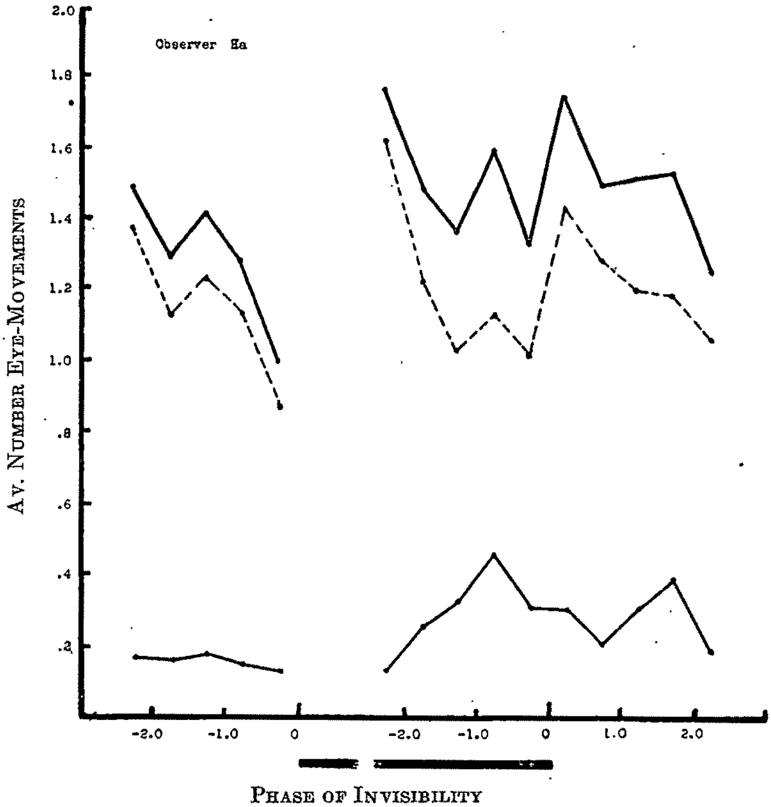


Fig. 10

Whether or not the comparison is significant we are unable to say, but it may mean that the presence of tremors favors fluctuations, while the absence of them removes a sufficient condition for the return of the stimulus-light, if not a necessary condition.

Another fact which was noted during the counting of eye-movements was the frequent coincidence of winks or re-fixations and brief disappearances, nearly always less than 1 sec. duration. We therefore studied all cases of disappearance of less than 1 sec. to see in how many of these cases a wink or re-fixation actually preceded. Examples of this coincidence may be found in Plate I, D_4 and H_3. Table III gives the results.

In the case of 2 *O*s at least half of the momentary disappearances were preceded by a wink or re-fixation. This is significant only when we examine the curves again, and find that the usual disappearances—those larger than 1 sec.—are unusually free from disturbances of this kind just preceding. What happens in the case of these short disappearances is probably this: during the wink or shift of fixation accommodation lapses, or else, as is generally known, the eye mediates no clear image due to a moving physiological stimulus. The stimulus-light is thus momentarily lost. When the eye comes to rest again a new image is mediated and *O* reports a reappearance. That these disappearances are so brief is also significant. Ordinarily a stimulus-light so weak that a chance disturbance causes its complete loss remains invisible for the usual period. Apparently where a wink or a re-fixation furnishes the disturbing factor, at the same time and in other ways, it promotes favorable conditions for visibility the moment the eye comes to rest.

TABLE III
Momentary disappearances preceded by eye-movements

O	Number	Percent preceded by			
		Winks	Re-fixations	Either W or R	P.E.
D	24	29.1	29.1	58.2	6.7
H	21	28.5	28.5	57.0	7.2
G	17	0	47.1	47.1	8.1
Ha	13	15.3	7.7	23.0	8.5

Our fourth special question, whether there are more fluctuations in the series where there are more eye-movements, cannot be answered from our data, nor is an answer so necessary after finding the more direct correlations. The rapidity of fluctuation is determined by many factors and in general varies with the distance of the stimulus from the limen. We did not establish a limen for the stimuli used during this photographic work, and the number of single series is so small that it would be useless to attempt a correlation between frequency of eye-movement and of fluctuations. We have only a rough indication of such a correlation in series like those in Plate I, D_2, W_1 and G_1. There is a decided absence of tremors in the first and an abundance of them in the second and third, whereas there are a few re-fixations in the first and none at all in the other two. If this correlation between frequency of tremor and fluctuations holds, we would have to conclude that whereas re-fixations furnish an occasional condition for visibility, tremors are a constant factor for visibility. Further evidence will have to be found before such a conclusion is justifiable.

Our data, like those of Liddell's, show many instances where fluctuations seem to go on independently of eye-movements. On the one hand, there are numerous reappearances without a detectable eye-movement other than the regular tremors. (See Plate I, W_1 and Plate II, G_4.) The possibility that a mere innervation of the eye-muscles is sufficient to reinforce the weak light, as McDougall has suggested, has not been eliminated. Such innervation might be expected to produce tremors in the non-atropinised eyes of our *O*s and these tremors should appear in the records. But there is little evidence from our curves that even the noticeable tremors have any occasional effect of this kind, of immediate reinforcement of the weak light. On the other hand, there are long invisibility periods in which numerous re-fixations seem to have little effect in bringing back the light. (See Plate II, H_1 and H_2.) These two instances, along with the incomplete correlation we find between eye-movement and fluctuations, furnish additional evidence that there are other factors apart from the oculomotor apparatus which make for or against perceptibility of a liminal stimulus.

From this section of our general problem we have evidence for the following conclusions. (1) Local retinal adaptation is a factor in the disappearance of lights during fluctuations with liminal stimuli. (2) Winks and re-fixations are occasional factors favoring the return of a liminal light which has disappeared. (3) Eye-tremors may furnish a constant factor for visibility. (4) Winks and re-fixations often bring about momentary disappearances of a liminal light. (5) There are other factors than eye-movements which promote the return of a liminal light which has disappeared.

We shall now turn to the central part of the optic sector to look for other factors.

IV. TESTS FOR CENTRAL FACTORS

If we take our historical background alone, we shall have little except theory to begin with. We have fatigue theories on the one hand and theories of facilitation and inhibition on the other, or hypothetical combinations of these effects. Without going into a detailed exposition, let us see what the theories can be made to mean in general terms. There may be a general cortical fatigue, as first suggested by Lange in connection with fluctuations,[72] or a local fatigue according to McDougall's physiological theory of attention.[73] There are also the 'facilitation' and 'inhibition' theories of Lehmann[74] and Slaughter[75] to account for the connection between breathing and the

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[72] Lange, *op. cit.*, 420.

[73] McDougall, *op. cit.*, 349 ff.

[74] Lehmann, *op. cit.*, 84 ff.

[75] Slaughter, *op. cit.*, 332 ff.

Traube-Hering waves and fluctuations. In this part of our problem we have confined ourselves to a study of the fatigue theories. We have now to ask whether the excitation of a part of the optic sector produces a condition in the central portion which reduces the receptivity for the same excitation.

This question may be answered by relying upon the fact that for corresponding points in the two retinas, the central portions of the optic sector are anatomically, and perhaps also functionally, identical. More specifically stated, we can present a liminal stimulus to a certain area of one retina, then, when it has disappeared, transfer the same stimulus to the corresponding area of the other eye. If there is a central fatigue and if this were the only factor, the light should remain invisible for the usual period. If, on the other hand, the conditions are entirely peripheral, the light should reappear immediately.[76]

This test was applied in the earlier part of our work with ambiguous results. The technique, to be described later, was so arranged that the moment *O* pressed his key to denote a disappearance the stimulus was automatically shifted to the other eye. Since *O* was working in a completely dark room this shift could be accomplished without his knowledge, and he could not always tell which eye was receiving the stimulation. One *O*, W, nearly always reported an immediate return of the light under these conditions; another *O*, Ho, reported normal fluctuations as if the stimulus had been continuously exposed to one eye; a third *O*, Di, gave mixed reports; at times the light came back immediately and on rarer occasions it did not. It was obvious that the conditions would have to be better controlled. We did not know whether the stimuli used were above or below the limen. Probably they were above the limen, since the *O* in every case selected the stimulus which would fluctuate. We did not know whether the stimulus was always striking corresponding points in the two eyes. In some cases it obviously was not, for *O* often reported that it moved slightly in pressing down his key. Since there was some evidence for central fatigue here we proceeded to find the limens for the *O*s in the later experiments and to provide a fixation point.

Assuming that there is some fatigue in the central part of the optic sector, we have the additional problem of its extent. Does it spread beyond the parts immediately involved in this particular neurological pattern? Is the spread, if any, limited to one hemisphere? These new questions may be tested by shifting the stimulus from one eye to another on non-corresponding points, involving in the center one hemisphere or both. This procedure can be checked by a shift from one area to another in the *same* eye, also involving one hemisphere or both. We have therefore 6 variations which will hereafter be known as follows:

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[76] We are indebted to Professor Kurt Koffka for suggesting this test of central *vs.* peripheral conditions, and to Miss Ruth Burke for aid in the statistical treatment of data in this section.

IA, stimulus continuously presented to the same area, one eye.
IB, stimulus shifted, after disappearance, to the corresponding area of the other eye.
IIA, stimulus shifted to a new area of the same eye, same hemisphere involved.
IIB, stimulus shifted to a new area of the same eye, the other hemisphere involved.
IIIA, stimulus shifted to the other eye, non-corresponding points, same hemisphere involved.
IIIB, stimulus shifted to the other eye, non-corresponding points, the other hemisphere involved.

As control experiments, an arrangement was so made that the stimulus was not shifted at the moment of disappearance, but at varied intervals afterward. The stimulus thus continued to act upon the original area longer than for complete 'adaptation,' and its effects upon the sensitivity for the new stimulation should therefore be intensified. This control will be designated as C_1 and it will be found in connection with all the variations except IA. A second control, to be called C_2, consisted in delaying the transfer of stimulation but in cutting the stimulation off from the original area in the interim. This interval of rest should allow for some degree of recovery from the effects of the original stimulation and the results should present a contrast to those in C_1 if there are transferred fatigue effects from the first stimulation to the second. It also gave us a basis for establishing the normal reaction-times to the liminal stimuli which we used in the previous section.

Technique and Procedure. The stimulus was the same as in Section II of this paper, a white circular patch with a red fixation-point. The IB series were alternated with the IA series from which the data in Section II are taken, so that *O* rarely knew whether the stimulus was given to one eye or alternately or to both.

The method of transferring the stimulus from one eye to the other was accomplished by means of a cardboard shutter 17.5 x 4.2 cm. In IB the shutter was placed vertically before *O*'s face about 35 cm. distant, so that the fixation point was visible to both eyes just above the upper end. When the shutter was turned slightly to *O*'s right, the stimulus was exposed to his left eye but not to his right. The shutter was fastened at its lower end to a rotary solenoid, the electromagnet and core from an automatic piano player. A current sent through the magnet coils turned the shutter to the left; a spring turned it to the right. The solenoid was controlled through a secondary electric switch in circuit with *O*'s key, so that when *O*'s key was pressed to denote a disappearance, the shutter would move from left to right, or from right to left, according as *E* desired and set the electric switch. In arrangement C_1 the shutter was controled by *E*'s key.

The shutter was so placed, and so adjusted in width, that the stimulus would begin its exposure to the second eye the moment the cutting-off began for the first eye, so that the complete circle was exposed to one eye or to the two jointly without intermission or duplication if possible. The shift was instantaneous. Although its reaction-time was not determined, it must have been of the order of 100 sigma, or less, since, in some of the other arrangements in which the stimulus was exposed in two different positions, phenomenal movement occurred. Since the stimulus was directly below the fixation-point and in the same plane, it should strike corresponding areas in the two eyes. Since convergence is often inaccurate in the dark, however, the stimulus might appear double for the two eyes. In case *O* reported that it appeared in two slightly different positions, the apparent distance of the fixation-point could be altered in the mirror until the stimulus appeared in identical positions for the two eyes.

In IIA two similar circular patches 42 mm. in diam. were exposed to the left of the fixation-point, one above the other, both about 40 mm. from this point. The stimuli were shielded from the right eye and the shutter was now placed nearer to the stimuli and in the horizontal position and adjusted so as to cover the two alternately. The fixation-point was visible to both eyes.

In IIB two similar circles were placed below and to the right and left of the fixation-point, again 40 mm. distant. The right eye was shielded from the two but not from the fixation-point. The shutter was placed in the vertical position as in IB but nearer to the stimuli so as to cover them alternately.

IIIA is a more difficult procedure. Both stimuli must be presented on the same side of the fovea, one to the right eye and one to the left. The arrangement of stimuli and shutter was as in IIA, but a cardboard shield, placed close to the eyes, was so cut off that the right eye was exposed to the lower spot when the shutter uncovered it and the left eye was exposed to the upper spot when the shutter uncovered it, and both eyes were exposed to the fixation-point at all times.

An entirely different but fairly simple procedure was followed in IIIB. Here the fixation-point was brought apparently much nearer than the plane of the stimulus. The single stimulus spot was therefore seen in two positions for the two eyes and the required shift was produced, *i.e.* from one eye to the other, involving non-corresponding retinal points and two hemispheres. The shutter was placed in the vertical position and merely shut off the view alternately for the two eyes as in IB.

In the control experiments (C_2), a new shutter was introduced similar in every respect to the first but 7.0 by 17.5 cm. It was placed nearer to the stimulus than the first and was always controlled by *E*'s key, covering or uncovering a stimulus-spot for either eye or both as desired when the key was pressed. The usual procedure in C_2 was as follows. *E* would close his key so that the stimulus next to be exposed to *O*, when he closed his key to denote a disappearance of the first stimulus, was covered. *O* would press his key to report a disappearance of the original stimulus and from 1–6 sec. later *E* would release his key, exposing the new stimulus. Both keys were in circuit with writing points which made graphic records along with a time-record.

The amount of time devoted to each variation differed from one *O* to another. At least two days were given to every one of the 3 or 4 chosen stimuli in IB. These series were alternated with the IA series and with control series. For the other variations usually only the liminal stimulus was used, two days for each variation with control series included.

Results. The evidence for or against central fatigue comes by way of a comparison of the results from IA and IB. As in the preliminary experiments we find individual differences, although not nearly so great. Three *O*s showed a mixed form of reaction in IB; sometimes the stimulus-light would be seen immediately as soon as it was shifted, at other times it would not. Ha consistently reported an immediate reappearance.

Variations are extreme in both IA and IB as one might expect where there is a complexity of conditions and none of them under rigid control. We feel justified therefore in all the following numerical comparisons in basing our conclusions upon averages and the differences between averages, depending upon a multiplication of cases to weight the evidence in one direction or another. The data which were secured during the 10–15 min. interval after the beginning of dark adaptation were not used here because the retina had not yet reached a stage of equilibrium. Series were also excluded in which an *O* reported mists and hazes to be so numerous as to interfere with observation.

TABLE IV[77]

O	Stimulus Value	IA			IB			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.$_{Av.}$	N	Av.	P.E.$_{Av.}$				
D	2250	22	32.09	4.900	89	2.06	.120	30.09	4.901	6.14	1.000
	2750	24	17.17	2.831	88	1.71	.071	15.46	2.832	5.46	1.000
	3250	48	15.77	1.854	110	1.52	.074	14.25	1.854	7.69	1.000
	3750	15	13.56	3.713	33	3.08	.280	10.48	3.723	2.81	.971
H	800	36	10.46	1.773	83	5.17	.404	5.29	1.818	2.91	.975
	1150	93	7.81	.554	174	3.56	.282	4.25	.622	6.83	1.000
	1500	129	5.78	.354	180	2.35	.143	3.43	.382	8.98	1.000
	1900	128	5.66	.375	157	2.69	.203	2.97	.426	6.97	1.000
G	925	36	19.47	2.355	38	14.47	1.657	5.00	2.879	1.74	.879
	1200	35	12.74	1.569	77	7.07	.908	5.67	1.813	3.13	.982
	1475	75	11.95	1.035	109	6.95	.624	5.00	1.208	4.14	.997
	1750	64	10.36	.880	32	6.41	.836	3.95	1.214	3.25	.986
Ha	All	338	4.03	.141	444	.85	.024	3.18	.142	22.39	1.000

When we compare the averages for the invisibility periods under conditions IA and IB, we find a decided difference in favor of the former. The data in Table IV show beyond doubt that the averages of invisibility periods for IA will almost certainly be greater than those for IB under our conditions. In the case of G only does there seem to be a chance for an average to be greater for IB. This result might mean that the central factors, if they exist at all, have a minor influence upon fluctuations. But the test is not a fair one. Although identical projection areas may be involved in the two eyes, a change of eyes changes a considerable portion of the whole retino-cortical sector. A change of a part of the optic sector may be equivalent to changing the whole as the configurationists so often tell us. Besides, many single invisibility periods in IB are so prolonged that there appears to be an inhibition of some sort, as indicated also in some verbal reports. D reported on one occasion, "Sometimes the stimulus immediately reappeared and at other times it continued to stay out." He remarked, "It stays in for a long time and disappears and stays out for a long time.. . . Sometimes when the light gray disappeared it would reappear very quickly and fairly brightly, but at other times there was the long duration I have mentioned before."

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[77] In most of the tables it will be noticed that all the results for Ha are averaged together. This was done because a change of stimulus-intensity, within the rage of stimuli used for her at least, made no difference in the results.

This delay in return is not necessarily due to the mists and hazes which sometimes hide the stimulus-light. The delay occurred in series after which *O* reported that the field was exceptionally free from mists or hazes, and in *E*'s own observation the field during these delayed returns often takes on an exceptionally black uniform appearance.

TABLE V

O	Stimulus Value	IBC$_1$			IBC$_2$			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.$_{Av.}$	N	Av.	P.E.$_{Av.}$				
D	2250	15	1.96	.155	14	1.69	.126	.27	.173	1.56	.853
	2750	14	1.91	.147	26	.97	.012	.94	.147	6.39	1.000
H	1500	18	1.12	.028	19	1.22	.119	−.10	.122	.82	.710
	1900	17	.95	.017	15	.62	.018	.33	.025	13.20	1.000
G	1750	24	2.80	.563	18	1.43	.219	1.37	.604	2.27	.937
Ha	All	22	1.03	.098	64	.74	.025	.29	.101	2.87	.973

We cannot infer from the first comparison alone whether there has been any lengthening of the average period at all due to central factors. A comparison of IBC$_1$ and IBC$_2$ will give some indication of this because, in the former arrangement, IBC$_1$, the stimulus is left upon the first eye after disappearance to continue its effects; whereas in the latter, IBC$_2$ the stimulation is cut off the first eye and not for some time exposed to the second. If, then, there is an inhibitory effect of the first stimulation upon the second the invisibility periods for IBC$_2$ should be shorter than those for IBC$_1$. This is, in fact, the case as demonstrated in Table V. There is an exception in only one *O*, H, where with one stimulus-value the difference is negative, but with uncertain probability.

There is another test which consists in a comparison of the averages from IB with those from IIA. In the latter the stimulus is shifted to a new area of the same eye, thus involving the same eye but different retinal and cortical elements. In Table VI the comparison shows that in every case the averages from IB are longer than those from IIA and we may be almost certain that they will always be so under our conditions.

But it may be objected that in IIA new factors come in. One is the fact that the stimulus reappears in a new phenomenal direction and another is that there is phenomenal movement. When *O* presses his key to denote a disappearance and the stimulus is automatically shifted to a new position above or below, as the case may be, he invariably sees movement, which is described in general as follows:

TABLE VI

O	Stimulus Value	IB			IIA			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.Av.	N.	Av.	P.E.Av.				
D	2250	89	2.06	.120	42	1.76	.139	.30	.183	1.64	.865
	2750	88	1.71	.071	122	1.15	.054	.56	.089	6.29	1.000
	3250	110	1.52	.074	55	.75	.032	.77	.081	9.51	1.000
H	1150	174	3.56	.282	30	1.11	.060	2.45	.288	8.51	1.000
	1500	180	2.35	.143	25	1.04	.032	1.31	.146	8.97	1.000
	1900	157	2.69	.203	34	.91	.027	1.78	.204	8.73	1.000
G	1475	109	6.95	.624	117	3.05	.218	3.90	.661	5.90	1.000
	1750	32	6.41	.836	33	3.23	.507	3.18	.977	3.25	.986
Ha	All	444	.85	.018	131	.80	.021	.05	.028	1.79	.886

> Ha. "The typical proceeding was, brightness appeared at the bottom [as *O* pressed her key to denote a disappearance at the top], flash off [as *O* released her key to denote a reappearance, the shutter covered it], then a dimmer brightness at the top, dying out gradually and leaving behind a black hole [negative after-image]." Again, "Below there appeared a rather good brightness and this progressed toward the top. There was more progression or movement here." D reported: "The stimulus was in the lower position. It disappeared, and as I signaled it reappeared above; when I signaled that I saw the light it immediately fell down to the original position and was visible during the whole drop. When it reaches the bottom the phenomenon is repeated." Again: "The movement that the stimulus makes when it jumps above attracts attention and, even though the retinal lights are strong, differentiates it from the retinal light."[78]

The objections are minimised if we use instead of IIA, the control series IIAC$_2$. Here the second stimulus is not uncovered until 1–6 sec. after *O* has signaled the disappearance of the first. No movement occurs and although *O* learns to expect the reappearance in a certain position, the arrangement is almost equivalent to the presentation of an entirely new stimulus. Indeed, it is chiefly upon these reactions that we have based our reaction-times to liminal stimuli. Table VII shows that there is still a delay in the reappearance in the case of IB. The differences in averages are practically certain for 3 *O*s but lie in both directions with much less probability in either direction for D.

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[78] This case of phenomenal movement is interesting as a by-product of the study. The time-interval for shifting stimuli is evidently of the order of magnitude which produces phenomenal movement. The movement here is opposed in direction to that of the moving shutter and even opposed to the order of exposure of parts of the stimulus-object; *i.e.* the remotest parts of the second stimulus are exposed first and the nearest parts last and the first stimulus is cut off in the reverse order. Facts which verify Wertheimer's cortical theory were quite evident in arrangements IIB and IIIB in which the two stimuli are given to the two eyes separately and good phenomenal movement occurred.

TABLE VII

O	Stimulus Value	IB			IIAC₂			Diff.	P.E.D	D/P.E.D	P.C.
		N	Av.	P.E.Av.	N	Av.	P.E.Av.				
D	2250	89	2.06	.120	19	2.50	.390	−.44	.408	1.08	.771
	2750	88	1.71	.071	18	1.65	.231	.06	.242	.25	.567
	3250	110	1.52	.074	22	2.04	.360	−.52	.367	1.42	.831
H	1150	93	3.56	.282	26	1.12	.027	2.44	.283	8.62	1.000
	1500	180	2.35	.143	13	1.03	.105	1.32	.177	7.46	1.000
	1900	157	2.69	.203	21	.76	.035	1.93	.206	9.37	1.000
G	1475	109	6.95	.624	9	2.69	.520	4.26	.812	5.25	1.000
	1750	32	6.41	.836	39	2.73	.261	3.68	.870	4.23	.998
Ha	All	444	.85	.018	89	.75	.019	.10	.026	3.85	.995

Another objection may still be offered. Do we know that the sensitivity of the two chosen areas is the same? To be sure, they are the same distance from the fovea, but one is below the fovea and the other is to one side of it. We can determine this, possibly, from a comparison of the results from IIAC₂ with those of IBC₂. IBC₂ should give us the approximate reaction-times and hence should indicate the sensitivity of the area used in IB as compared with that used in IIA and IIAC₂. If the averages for IBC₂ are less than, or equal to, those for IIAC₂, then the area used in the former is just as sensitive or more sensitive than that used in IIAC₂. This is apparently the case, as revealed in Table VIII. The differences lie in both directions and, if favored in either direction, they would probably lie on the side of IIAC₂ as being greater. The only other obvious difference which may exist in the two cases is that IB involves two

TABLE VIII

O	Stimulus Value	IBC₂			IIAC₂			Diff.	P.E.D	D/P.E.D	P.C.
		N	Av.	P.E.Av.	N	Av.	P.E.Av.				
D	2250	14	1.69	.126	19	2.50	.390	−.81	.410	1.98	.909
	2750	26	.97	.012	18	1.65	.231	−.68	.231	2.94	.976
	3250	20	.85	.031	22	2.04	.360	−1.19	.361	3.29	.986
H	1150	4	1.97	.400	26	1.12	.027	.85	.401	2.12	.923
	1500	19	1.22	.119	13	1.03	.105	.19	.158	1.20	.791
	1900	15	.62	.018	21	.76	.035	−.16	.039	4.10	.997
G	1475	4	4.47	1.405	9	2.69	.520	1.78	1.497	1.19	.789
	1750	18	1.43	.219	39	2.73	.261	−1.30	.347	3.75	.993
Ha	All	64	.74	.025	89	.75	.019	−.01	.031	.32	.585

eyes and two hemispheres whereas IIA involves one eye and one hemisphere. But IIIB also involves both eyes and both hemispheres and the data show that its average periods will always be less than those of IB. A study of the relation of the two eyes and the two hemispheres under our conditions yields no conclusions which invalidate the comparison of IB with IIA.

Extent of Central Fatigue. We have shown that there is evidence for central 'fatigue' or inhibition however slight and unstable it may be.[79] We are now ready to consider the question of its extent. Our data give no unambiguous answer to this question. But let us see what may be found.

TABLE IX

O	Stimulus Value	IIA			IIB			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.$_{Av.}$	N	Av.	P.E.$_{Av.}$				
D	2750	122	1.15	.054	105	.94	.040	.21	.067	3.13	.982
H	1500	25	1.04	.032	63	1.02	.024	.02	.040	.50	.632
G	1475	55	3.45	.345	117	3.05	.218	.40	.408	.98	.645
Ha	4900	131	.80	.021	139	.51	.008	.29	.022	13.19	1.000

The question whether the inhibitory effect extends throughout the visual center of the hemisphere involved may be tested by a comparison of the results from IIA and IIB. In both cases the same retina is involved, but in IIB two hemispheres. If the inhibitory effects spread in the same hemisphere, the invisibility periods in IIA should give an average greater than in the case of IIB. Likewise, the averages for IIIA should be greater than those for IIIB. A third test should be a comparison of IIAC$_1$ and IIAC$_2$. The former should give greater averages than the latter. Tables IX, X, and XI do not exhibit these necessary conditions. Yet the 4 *O*s agree that the averages for IIA will exceed those for IIB—2 of them with certainty and 2 with some doubt. In Table X one *O* is certain to give a positive difference while the other 3 give negative differences. Although the second test might be questioned, because it brings in the two different eyes, the third test is crucial and cannot be neglected.[80]

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[79] We discovered during the course of the experiments an important verification of this fact in a qualitative study of a similar nature by K. Dunlap (Light spot adaptation, *Am. J. Physiol.*, 55, 1921, 201–211). F. Allen (Reflex sensations in vision, *J. Opt. Soc. of Amer.*, 7, 1923, 583 ff.), finds, however, just the opposite effect with intense stimuli. White light in one eye sensitizes—as Allen finds—the other, especially in the red, green, and violet. Although he has no evidence for a similar facilitory effect of weak light, he believes that "the reflex effects excited by them are proportionately large—even disproportionately large—with intense colors." (See also by the same author, On reflex sensations and color contrast, *ibid.*, 913 ff. Earlier studies which bear on the same fact are by E. B. Titchener (Über binoculäre Wirkungen monoculärer Reize, *Phil. Stud.*, 8, 1893, 231–309), and S. I. Franz (*Psychol. Monog.*, 12, 1899, 44 ff.).

[80] Professor Dunlap presents evidence to show that the inhibitory effect does spread to neighboring functional parts of the same hemisphere (*ibid.*, 210).

We may next ask whether the two hemispheres facilitate or inhibit each other. Again there is no unequivocal answer in our data. Tests of this question consist in comparisons of the data from IIA with IIB; of IIIA with IIIB. If there is facilitation the averages from IIA should be greater than those from IIB; those from IIIA should exceed those from IIIB. By facilitation we mean here that a removal of a stimulus affecting one hemisphere favors receptivity of a stimulus affecting the other. This would probably mean mutual inhibition of the two hemispheres for simultaneous stimulation. Further evidence would be forthcoming if the averages from IIIBC$_2$ were greater than those from IIIBC$_1$ (see Table XII). We have already found the first two tests to be conflicting in their tendencies (Tables IX and X) and the third test is negative in the case of the 2 *O*s for whom there are data. Indeed, if we were to base our conclusion upon the last two tests, we should have to say that the two hemispheres mutually inhibit each other for successive stimulations and facilitate each other for simultaneous stimulations. This conclusion would hardly be safe in view of the lack of data in the third test. The second test, the comparison of IIIA with IIIB, however, is so unanimous for 3 *O*s that it deserves special consideration. What other meaning it may have we are unable to say.

TABLE X

O	Stimulus Value	IIIA			IIIB			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.$_{Av.}$	N	Av.	P.E.$_{Av.}$				
D	2750	148	.90	.007	239	.98	.026	−.08	.027	2.97	.977
H	1500	70	1.02	.010	181	1.57	.069	−.55	.069	7.97	1.000
G	1475	121	1.98	.084	172	3.76	.243	−1.78	.257	6.93	1.000
Ha	4900	109	.69	.019	244	.55	.009	.14	.020	7.00	1.000

TABLE XI

O	Stimulus Value	IIAC$_1$			IIAC$_2$			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.$_{Av.}$	N	Av.	P.E.$_{Av.}$				
D	3250	23	.72	.029	22	2.04	.360	−1.32	.361	3.66	.994
H	1150	19	.94	.057	26	1.12	.027	−.18	.063	2.86	.973
H	1500	24	.85	.041	21	.76	.035	.09	.054	1.67	.674
G	1750	77	1.82	.109	39	2.73	.261	−.91	.283	3.22	.985
Ha	4900	41	.51	.013	89	.75	.019	−.24	.023	10.43	1.000

TABLE XII

O	Stimulus Value	IIIBC$_1$			IIIBC$_2$			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.$_{Av.}$	N	Av.	P.E.$_{Av.}$				
H	1500	15	1.17	.207	42	1.05	.052	.12	.213	.56	.697
G	1475	23	2.22	.246	83	1.14	.069	1.08	.255	4.24	.997

A final problem can be tested by our data, although it has little significance for fluctuations. It concerns the retinal consequences of local stimulation with weak stimuli. Does stimulation of a retinal area facilitate or inhibit succeeding stimulation of other areas? We have Heyman's law to the effect that simultaneous stimuli inhibit one another. According to this law, we might expect facilitation from the removal of the first stimulus upon the second. If this were true, the averages in case of IIIA should be greater than those of IIA; those in IIIB than those in IIB; and those in IIAC$_2$ than those in IIAC$_1$. The data are favorable to this conclusion (see Tables XIII, XIV, and XI), except in the case of Table XIII. Here again the relatively low averages for IIIA upset the otherwise perfect correlation.

TABLE XIII

O	Stimulus Value	IIA			IIIA			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.$_{Av.}$	N	Av.	P.E.$_{Av.}$				
D	2750	122	1.15	.054	148	.90	.007	.25	.055	4.55	.999
H	1500	25	1.04	.032	70	1.02	.010	.02	.033	.61	.659
G	1475	55	3.45	.345	121	1.98	.084	1.47	.357	4.12	.997
Ha	4900	131	.80	.021	109	.69	.019	.11	.028	3.93	.995

TABLE XIV

O	Stimulus Value	IIB			IIIB			Diff.	P.E.$_D$	D/P.E.$_D$	P.C.
		N	Av.	P.E.$_{Av.}$	N	Av.	P.E.$_{Av.}$				
D	2750	105	.94	.040	239	.98	.026	.04	.048	.83	.712
H	1500	63	1.02	.024	181	1.57	.069	.37	.073	5.07	.999
G	1475	117	3.05	.218	172	3.76	.243	.71	.326	2.18	.929
Ha	4900	139	.51	.008	244	.55	.007	.04	.011	3.64	.992

If we make a general comparison of the averages from the 6 variations and then rank them for all 4 *O*s combined, we shall find them coming in the following order: IA, IB, IIA, IIIB, IIB, IIIA. If IIIB and IIIA were interchanged we could easily conclude that the nearer the second stimulation comes to the first, peripherally or centrally, the more is it inhibited by the first. This certainly holds so far as identical positions in the retina (IA) and in the projection center (IB) are concerned. IIA also shows a somewhat longer delay in reaction to the second stimulus as compared with the three remaining variations. Whether this continued inhibition from the first stimulation to the second neighboring stimulation exists and whether it is central or peripheral is a question which we have not answered. If such an inhibitory effect can be shown, as Dunlap thinks, it would be contrary to Heymans' law, unless this law applies only to the retina. Our other variations, IIB, IIIA and IIIB and controls, deny any uniformity of differences which can be interpreted as either central or peripheral inhibition for neighboring successive stimuli. We are for the present inclined to the conclusion that the central fatigue or inhibition is quite limited in its extent.

If one does not wish to base a conclusion of central fatigue upon averages from results which are so variable, there is still another method of comparison. We may ask ourselves in the case of IB what percent of the reactions to the second stimulus actually showed effects of central inhibition; *i.e.* what percent are certainly longer than the *normal* reaction-time to such stimuli. In order to find the normal reaction-times of our *O*s to the selected stimuli we may take their reactions to these stimuli under the conditions of IIA, IIB, and IIAC$_2$, disregarding any inhibiting or facilitating effects. Adding to every average reaction-time three times the standard deviation, we established the approximate upper limit of the normal reaction-time to every stimulus-value and used this as a criterion of delayed reactions. Any reactions in IB which are longer than this criterion are therefore certainly delayed.

Table XV

Percent of Delayed Returns

O	IA		IB		IIIA		IIIB	
	%	P.E.	%	P.E.	%	P.E.	%	P.E.
D	82.2	2.3	16.4	1.5	0.0	0.0	5.6	1.0
H	75.7	1.5	36.4	1.3	1.4	1.0	10.6	1.7
G	30.5	2.6	15.5	2.1	0.8	0.6	5.3	1.1
Ha	81.0	1.4	5.1	0.7	2.5	1.0	0.0	0.0

Table XV shows the comparison of the various arrangements in the case of every *O*. The delayed reactions (percent) are lumped together for every *O* because every stimulus-value has its own criterion. IIA and IIB are omitted from the table since they were used in getting the criteria. Their percentage of delayed reactions should be approximately zero. The results demonstrate that from 5–35%, depending upon the *O*, of the reactions in IB are certainly delayed, and many more than this may be so delayed, owing to central inhibition.

Is there any evidence for a rhythm of receptivity such as one should expect along with the breathing and Traube-Hering waves? Yes, but only in the fact that the second stimulus is so unequally reacted to under the same kind of presentation. The light is not always equally perceptible even when an entirely new retino-cortical pattern is stimulated as in IIIB and IIIBC$_2$. To determine whether the physiological rhythms are responsible, one should have a record of the waves along with reaction-times to liminal stimuli, to see whether the short reaction-times coincide with certain phases of the rhythms.

But there are other possible explanations of the irregularity in reaction-time. One is the interference of 'retinal lights,' mists, and hazes which come into the field and influence receptivity to a marked degree. The *O*s' verbal reports are filled with such cases of interference. Another possible factor is eye-movement. Whereas hazes and mists may account for delayed reappearances in IB, eye-movements may be responsible for immediate returns. Both factors, so far as can be determined, operate equally in nearly all the variations, however, so that our previous comparisons will still hold.

It was suspected, however, in the case of those *O*s for whom the stimulus nearly always returned immediately under the IB conditons, that eye-movements might be responsible. Although *O* could not possibly see the shutter move and it was practically noiseless, the change of stimulation might produce eye-movements. Attempts to photograph eye-movements during the IB series were successful in only one case. The difficulty was, of course, that with the blue photographic light in the *O*'s left eye there was a lowered sensitivity in that eye. The limens for the two eyes would be entirely different. With H we used the ultra-violet filter for the photographic light. This reduced the difference to a minimum, but still there was some difference, both in intensity and in quality. We equalised the white stimulus-light for the two eyes by placing wedge-shaped filters of gray and amber before the right eye. The amber was necessary because the stimulus-light appeared yellowish, by contrast, to the left eye. *O* reported that so far as he could detect the conditions were equivalent to the earlier ones in IB. The results show no unusual eye-movements which can be attributed to the action of the shutter.

With all the *O*s we were able to apply a more rigid test of fixation using procedure IIB. Here the stimulus is presented to the left eye alone and the light appears to jump from left to right, or right to left, when it reappears. Some records show steady fixation throughout this kind of series (see Plate II, D_1, W_2, and W_3) whereas most of them show unusual tremors at the moment of reappearance, that is, when the phenomenal movement occurs (see Plate II, D_5). Only in a few cases are there unusual tremors at the moment of disappearance which might be attributed to the movement of the shutter. These movements might mean an oculomotor flux which could have its effects on the visual cortex, removing fatigue effects and working for greater receptivity. Nowhere in the preceding section, however, have we found evidence that tremors do signify such conditions favorable for immediate receptivity.

Since the invisibility periods differ widely under the IB conditions, even when the field is free from mists and hazes, and since there may be immediate returns without eye-movement, we are led to the conclusion that this central inhibition is a very unstable, transitory sort of thing. This is just what one would expect if the all-or-none law of nervous conductivity holds for the simple cortical patterns involved here. Certain phenomenological descriptions, to be treated in the next section, also point in the same direction. We may therefore venture the following conclusions from this section. (1) In fluctuations the liminal stimulus produces a central fatigue or inhibition, a reduced readiness to respond to the same stimulation. (2) This fatigue is relatively local, probably limited to a small part of the optic sector. (3) This fatigue appears to be very unstable and of the all-or-none type.

V. Does Attention Fluctuate?

What can we say regarding the general problem? Are these fluctuations with liminal visual stimuli to be regarded as fluctuations of attention as they have been called historically? If the question means what it has usually meant our answer must be a flat negative. According to this meaning of the question we must ask whether the fluctuations are manifestations of a fluctuating attention or a rhythm of attention, or are manifestations of extra-attentional factors. Our results, in agreement with some of the earlier studies, demonstrate an abundance of extra-attentional factors. Our demonstration that the fluctuations fit into the phi-gamma hypothesis suggests the operation of a number of factors rather than one. The demonstration of peripheral factors, such as local retinal adaptation, eye-movements and accommodation, is a good beginning toward an enumeration of these several factors. It will be recalled that those who hold the above meaning of the question accept the presence or absence of peripheral factors as a crucial test. It is also difficult to explain why the rhythm of attention should change with the change in intensity of the stimulus.

Suppose again that we take the two-level conception of attention. As we indicated in the very beginning, fluctuation of attention then may mean one of two things. It may mean a shifting of content in the two-level pattern, a redistribution of clearness in which one element, say our circular light patch, may periodically suffer to the extent of extinction, or it may mean a periodic loss of the degree of attention, of the difference between the upper and lower levels. If the latter type of change rests upon a general cortical receptivity, as indeed it must, as for example that condition supposed to be produced by a physiological rhythm such as breathing or the Traube-Hering waves, then again we cannot call the phenomenon a fluctuation of attention, because the same phenomenon has been shown to be occasioned by too many local factors. We cannot, of course, deny that there may be such a fluctuation of attention, that is, one dependent upon general physiological rhythms, but certainly the fluctuation of a minimal visual experience is not a simple manifestation of it.

If by fluctuation of attention is meant a shift in the pattern of clearness affecting changes in the relative clearness of mental contents (this change usually falls however, under the phrase 'shift' of attention or 'distraction' of attention), again the answer to the general question must be in the negative. It is even doubtful whether 'shifts' of attention have any more influence upon the fluctuation of minimal experiences than they have upon the average phenomenon which requires careful discrimination. That is the import of the few verbal reports in which the *O*s made any mention whatever of their own attention during fluctuations.

D reported as follows on a number of occasions: "I noticed during the first part of the experiment when the stimulus was present in the field, that I could direct my attention variously without in any way affecting the stimulus. I directed my attention to the biting-board, to the head-rest, to the light that seems to come in from the left side of the room, which I never noticed before. In spite of the fact that they seized my attention the stimulus persisted. . . . I have noticed this many times before but have neglected to report it." "I noticed no correlation whatsoever between the shifts of attention and appearance and disappearance of the stimulus. . . . After it has disappeared I can, on the contrary, attend away from all those things and direct my whole efforts to the expecting and waiting for the stimulus, but to no avail." Ha reported: "My attitude toward the stimulus is passive. I think the stimulus is at all times in the lower level of attention. My active attention is given to keeping my eyes on the fixation point." In the latter case a shift of clearness would probably *favor* the stimulus-light and hence would not cause a disappearance.

Evidence from Phenomenology. The question may be decided upon the basis of phenomenology. One may contend that the conditions which underlie the fluctuations are unimportant in answering the question; that the phenomenon can be called fluctuation of attention regardless of whether the physiological conditions are peripheral or central, general or local. But the more specific question which we now have to answer is whether the attributive changes which occur during fluctuations are at bottom a matter of clearness or attensity. While we are examining the descriptive reports we shall also notice what additional evidence they contain for the conclusions already reached and for some of the older theories, in particular the streaming phenomenon of Ferree.

The manner of disappearance of the faint patch of light is by no means identical in all cases; but the typical procedure includes qualitative, intensive and extensive changes. The qualitative changes consist in a change in the direction of the background, whether the latter is the more or less clear dark gray field or is a mass of formless haze or light gray mist which often occupies the center of the field. The spot and the background are often said to approach each other in quality and to fuse into a homogeneous ground. Intensive and extensive changes are most often characterised as a fading out, or a thinning out or loosening up in texture, together with a loss of contour and some spreading, or else a contracting of the spot into a harder, smaller, more flame-like mass which flickers out. The disappearance may be a slow gradual fading out, or a sudden flickering out, or both—that is, a gradual fading, cutting off abruptly at the end. The following reports testify to this generalized description.

Ho. "There is a course of disappearance; the very end comes rather abruptly. Sometimes before this there is a darkening, fuzzing over, a diffusing of gray with darkness. It becomes weak, diffuse, loose. Sometimes it goes out gradually, just becoming more and more diffuse, but often at the last it snaps out."

"It seemed to fade very gradually for a short period, then disappear all of a sudden as though some screen had intervened between my eyes and it, or as if it had been suddenly immersed in some opaque material. . . . At times there is the appearance of a flame in the stimulus. It seems to waver and shimmer and to disappear by way of a flicker."

Di. "There was a fluctuation of the more and the less intense; of the more and less definite boundary." "It expands, becomes loose, until it is no more than a very expanded luminous mist, then it is gone as the mist dissolves and disintegrates when the sun shines upon it."

D. "The spot changes considerably both in quality and in texture. The spot becomes more and more vapory until it cannot be distinguished from the retinal lights with which it fuses. This sort of disappearance I will designate as a fadeout; the other kind that I mentioned might be called a snapout." "The spot sank to the lower left, winking and blinking along its way until complete disappearance."

H. "The stimulus has two characteristic ways of coming in and disappearing. First it may come in or disappear suddenly, on the all-or-none principle. The other way is when it fades out gradually into the background."

One cannot be sure of the meaning of these two characteristic ways of disappearing, but they must mean the operation of different factors. If one wished to theorize on the basis of what has gone before, one might well attribute the gradual fading to retinal adaptation and the flickering and sudden disappearance to neural inhibitions of an all-or-none type. It is significant from the phenomenonological point of view that not a single direct report of attensive changes can be found. This may well be due to the fact that the *O*s are rarely called upon to make such reports and had no specific instructions to do so here. On the other hand no specific instructions were given for any attributive description and the *O*s knew that the problem lay in the field of attention. The instructions given were merely to "describe the temporal course of your experience."

The reappearance is more often sudden and since it has a short duration descriptions are less complete. Attributive changes are approximately the same as for disappearance, except that they are reversed in time. Sometimes there is a kind of hint of light by way of warning and then a rapid appearance. At other times there is an unexpected return to full intensity. The latter case, as we have mentioned before, frequently follows eye-movements. The following descriptions are typical.

Ho. "When first I get it after disappearance, the reappearance is of very short duration; a few rapid fluctuations in and out. In nearly every case it comes in conversely to the way in which it went out." "The return of the light gray is not uniform. It is usually one of two kinds. Either at first I get a very, very faint hint of light gray quality out there, sort of a halo effect, and then with a little more search around, the light gray comes. The other is that the light gray pops in suddenly with full extent and full intensity."

H. "Again it seemed as if the coming in of the stimulus was presaged by or signaled by a general lightening up of the haze about the fixation point. As it came in the field got darker and it stood out in bold relief."

There were a few reports of the objective and subjective fluctuations first pointed out by Eckener. They are best described by H.

"Sometimes the stimulus seems to disappear in a wholly objective way, that is, the stimulus itself goes out; something out there goes out. At other times it seems as if the stimulus were still there but something has happened to me so that I can't see it. In the first case the stimulus goes out in a clear, definite, objective fashion; I notice no change within myself; I am seeing something and then seeing something else. In the other case it seems as if something happens to me to prevent my seeing the stimulus. In this case it goes out more gradually."

It is not easy to say what the meaning of this report is. That the two types of fluctuation should carry the two meanings of objectivity and subjectivity must be a sign of some fundamental difference. What the difference is remains a problem.

The Streaming Phenomenon. Although we made no specific studies upon Ferree's streaming phenomenon,[81] which formed a part of his theory of fluctuations, numerous reports indicate the influence of various entoptic processes upon the weak stimulus-light. The field is occasionally full of various phenomena which sometimes cut across and obliterate the stimulus-light. The following reports are typical.

Wa. "I did not get the effect of the white space moving to the side, but of a screen; a portion of the field came in between and blotted it out." "As a rule its obliteration took the form of innumerable entoptic patterns. At times they were almost as bright as the stimulus itself." "The moving film appeared as a series of waves of darker portion and lighter portion bebetween moving downward and to the right." "The fluctuation was preceded on each occasion by a very definite concentric closure of a black screen, much like the shutter of a camera. It did not appear to be on a plane surface but on a spherical or concave surface. The blackness of the screen was very intense."

Ho. "The disappearance almost always seemed to be a blotting out by way of other visual qualities which covered up the light gray quality."

G. "Through most of the period there were vertical bands or horizontal bands cutting across, sometimes moving and sometimes not, sometimes obscuring the exposure-light."

D. "It disappears in two ways; sometimes suddenly, at other times more slowly, as if washed over by retinal lights."

H. "It seemed as if something moved across the stimulus many times when it went out."

These streaming movements are also confirmed indirectly by the movement of the stimulus-light itself. This is known as Charpentier's illusion or as the autokinetic sensation of Aubert. All *O*s reported this phenomenon on numerous occasions. The movement usually takes a direction intermediate between the horizontal and vertical axes of the field.

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[81] Ferree, this JOURNAL, 19, 1908, 129 ff.

It was suggested by one of the *O*s that the movement of the stimulus-object is apparent because the greater part of the visual field is streaming in the opposite direction.[82] The *O* refers the movement to the smaller object, just as when the moon appears to move behind floating clouds. This state of affairs is implicitly described in the report of W when he says, "The stimulus seemed to be moving in a downward right direction, behind a veil of some opaque material. It almost covered up the stimulus, yet I had the impression that the stimulus was moving." It is significant, too, that the direction of the movement is identical with the most usual directions of the streaming movements found by Ferree.

There is nothing in any of the reports, however, which indicates a connection between eye-movements and the streaming processes; but the *O*s had no instructions to report upon this matter. All reports indicate a connection between the streaming phenomena and disappearance, but we have seen (p. 560) that the periods just before disappearance are peculiarly free from eye-movements. There is a lack of verbal statement indicating a connection between the streaming phenomena and reappearance. This does not, of course, disprove a connection, and on the positive side we have found that eye-movements are most numerous just preceding reappearances. If Ferree's streaming phenomena are manifested by the mists and hazes which infest the field and obliterate the stimulus, there is some evidence for the connection, because these hazes are most numerous when the *O* reports extremely good fixation. Good fixation, then, favors the formation of hazes, and eye-movements may have the effect of clearing the field by producing an acceleration of the streams upon the retina. In this sense Ferree's theory seems to be essentially correct. But this is not the only connection between eye-movements and reappearance, nor is local retinal adaptation *plus* streaming movements adequate to a complete explanation of fluctuations, as we hope the earlier sections have demonstrated.

VI. SUMMARY AND CONCLUSIONS

In this paper we have attempted first of all to set the problem of fluctuations with liminal visual stimuli in its proper historical light; to show that this phenomenon is nothing more than a matter of the limen; to discover the ways in which the phenomenon is dependent upon the intensity of the stimulus; to point out the operation of certain peripheral and central physiological factors; and lastly, to decide the general question whether the phenomenon is to be regarded as a fluctuation of attention. We have been led to the following conclusions.

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[82] We are indebted to Dr. Dallenbach for this suggestion. Further studies in preparation will test this observation.

(1) Fluctuation with liminal visual stimuli is a phenomenon which is connected with all liminal stimuli, as attested by (a) the fact that it agrees with the phi-gamma hypothesis, and (b) the fact that the stimulus value which gives 50% visibility during fluctuations is near the liminal value which is found in the usual manner if we make allowances for local adaptation.

(2) The length of the periods of visibility and invisibility and the total period, are functions of the intensity of the stimulus.

(3) There are a number of physiological conditions, both peripheral and central, involved. Among them are (a) local retinal adaptation, (b) eye-movements, and (c) local central fatigue or inhibition.

(4) Neither from psychophysical evidence nor on the basis of phenomenology can this phenomenon be called a fluctuation of attention.

COLOR SEQUENCES IN THE AFTER-IMAGE OF WHITE LIGHT

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I. INTRODUCTION

The procession of colors which occurs in the after-image of a strong white light has been a source of interest to many observers. The phenomena were described by Helmholtz under the title *Farbiges Abklingen der Nachbilder* and are referred to by English writers as "the flight of colors." An extended resumé of the literature was given by the present writer in the *Psychological Bulletin*,[1] so a review is not given here. It was shown there that descriptions of the phenomena which have been given by various writers have differed considerably. Recent contributions to our knowledge of the subject have been made by Miss Shuey.[2]

The observations reported in this paper were made in an attempt to discover whether there is a relative constancy in the character of the color sequence. The problem grew out of the consideration of the fact that the conditions of observation in previous work had not always been even approximately constant and comparable. In this experiment the attempt was made to control as much as possible the conditions of observations for every *S* in the group.[3]

II. EXPERIMENTAL PROCEDURE

The apparatus used was set up in a room from which all light from external sources was excluded. The essential feature of the apparatus was a light-tight box, suitably wired, having a circular aperture in the front wall. The dimensions of the box were 3 ft. in length, 11 in. in height, 10 in. in width. The whole arrangement was elevated so as to bring the center of the circular aperture on a level with the *S*'s eyes. The aperture had a diameter of 5 cm. The source of light was a 100 watt tungsten lamp placed at a distance of 30 cm. behind the aperture. A plate of milk glass was inserted between the lamp and the plate in order to diffuse the light. With the size of aperture used in this series of observations there was a light surface which was regarded by all the *S*s taking part as homogeneously bright. A curtain was mounted on the top of the box and slightly forward of its front wall. This curtain was released simultaneously with the extinction of the light, and in falling effectually prevented any prolongation of the stimulus during the cooling of the filament. At the rear of the box was a small chamber in which the *E* made a record of the reports of the changes in the image as they were announced by *S*. This was lighted by a small dry-cell lamp, the whole being heavily curtained to prevent any leakage of light.

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[1] W. Berry, The flight of colors in the after-image of a bright light, *Psychol. Bull.*, 19, 1922, 307-337.

[2] A. M. Shuey, The flight of colors, this JOURNAL, 35, 1924, 557-582; The effect of varying periods of adaptation on the flight of colors, *ibid.*, 37, 1926, 528-537.

[3] The observations here reported were completed in the Psychological Laboratory of the University of Chicago, under the direction of Professor Harvey Carr, in 1922. An abstract of the results appeared in the University of Chicago Abstracts of Theses, Humanistic Series, I, 1924. The writer takes this opportunity of thanking Professor Carr for his interest in the work and for his stimulating influence throughout its course. Thanks are also given to the persons whose patience and good will in coöperating with the writer made these observations possible.

The method of procedure in every case was the same. *S* sat in a chair directly facing the light. The distance from the light surface to the eyes was 150 cm. A dark adaptation period of 10 min. was allowed. Shortly before the expiration of the period, and before the light was exposed, a metronome was started, beating 60 to the minute. Every fourth beat a bell was rung. The light was exposed for 60 sec., during which period *S* steadily fixated the center of the light surface. (A constant intensity of stimulus and a constant size of stimulus were used in the observations reported in this paper.) The light was then extinguished and the fall curtain released. This coincided with one of the periodical strokes on the bell and served to mark the beginning of the secondary period, the period of the after-image. *S* then began to report the changes in the after-image, using a system of description previously agreed upon and described in a subsequent paragraph. *E* immediately indicated on record cards the reports made by *S* during each period of 4 sec. This provided a means of measuring the duration of the flight and a record of the qualitative changes which were observed. Only one observation per day was made by any *S*, the observation being made, whenever possible, at the same time each day. All the *S*s were graduate students in the Department of Psychology.

The question of terminology was rather troublesome. All the *S*s were well acquainted with the difficulties inherent in an experiment involving the use of color terms. From the outset the convention of using 'simple' color terms was adopted. These were defined as the usual names given to the well-known divisions of the spectrum, together with such descriptive terms as seemed necessary to complete the report. These were 'dark,' 'bright,' 'saturated,' etc. Compounds such as 'yellow-green,' 'blue-green,' etc., were included. All the *S*s had a practice series of observations, in order to accustom them to the rather fugitive character of the phenomena, and to enable them to acquire some proficiency in reporting the progress of the changes. Twelve *S*s took part, including the writer.

On account of the fugitive character of the image, and the fact that changes took place in two, three, or even four areas of the total image at the same time, the *S*s were instructed to attend strictly to only one of these areas, and to report the changes there. The material presented in this paper is on the flight of colors in the center of the image. This area corresponds to the one described by Homuth[4] as *Bildkern*, only in this instance the form of the colored mass was circular, corresponding to the circular light surface. Fechner,[5] Séguin,[6] and Homuth have called attention to the advisability of this method of procedure in the interest of a clearer and more accurate description of the phenomena.

III. RESULTS

(*1*) *The General Character of the Color Phenomena in the Image.* It is, of course, impossible to present all the data gathered on the characteristics and content of the color sequences for every *S* through a series of observations. Realizing this, I have attempted to represent graphically several typical records of the observations made in this investigation. The data obtained were transferred to a scale and the behavior of the images charted in the following manner. The vertical line represents the order of colors from red at the top to purple at the foot of the line. The horizontal line represents the order of phases in the flight of colors. A 'phase' is defined as either the appearance of another color or a gray (achroma) in the area of the image being observed, or the disappearance of the image altogether. A point on the chart represents the color which was reported in a given phase. Where there was continuity in the color sequence of the image, the points are joined by a straight line. A break in the line represents a phase in which the image was reported "gone." The appearance of a gray (achroma) phase is indicated by the dotted lines joining two points. Several examples of these charts are given on pp.587-8. The first chart, *e.g.* may be read as follows: the first phase reported was green, followed by orange. Here there was a break in the continuity, the image being reported as "gone." This was followed by yellow, another break appearing at that point. The sequence of colors is then given, orange being followed by purple, and so on through the entire flight. It must be clearly understood that this method of representing the course of the image is quite arbitrary. The charts are intended to show only the sequences from phase to phase in the images. There is nothing quantitative in the charts, the lengths of the lines joining the points depending entirely on the arbitrary pattern of the scale adopted. It is believed by the writer that this method has certain advantages over other methods of showing the course of the color changes. The charts, not all of which are reproduced here, show some striking features which will be discussed in the succeeding paragraphs.

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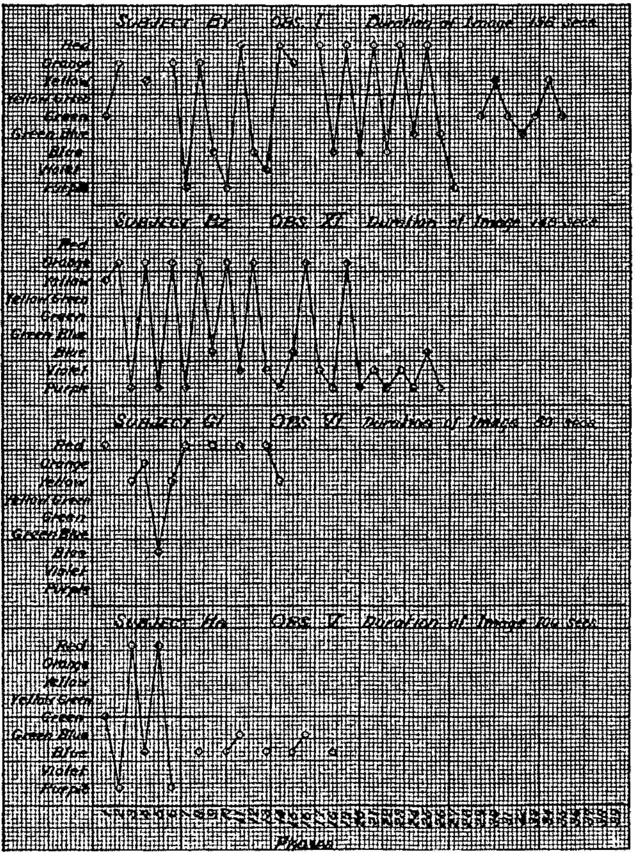
[4] P. Homuth, Beiträge zur Kenntnis der Nachbilderscheinungen, *Arch. f. d. ges. Psychol.*, 26, 1913, 181-268.

[5] G. Th. Fechner, Über die subjektiven Nachbilder und Nebenbilder, *Pogg. Ann.*, 50, 1840, 445-470.

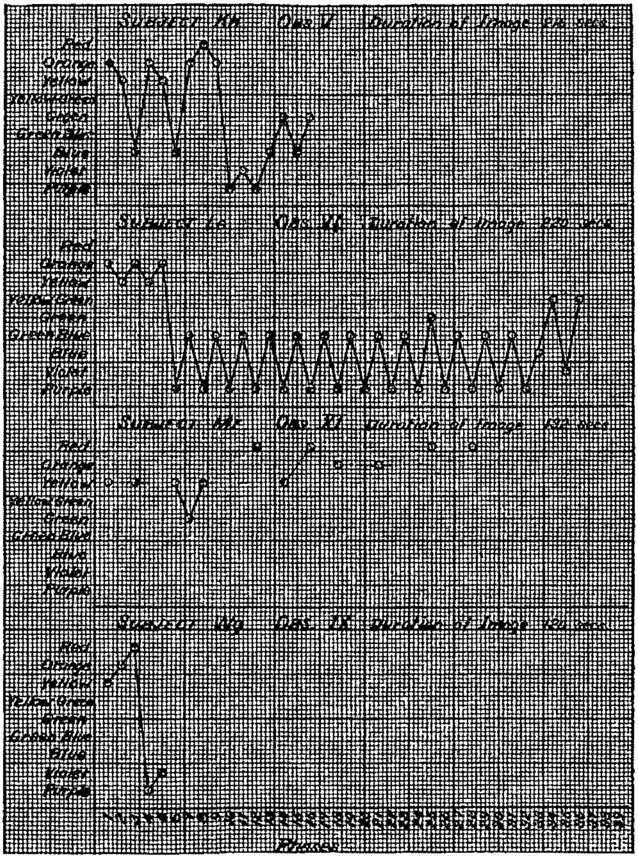
[6] J. M. Séguin, *Ann. de Chimie et de Physique*, Ser. 3, 41, 1854, 413-431.

Taking the results as a whole, all the spectral colors were present in the images, together with purple and gray. The colors which appeared most frequently, irrespective of the places they occupied in the flights, were purple, blue, and red, the colors yellow, orange, and green following in this order, with slight differences in their relative frequencies of appearance. The colors which appeared least frequently were yellow-green and violet. Of the 12 *S*s in this group, two reported all the spectral colors in their observations. This statement does not mean that all the colors were present in any one of the flights. Two *S*s reported only 5 colors in the entire series. Two *S*s reported red as the most frequently seen color in the series, two reported yellow, one reported green, three gave blue, one gave violet, and three gave purple. All the *S*s reported the appearance of yellow, blue, and purple, although with different frequencies. Kr failed to give a single report of red or green. Gl did not report green at all. Mr and Wg gave it once only. Very noticeable individual differences in the frequency of the appearance of gray and the disappearance of the image during the flights were noted. One *S*, Mr, regularly reported the appearance of 'black' in alternation with some colored phase. This was the outstanding case in the entire series of the instability of the image and the regular appearance of non-colored phases. The latter amounted to almost 33% of the total phases reported, while the disappearances of the images amounted to 27%. On the other hand, there were two *S*s whose records show that the images were remarkably stable and free from achromatic phases. These were Le and Wg, who did not give a single report of 'gray,' or 'gone.' These two *S*s showed differences in the number of phases per flight; the former consistently had a relatively large number, the latter a relatively small number of phases. Kr and Kh showed relatively great stability in the images. Gl and Hn had images which were relatively unstable.

Further analysis of the records revealed the fact that the content of the flight varied with the different stages of the image. In all cases the images showed several phases, the number, however, varying with the individual. In order to show how the different stages of the images differed, every record of every observation was divided into 3 parts, irrespective of the number of phases in the flights. The total number of phases in each flight



Charts Showing the Course of the Color Changes in the After-Image of a Bright Light Cetnral Area



CHARTS SHOWING THE COURSE OF THE COLOR CHANGES IN THE AFTER-IMAGE OF A BRIGHT LIGHT CENTRAL AREA

TABLE I

Total Content of the Flights in the After-Images of a Bright Light. Figures Show the Number of Times a Given Color Appeared in the First, Second, and Third Divisions of the Course of the Flights (12 *S*s, 12 Observations Each)

S	Division	Red	Or.	Y.	Y-G.	Gr.	G-B.	Bl.	Vio.	Pur.	Gray	Gone
By	First	23	21	2		17		12	10	8	9	2
	Second	29	2	8		8	11	19	13	6	3	6
	Third			10		49	16	3	1	15	7	24
Bn	First	7	6	3	2	7	4	1	9	9		1
	Second	3	2	2	1	6	6	10	11	2		7
	Third		1	5	7	9	8	3	5		1	13
Bz	First	18	27	6		4	1	5	3	22		
	Second	9	22			5	1	14	14	21		
	Third	1	5			2	2	25	8	37	2	2
Gl	First	9	9	16				6		2	5	11
	Second	18	2	11				3		1	15	8
	Third	26	3	5				1			19	7
Hn	First	18	1	2		9		4		13	2	5
	Second	3		1		1	6	17	1	9	2	14
	Third		1	1			7	20		1	4	20
Kh	First		17	10		1		6	7	7	1	
	Second	1	3			6		15	6	18		
	Third		2			23		15	3	6		
Kr	First		2	12				1		1	2	
	Second		2	3			1	5		1	6	
	Third							3			6	
Le	First	13	24	22	4	8	30	7	1	39		
	Second					2	48	23		75		
	Third			2	19	1	32	39		55		
Mr	First			64		1		10		2	44	28
	Second	6	12	28				1		1	56	45
	Third	18	35	2							50	49
Ml	First	12		10	6	12	6	4	8	11	2	
	Second	5		2	2	16	12	2	4	17	10	4
	Third	6		2	1	11	6	2	1	13	14	8
Rl	First	5	12	3	3	9	1					
	Second	14	5			3	3	4		6	1	2
	Third	4	1	1	1	7	10	10		4		
Wg	First	1	5	12			1	7				
	Second	11	4	1				2		3		
	Third					1		7	1	10		

of colors was ascertained, and the number divided by 3. This method gave a first, second, and third division of the flights, which were examined to determine the content in each division. Table I shows the result of this analysis. The numbers in the table indicate the frequency with which a certain color appeared in each of the three divisions respectively for various individuals. The same data for the group of observers as a whole are given in Table II.

TABLE II

Summary of Table I, Showing the Frequency of the Appearance of Each Color in Each Division of the Course of the After-Images for the Entire Group of *S*s

Division	Red	Or.	Y.	Y-Gr.	Gr.	G-Bl.	Blue	Vio.	Pur.	Gray	Gone
First	106	124	162	15	68	43	63	38	114	65	47
Second	99	54	56	3	47	88	115	49	160	93	86
Third	55	48	28	28	94	81	128	19	141	103	123

From Table I, it is evident that there were some individual differences among the members of the group in respect to the content of the flight in each of the 3 parts of the course of the image. Every *S* seemed to have his or her own individual pattern in the behavior of the colored images, which persisted, moreover, through the entire series. For example, Mr showed a preponderance of the red and orange colors in the final part of the images, and there was a marked predominance of yellow in the first part. On the other hand, the red and orange colors dropped out completely in the second and third parts in the case of Le, and in these divisions the green-blue, blue, and purple colors were the predominant colors present. Another *S*, Bz, showed a marked grouping of the red and orange colors, on the one hand, and the blue and violet colors, on the other, in the first and final divisions respectively.

Table II seems to offer some slight evidence of a group tendency with respect to the behavior of the colors in the different divisions. Taking the combined results, it will be seen that there was a progressive diminution of the frequency of the red, orange, and yellow colors, and an increase in the frequencies of the colors green to purple, excluding violet. In addition, there was also a progressive increase in the frequency of the appearance of 'gray,' usually a very dark gray or black, and evidence of greater instability of the image in the later stages of its course.

TABLE III

Number of Times a Given Color Appeared in the Initial Phase of the After-Images (12 *S*s, 12 Observations Each)

S	Red	Or.	Y.	Y-Gr.	Gr.	Gr.-Bl.	Bl.	Vio.	Pur.
By		2			6				1
Bn		2	1	2	2	1		2	1
Bz	3	3	5		1				
Gl	4	2	3				1		1
Hn		1	1		8		1		1
Kh		4	4		1		2		
Kr			12						
Le		5	3		4				
Mr			7				3		1
Ml			8	4					
Rl		2			9	1			
Wg			4			1	7		
Total	7	21	48	6	31	3	14	2	5

(*2*) *Initial and Terminal Colors.* In most cases the colors of the after-image were seen immediately after the light was extinguished. There were 7 cases in the series where brightness of a very high order was reported in the first phase, this being followed by some color. Table III shows the character of the initial phase in the flight for every individual. The figures indicate the number of times in 12 observations a given color was the first one seen.

It will be seen that only one *S* commenced the flight with the same color through the entire series of observations. The other *S*s showed more or less tendency to vary somewhat the initial color. Bn gives an especially good example of this tendency. Yellow was the most frequently seen first color, with green, orange, blue, red, yellow-green, green-blue, and violet following in that order. In his observations on the after-image, Franz wrote, "From the after-image experiments in the series of anthropometric tests . . ., it was found that the after-image when first seen was sometimes positive and sometimes negative; and the colors varied greatly, being distributed in the first place as follows: negative or dark, 33%; light or white, 29.4%; blue, 13.7%; purple, 9.8%; green, 5.9%; yellow, 3.9%; red, 2.0%; miscellaneous, 2.0%."[7] The individual variability in the flight of colors is brought out rather clearly by both groups of figures. Miss Shuey[8] has recently published some observations on the color phenomena of the after-image. She used 4 different intensities of stimuli, with an exposure of 40 sec. Her results, with the same intensity of stimulus as was used in this experiment, are as follows: "20 of the 55 images began with green, 8 with yellow, 6 with an 'unnamed' light, 6 with yellow-green, 6 with purple, 2 with blue, and 1 with red." Marked variability and individual differences seem to be a characteristic of the first stages of the color sequences of the after-image.

By 'terminal color' of the flight is meant the last color reported in an observation of the center disk or area of the image. Usually this part diminished in size to a very small circle of color seen against a dark, or unsaturated, colored ground. In some cases the final phases of the flight were made up of alternate disappearance and re-appearance of the colored center. Consequently it was decided to define the terminal color as that last seen, even though the *S* was able to report the presence of the image in other terms. One *S* always reported green as the terminal color; another showed a constant tendency to see yellow or yellowish-green last. Two *S*s saw the red and orange colors, or the red, orange, and yellow colors respectively, as terminal colors, and two always reported the blue, violet, and purple colors last.

(*3*) *Cycles of Color.* One of the primary purposes of this investigation was to obtain data on the so-called cycles of colors in the after-image. McDougall has called attention to and has described these features of the flight of colors in some detail. He says, "The two observations last described illustrate a very constant feature of such after-images, namely, the tendency of the three primary colours to follow one another in a recurring cycle of the order G, R, B, G, R, B."[9] He adds, "In the one case the cycle passes from G to G, in the other from B through G, R, B to G, again, thus almost completing the double cycle." The present writer has elsewhere pointed out that, "it is by no means easy to disentangle the facts from the account given by McDougall. Apparently he used different durations, as well as different intensities, but they seem to have been used in an irregular order."[10]

The data gathered in this investigation were carefully scrutinized for the evidences of recurrent cycles of colors in the images. In this report the term 'cycle' means the appearance of two or more colors in a sequence, which sequence is repeated one or more times. A cycle is called a '3-component recurrent cycle' when 3 colors in sequence are repeated one or more times in the same order. In like manner, a '2-component recurrent cycle' has 2 colors in a rhythm, repeated one or more times.

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[7] S. I. Franz, On after-images, *Psychol. Monog.*, 3, 1899, no. 12, 39.

[8] A. M. Shuey, The flight of colors, this JOURNAL, 35, 1924, 569.

[9] W. McDougall, *Mind*, N. S., 10, 1901, 240.

[10] Berry, *op. cit.*, 323.

(*a*) *Three-component recurrent cycles.* Only a few cases of 3-component color cycles were found. Not one example of the recurrent sequence of red, green, blue was reported. There were 2 sequences of purple, red, blue. There was one example each of the following sequences respectively: orange, purple, blue; orange, yellow, blue; and purple, violet, blue. One example was orange, violet, purple, followed by red, blue, purple. These sequences were reported by but 4 *S*s of the 12, 2 of the *S*s giving 2 examples each.

If the total number of the reported phases in the entire series be taken into consideration, it will be seen that there is some justification for saying that these 3-component color cycles are evidently relatively rare. The total number of phases reported was 2543. Of these, 2026 were described by the use of one or other of the color terms. This number would provide at the maximum for 675.3 possible sequences of 3 colors each.

(*b*) *Two-component recurrent cycles.* This type of color cycle is made up of 2 colors in a sequence, which is repeated one or more times during the course of the flight. They will be referred to as the oscillation cycles. Eight of the 12 *S*s reported this type. Two *S*s reported them in each observation throughout the series, one *S* saw them in each observation but one, two *S*s saw them in 6 observations, one *S* saw them in 5, another in 4, and another in 3 observations. Four *S*s did not give definite evidence of the oscillation cycle of colors. Of these 4 *S*s, one showed a definite tendency to see an oscillation between a colored phase and an uncolored phase, or a disappearance of the image. Another *S* reported a large number of 'partial' oscillation cycles, in which the colors were not exactly repeated. One example of this is the sequence red and violet, followed by orange and purple. There were 2 *S*s who reported images in which there was no evidence of a cycle at all. There was a gradual transition from one color to another without repetition of any of the components in the sequence. Kr's records showed the following general changes in the course of the flight: yellow, orange, blue; the flight generally terminating in a dark gray or black. Wg's records showed the following general changes: blue which becomes lighter, yellow, orange which passes into reddish orange, red, reddish purple which becomes darker, passing finally to a dark blue. These two types of flights resemble strongly some of the flights reported in the literature. With these two exceptions, it may be said that the oscillation type of color change in the flight of colors was manifested, either completely or partially, throughout the entire series. There were, however, very marked individual differences in the 'pattern' of the cycles. Homuth has described a type of oscillation in the after-image which resembles somewhat the type found in this investigation. He distinguished two kinds of oscillation: (1) a regular or irregular disappearance of the after-image; (2) a rivalry between colors of the after-image extending over a smaller or larger part of the surface of the image. He says further, "Ich glaube übrigens, dass die erste Form nur ein Sonderfall der Zweiten ist."[11] As has been shown above, there were 3 *S*s in this investigation who reported examples of what might be called the 'disappearing' oscillation cycle.

Some of the marked individual differences in the pattern of the cycle may be summarized as follows:

(1) There were differences in the relative number of the cycles. In some cases the sequence of 2 colors was repeated only once, making one

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[11] Homuth, *op. cit.*, 222.

cycle; whereas in some other cases the sequence was repeated several times, thus completing several cycles. Table IV summarizes the data obtained. The number of cycles, the number of phases in the cycles, the total number of phases in the entire series, and the percent the number of phases in the cycles forms of the total number of phases, are indicated for every *S* who reported this type of color change, through 12 observations.

TABLE IV

S	No. of Cycles of Colors in After-Images	No. of Phases in the Cycles	Total Number of phases in the Images	Percent
By	27.5	110	325	33.8
Bz	27.5	110	256	43.0
Gl	3.0	12	177	6.8
Hn	4.0	16	162	9.9
Kh	14.0	56	147	38.1
Le	92.0	368	444	83.0
Ml	7.5	30	209	14.3
Rl	7.5	30	109	27.5
Total	183.0	732	1829	

TABLE V

S	Colors	No. of Cycles	*S*	Colors	No. of Cycles
By	Red, blue	11.5	Bz	Purple, orange	9.0
	Red, violet	7.0		Blue, purple	9.0
	Green, purple	3.0		Purple, red	4.0
	Yellow, green	2.0		Orange, violet	3.5
	Green, orange	2.0		Green, purple	1.0
	Red, gr-blue	1.0		Purple, violet	1.0
	Yellow, gr-blue	1.0			
Gl	Red, yellow	2.0	Hn	Purple, red	2.0
	Blue, yellow	1.0		Green, red	1.0
				Blue, purple	1.0
Kh	Green, blue	5.0	Le	Gr-blue, purple	54.5
	Purple, blue	4.5		Purple, blue	24.0
	Green, purple	1.5		Blue, y-green	6.0
	Orange, violet	1.0		Orange, yellow	4.5
	Violet, green	1.0		Orange, green	1.0
	Violet, purple	1.0		Gr-blue, y-green	1.0
				Purple, green	1.0
Ml	Purple, green	4.0	Rl	Red, gr-blue	3.0
	Purple, gr-blue	2.5		Orange, red	2.5
	Yellow, red	1.0		Red, purple	1.0
				Orange, yellow	1.0

(2) There were also differences in the character of the components of the cycles as well as in the frequency of their appearance. The distribution of the 183 cycles of the entire series according to the individual reports is indicated in Table V.

There were 22 combinations of colors, and the frequencies ranged from 1 to 54.5 cycles. The most frequent cycle was green-blue and purple, but this, it will be noted, was found principally in the records of one *S*. The pattern of the flights for this *S* was that most nearly constant throughout the entire series. A very clear idea of it may be gained by glancing at the examples of typical flights represented in the chart on page 588. Excluding the records of this *S*, this combination of colors appeared in 2.5 cycles. Similarly, the pair of colors red and blue, appeared in 11.5 cycles, all of them in the records of one *S*. The pairs green and red, and blue and yellow, appeared respectively in only one cycle each.

(3) Although there were some exceptions, there is evidence to show that the oscillations were more numerous during the middle of the flights than at other periods.

(4) *The Duration of the Flight.* The attempt was made to determine the duration of the flight of colors. Although there are always certain difficulties in the measurement of such phenomena as the after-image, it was believed that some useful data could be obtained by the use of a simple method of measurement. This has already been described. To recapitulate, a metronome was started just before the light was exposed. This metronome rang a bell every fourth second, and the extinction of the light coincided with one of these strokes on the bell. The secondary period was measured by the number of periods which elapsed until the image disappeared. It was decided to define the end of the flight as coinciding with the end of the period in which the color was last reported in the observation. The figures are set forth in Table VI with the full knowledge that their value is approximate.

TABLE VI

S	Median	Av. Dev.	Range Upper	 Lower
By	126	14.3	156	112
Bn	118	29.6	200	58
Bz	146	10.0	172	124
Gl	86	20.3	144	60
Hn	96	13.3	140	60
Kh	134	31.1	212	104
Kr	94	20.3	140	52
Le	198	14.3	220	148
Mr	156	27.3	200	120
Ml	112	20.3	184	72
Rl	134	19.3	180	80
Wg	116	28.0	204	80

In several cases the after-image persisted as a colorless disk for some time after the colors had disappeared. Taking the figures as they stand, it will be seen that there is considerable variability. Sometimes an *S* would report a flight of long duration, which would be followed by one of short duration, which reverted to one of longer duration. Miles found 3 types of *S*s in his experiments: one in which there was very little variation from test to test, another in which the *S*s began with long durations and steadily decreased them, and a third type in which there were at first no images, then images with a gradually increasing duration.[12] Our data do not indicate the presence of these types among our *S*s. Whatever variability was present, existed throughout the series for each *S*. In her recent article, Miss Shuey reports that the average duration of the images was 3 min. 35 sec. (215 sec.).[13]

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[12] G. H. Miles, The formation of projected visual images by intermittent retinal stimulation, *Brit. J. Psychol.*, 8, 1915, 93-126.

[13] Shuey, *op. cit.*, 565.

It has already been pointed out that the number of phases in the images varied with the individual series of observations. It was desirable to determine whether or not the number of phases was in any way a function of the duration of the image. In order to show the relation between these two features, Table VII was prepared. Here the medians of the number of phases, the average deviation from the median, and the range (upper and lower) are given.

TABLE VII
Total Number of Phases, Medians, Average Deviations, and Ranges of the Numbers (12 *S*s, 12 Observations Each)

S	Total Number of Phases	Median	Av. Dev.	Range Upper	Range Lower
By	325	26.5	3.2	38	20
Bn	151	13.0	2.0	16	8
Bz	256	20.0	2.3	30	17
Gl	177	12.0	6.4	28	7
Hn	162	13.0	2.3	18	8
Kh	147	12.0	3.1	21	6
Kr	45	4.0	.9	6	2
Le	444	37.5	3.8	43	23
Mr	452	36.0	6.1	55	28
Ml	209	17.0	2.6	25	11
Rl	109	8.0	2.6	15	5
Wg	66	5.5	1.1	8	4

The rank correlation between the medians in Table VI and Table VII is .62.

In the first place, the correlation between the medians is fairly close. In the main, those *S*s who regularly had images which lasted for a relatively long time, reported the largest number of phases. In the second place it will be seen that there is some evidence of the relative consistency of reports from every *S*. The two largest average deviations are 6.4 and 6.1 respectively. These, it must be noted, are for those *S*s (Gl and Mr) whose images were regularly unstable and vacillating. There is little or no evidence that the members of this group tended to approach group uniformity in this respect.

(5) *The Relative Rapidity of the Phase Changes.* Fechner and others have pointed out that the colors in the flight tend to appear and pass rapidly at the outset, but to become more stable as the image progresses. "Im Allgemeinen dauert, wie schon früher bemerkt, jede spätere Phase länger als die vorhergehende."[14] In the main, the results of these observations confirm those of previous observers. Taking each record on the basis of its duration in time, and dividing this by 4, it was possible to ascertain the number of phases in each quarter of the flight. Five of the *S*s showed evidence of a decrease in the number from the first through to the last quarter. The rate of decrease was unequal from quarter to quarter, and varied with the individual. One *S* showed a slight increase from the first through the second, third, and fourth quarters. Two *S*s showed a slight decrease from the first to the second, an increase to the third, followed by a slight drop to the fourth quarter. Two others had more rapid phase changes in the second and third quarters than in the first and fourth. One showed a gradual decline through the first three quarters and a slight increase in the fourth, while another showed a decline to the second and a gradual in-

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[14] Fechner, *op. cit.*, 449.

crease through the third and fourth. Two *S*s (Kh and Wg) regularly reported long stretches of one color (green and blue respectively). There was not a single instance of the prolonged occupation of the center of the image by the color red.

IV. SUMMARY OF RESULTS

(1) The after-image of a bright light contains areas which are variously colored and which exhibit color changes in the course of the duration of the image. In this investigation, only one of these areas was made the object of a series of observations to determine the characteristics of the color changes. This was the central area, corresponding in form with that of the primary stimulus.

(2) There were marked individual differences in the procession of colors in this area of the image. This conclusion holds with respect to the specific colors in the sequences, and also to the relative frequency of their appearance.

(3) As the image waned, the character of the flight of colors (procession of colors) changed. The changes were not, however, approximately identical for all *S*s. Colors which tended to disappear from the later stages of the sequences in the images in the observations of some of the *S*s, tended to increase in the frequency of their appearance in the corresponding stages of the images in the observations of other *S*s.

(4) Only a few cases of 3-component recurrent color cycles were observed in this investigation. There were, however, numerous cases of 2-component recurrent color cycles. This type was called the oscillation cycle, from the fact that there seemed to be a rhythm of oscillation between two colors in the procession of colors. Within the limits of this investigation, and in the cases where this type was manifested, there was evidence of a relative constancy in the pattern of the cycle or rhythm for the individual *S* reporting them. There seemed to be, however, different patterns for different *S*s.

(5) The duration of the flight of colors varied somewhat for different *S*s. The number of the phases (changes within the procession or flight of colors) also varied considerably. There was a positive correlation between the duration of, and the number of phases in, the image.

(6) In the main, the colors changed more rapidly in the initial stages of the image, and later tended to change slowly.

THE PROPERTIES OF SPACE AND TIME IN KINAESTHETIC FIELDS OF FORCE*

By C. O. WEBER, Wells College

I. THE PROBLEM AND ITS IMPORT

"Whatever can be measured is real," exclaimed Max Planck; and his own triumphant success in proving the existence of quanta of electrical energy well justifies the statement. Yet there is another criterion of the real in science whose present difficulties rival the success of the principle Planck announced. This second criterion may be expressed in the phrase, "the real is the unchanging." Heretofore, this principle found perfect embodiment in the law of conservation, which Millikan regards as the 'cornerstone' of all scientific construction. It also found typical expression in the Newtonian view (which is now the traditional view of all mankind) that space and time are absolute 'reals' because they are fixed and invariable, because a unit of time and of space is always and everywhere the same. Time and space remain always and everywhere the same because *we assume that they are unaffected by the presence of physical masses or conscious observers.* Now, Einstein and his school of physical relativists assume that space and time are affected by the presence of bodies, *i.e.* gravitational fields, with the consequence that space and time cease to be absolutes. Yet, true to the instinctive human belief that the real is the unchanging, these physicists find a new absolute, namely, the velocity of light, which is the same for all observers.

From the principle that the properties of space and time depend on the presence of fields of force there flows that list of strange properties of space and time which common sense, schooled in the Newtonian tradition, finds so bizarre. Euclidean space now becomes 'real' only where gravitational influence does not reach, *i.e.* nowhere! Real space is always non-Euclidean. In a strong field of gravity space is contracted ('puckered'), time intervals are shortened, straight lines become curved, and parallel lines and degrees of angle obey new laws.[1] But reflection will show that if we consider the *kinaesthetic* perception of space, these alleged properties of space and time at once become plausible. To begin with, our muscles always perceive space *as affected by gravity*—their own weight invariably insures this result. Where there is intensified gravity, there is intensified 'effort,' and space should take on 'local characteristics.' We anticipate that phenomenally, Einstein's world is one in which we may feel 'kinaesthetically at home,' so to speak.

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*Read at the 35th annual meeting of the American Psychological Association, Philadelphia, December 30, 1926.

[1] See H. Poincaré, *Science and Hypothesis*, 1905, 29-32.

Accordingly, a new field of investigation is open to psychology, devoted to the study of the effect of fields of force on our perception of space and time. The two following experiments are a demonstration of the fact that the new physical theories of time and space permit of a psychological approach. The first is an experiment on the reproduction of distances; the second an experiment on the reproduction of time intervals.

II. THE REPRODUCTION OF DISTANCES UNDER LOAD

To determine the effects of effort[2] on the properties of space I had 5 *S*s reproduce a fixed distance of 25 cm., but under varying load. The chief instrument employed for this purpose was the kinaesthesiometer devised by Michotte.[3]

This instrument is perhaps little known in this country, but it is admirable for studying forearm movements. A horizontal beam, housed in a ball-bearing boxing, moves over a curved, graduated scale. This metal scale is placed at just that distance from the center of rotation of the beam, at which its units are at once degrees of arc and metric units. The horizontal beam carries the forearm of the subject, and movements can be made away from and towards the body. A heavy metal frame, provided with leveling screws, forms the base of the instrument. (This instrument can also be used as an ergograph.) The starting point of the movements can be altered by the use of a set screw, thus enabling the experimenter to eliminate the sense of position as a criterion. Leuba took this precaution in his experiments because he was interested in the sensory criteria involved in arm movements. We did not eliminate the criterion of initial position, nor others, such as the sound of the instruments, habits of counting, and tapping movements on the part of the *S*s. We hoped to demonstrate that all *S*s make systematic errors due to load, despite any criterion or aid used by them except vision itself.

To this apparatus I added a sliding carriage, which moved along the metal scale, and could be variously loaded. When the moving beam, carrying the forearm, moved along the scale, the carriage was smoothly pushed before it. The energy required to move it was the energy required to overcome the friction between the metal scale and the metal carriage. The law of friction involved is simple: the friction between two constant surfaces is directly proportional to the force pressing them together. Thus, to double the load on the carriage results in doubling the resistance it offers.

A Dunlap chronoscope, run from a rectifier, was so attached that the time of all movements was automatically measured. To accomplish this, electrical contacts were so attached to the carriage that at the instant the subject moved the beam against the carriage the chronoscope was set going, and stopped only when the movement of the carriage was stopped. When *S* returned the beam to the initial position, the carriage was left stationary on the scale, thus registering the distance to which *S* moved.

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[2] I employ the term 'effort' because of the brevity with which it implies the mental accompaniment of load.

[3] Sold by Leon Roland-Cordier; 30 Rue de Ligne, Héverlé-Louvain. In principle, it is like Leuba's device described in this JOURNAL, 20, 1909, 370-373.

It must be noted, therefore, that *S* reproduced only the movement away from the body with the right forearm. The *S*s quickly adapted themselves to this *Aufgabe*, would visibly relax at the end of the out-going movement, and return in idle fashion to the point of origin.

Every *S* made 50 reproductions of the constant distance of 25°, for each of four weights: 100, 150, 200, and 250 grm. respectively. But of each set of 50 reproductions, 25 were made with an empty carriage after sensing the standard distance with the carriage loaded (the 'weight first' or W. F. series). Conversely, the remaining 25 comparison movements were made with 'weight last.' We thus got 1000 judgments in all. Chance order was followed so far as the various weights were concerned, the same weight being used not more than twice in succession. However, when working with a given weight we invariably performed with it a pair of judgments in the following time order: standard loaded, comparison unloaded; then standard unloaded, comparison loaded. Thus throughout a sitting the carriage was alternately loaded and unloaded. We thus sought to eliminate contrast effect by eliminating the *expectation* to which it is generally ascribed. The distance errors made by our *S*s (see below) fit the theory of error due to contrast, yet they could not have been due to defeated expectation. If by any chance we loaded both standard and comparison, our *S*s almost invariably showed by sign or word their awareness of broken routine.

I present below two tables which give for every *S* the average distances and durations of standard and comparison movements. Table I contains the data obtained when the standard was loaded, and Table II the data for the 'weight last' series. Distances are recorded in centimeters, and the durations in sigma. The standard distance, always 25 cm., is not given. The items under D are the comparison distances. Under T′ are the average durations of the standard movements, and under T″ the average durations of the comparison movements.

The above results show that when distances are reproduced under load, all *S*s tend to give a distance shorter than the unloaded standard, and this systematic error is proportional to the weight used. The duration of these movements (executed incidentally since the *S* has her attention on the distances) becomes longer the greater the load. If now we consider the results when the standard is loaded (Table I) and the comparison movement unloaded, the space errors become positive. The greater the load of the standard movement, the greater is the plus error of the *S* when she tries to reproduce it. However, the durations behave as they did in the previous case. That is, despite the fact that the carriage is unloaded, *S* takes more time for making a comparison movement after 'sensing' a distance under load. Although all of my *S*s were wholly untrained, their results are (with several exceptions at local points) uniformly alike.

One significant feature of the above results must be noted. We have pointed out the rule that the heavier the load on the standard movement, the greater is the plus error of the comparison movement that follows. This rule is definitely broken in Table I, in the average (for all *S*s) of the 250 grm. distance column. The average distance for this weight (26.66) is practic-

TABLE I

The Effects of Load on the Extents and Durations of Arm-Movements, Standard Loaded and Comparison Unloaded

S	100 Grm.			150 Grm.			200 Grm.			250 Grm.		
	D	T′	T″	D	T′	T″	D	T′	T″	D	T′	T″
Ba	30.59	650	573	31.02	665	555	31.84	695	570	31.06	720	606
Bo	25.11	956	793	25.62	1033	851	25.58	1060	845	25.05	1120	866
Ma	25.10	795	920	25.28	898	961	25.58	893	948	25.76	928	961
Mo	26.93	693	580	26.61	715	585	26.16	725	583	26.29	755	555
Tu	25.53	746	700	25.54	801	716	25.29	813	766	25.15	768	743
Av.	26.65	768	713	26.82	822	734	26.89	837	742	26.66	858	746

TABLE II

The Effect of Load on the Extents and Durations of Arm-Movements, Standard Unloaded, Comparison Loaded

S	100 Grm.			150 Grm.			300 Grm.			250 Grm.		
	D	T′	T″	D	T′	T″	D	T′	T″	D	T′	T″
Ba	29.63	565	616	29.31	588	616	28.88	603	646	28.34	563	670
Bo	24.32	898	920	23.56	928	1010	23.48	896	1001	23.23	883	1043
Ma	24.95	770	985	24.50	798	1026	24.43	803	1058	24.67	843	1058
Mo	25.38	623	621	25.48	613	645	25.38	661	733	25.40	620	768
Tu	24.80	675	815	24.74	683	831	24.83	700	860	24.85	598	871
Av.	25.82	706	791	25.52	722	826	25.40	733	860	25.29	821	882

ally identical with the equivalent figure for the 100 grm. weight (26.65). The writer believes that this is no accident. In an experiment with 7 *S*s one year ago, with this same apparatus, we found that a load of 300 grm. was sufficient to make all *S*s reverse their errors of distance entirely, carrying the 300 grm. load farther than the standard and unloaded movement. This reversal of error occurs at different intensities of load for different *S*s.

III. THE REPRODUCTION OF DURATIONS UNDER LOAD

With this same apparatus I now carried out an experiment on the reproductions of a time interval of 550σ.

The procedure was as follows. *S* remained passive while 'sensing' (by hearing) this time interval, marked out by the clicks of a metronome. Then *S* sought to reproduce a time interval equal to the standard, by means of two other clicks, which she had to produce, however, by moving the carriage. *S* was thus compelled to reproduce a time interval under muscular strain or effort, which effort began and ended promptly with the reproduced time interval. *E* recorded the actual time which *S* gave as equivalent to the standard time, and also the incidental distances to which *S* moved the carriage. Five loads were used: 50, 100, 150, 200 and 250 grm. respectively. Twenty-five trials were made with every weight for every *S*. I employed 8 *S*s, thus giving 1000 measures in all. I present the results in Table III below. Distances (D) and durations (T) again occur in terms of centimeters and sigma, and both pertain to the *S*'s reproductions.

In general, these results again show that the presence of load causes the reproduced durations to be too long, in proportion to the size of the load used. The incidental distances, on the contrary, become shorter with increasing weight. In the average results, there is only one exception to the rule that increasing load results in decreasing distance, and that occurs with the 200 grm. weight. This exception, however, is found in the individual results of 7 out of the 8 *S*s used. An explanation may perhaps be found in the fact that this weight while moving set the carriage into a slight tremor—which was not noted and corrected until late in the experiments.

Other tentative experiments of mine point to the same central fact that fields of force produce 'local characteristics' in our perception of space and time. In one tentative experiment (now under reinvestigation) I found indications that *S*s reproducing a standard triangle with an iron stylus over an electromagnet make their triangles too small (as measured by use of the planimeter) when the magnetic field is weak, but too large when the field is strong. This is in harmony with the results of arm-movements when a very heavy load is used. In other tentative experiments I found that *S*s, thinking that they were drawing straight lines with an iron stylus over a magnetic field, were really drawing arcs of ellipses. When the field was weak, these elliptical arcs were drawn with the 'concave' side facing the magnetic field. However, when the field was strong, and *S* was clearly conscious of the pull of the magnet, the elliptical arc which she produced for a 'straight line' presented its convex side to the magnet, so that the magnet would fall outside of the area of the ellipse we should get if we completed the arc. This looks like a case where *S* seeks to compensate for the

TABLE III

The Effect of Load on the Reproductions of a Time Interval of 550 σ.

S	50 Grm.		100 Grm.		150 Grm.		200 Grm.		250 Grm.	
	D	T	D	T	D	T	D	T	D	T
Bo	21.50	881	19.70	873	19.02	1015	19.19	1068	18.37	1176
Cl	18.43	946	17.00	1003	16.84	998	16.90	1038	17.26	1085
Ep	23.23	610	22.25	625	20.00	636	21.00	630	21.00	708
La	16.86	888	16.62	1066	16.75	1093	16.34	1123	16.25	1163
Mo	22.74	1121	20.53	1156	20.85	1221	21.52	1228	18.97	1294
Sh	24.79	631	24.50	705	24.35	696	24.76	741	23.35	730
Sp	16.46	826	17.80	933	18.08	951	18.90	953	17.02	1061
We	19.50	451	17.66	616	17.58	728	16.62	773	16.41	868
Av.	20.44	794	19.51	873	19.18	917	19.40	944	18.58	1016

TABLE IV

Percent of V.E. for Extents and Durations of Comparison Movements, Standard Loaded, Comparison Unloaded

S	100 Grm.		150 Grm.		200 Grm.		250 Grm.	
	D	T″	D	T″	D	T″	D	T″
Ba	8.2	21.2	7.3	14.9	8.2	21.5	9.3	22.3
Bo	4.8	8.8	5.1	9.3	4.4	11.5	2.4	8.0
Ma	6.1	12.3	4.3	13.3	6.1	11.3	6.2	10.7
Mo	5.5	14.9	5.2	19.3	4.5	21.7	7.4	18.7
Tu	5.7	20.9	4.9	15.4	3.7	12.4	4.8	13.7
Av.	6.06	15.62	5.36	14.4	5.38	15.68	6.02	14.68

pull of the magnet, but overdoes it. We are now engaged in a careful retrial of this experiment also, and the new results may be quite different.

IV. EXTENTS AND DURATIONS AS MUTUAL CRITERIA IN ARM-MOVEMENTS

What is the underlying mechanism of these errors, and what are the sensory criteria upon which we ordinarily depend in reproducing distances and durations? Here we touch upon many vexed controversies in the history of psychology. The question regarding the sensory criteria involved has been subject to the closest scrutiny by others. We secured introspections from our *S*s in the form of answers to questions. Because our *S*s were wholly untrained, we do not care to offer these data for consideration, except to mention (with the greatest diffidence) several points elicited. The *S*s were always surprised to learn that the standard remained the same distance. A standard distance executed under load seemed longer to them. They testified to a marked consciousness of strain with the 250-grm. weight. Perhaps the reversal of error characteristic of the heavy weights is marked by the advent of this consciousness of effort. Most of the *S*s confessed to almost complete inattention to the experiment towards its end; but, curiously, this was the time when the typical errors which my tables exhibit were most nearly constant. When reproducing extents, most of the *S*s mentioned the tendency to image the distance which they were not allowed to see. In reproducing durations, there were impulses to count, to make rhythmic movements with the idle hand or the feet, etc. For adequate studies of the sensory criteria involved, I refer the reader especially to the references below and their bibliographies.[4] In this paper, I confine myself to the point upon which my data particularly bear. This is the contention of Kramer and Moskiewicz,[5] Jaensch,[6] Leuba,[7] and others, that judgments of extent rest on the judgments of duration which accompany movements. Woodworth[8] and Hollingworth[9] dispute this result. Hollingworth concludes, from an extended experiment, that the perception of duration cannot be the basis upon which we judge distances. This is evident, for one thing, from the fact that the variable error for the durations of movements is greater than the variable error for the extents of these same movements. Judgments of extent, therefore, cannot rest on data more variable in error than they are themselves. My own computations of variable error for the arm-movements experiment agree with Hollingworth's finding that the errors (percent of variable error) are always greater for the durations than for the extents. The tables below give the average percent of V.E. for every *S*.

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[4] A most adequate criticism of the view that sensations alone can account for space perception is W. McDougall's analysis (*Outline of Psychology*, 1923, 235-264). Cf. also E. B. Titchener, *Textbook of Psychology*, 1909, 160-182; J. Jastrow, *Mind*, 1886, 539-554; H. D. Cook, Tactual and kinaesthetic space, *Psychol. Bull.*, 12, 1915, 214-216; W. W. Costin, The influence of the variation of weight upon the judgment of extent, *Psychol. Mon.*, 10, 1909, 38-54; W. B. Pillsbury, Does the sensation of movement originate in the joint, this JOURNAL, 12, 1900, 346-353; L. B. Hoisington, On the non-visual perception of the length of lifted rods, this JOURNAL, 31, 1920, 114-146; F. Kennedy, *Psychol. Rev.*, 7, 1900, 305-309; H. K. Wolfe, Some judgments on the size of familiar objects, this JOURNAL, 9, 1897, 138-166; R. S. Woodworth, On the voluntary control of the force of movement, *Psychol. Rev.*, 8, 1901, 350-359; H. Pioncaré, *Foundations of Science*, 1921, 415 ff.; M. F. Washburn, *Movement and Mental Imagery*, 1916. Washburn's book contains analyses which are indispensable to a proper understanding of the above results, especially as regards the reversal of errors characteristic of large load.

[5] F. Kramer and G. Moskiewicz, Beiträge zur Lehre von den Lage- und Bewegungsempfindungen, *Zsch. f. Psychol.*, 25, 1901, 101-125.

[6] E. R. Jaensch, Über die Beziehungen von Zeitschätzung and Bewegungsempfindung, *ibid.*, 41, 1906, 257-279.

[7] J. H. Leuba, An apparatus for the study of kinaesthetic space perception, this JOURNAL, 20, 1909, 374-385.

TABLE V

Percent of V.E. for Extents and Durations of Comparison Movements, Standard Unloaded, Comparison loaded

	100 Grm.		150 Grm.		200 Grm.		250 Grm.	
S	D	T″	D	T″	D	T″	D	T″
Ba	5.3	21.1	7.2	19.5	5.9	21.3	6.0	19.1
Bo	3.6	7.8	5.6	13.5	3.2	7.4	4.8	8.6
Ma	4.1	12.6	4.1	11.8	3.4	9.4	6.2	15.3
Mo	5.8	17.8	5.9	17.7	4.5	16.2	4.0	23.0
Tu	5.0	19.8	3.4	13.2	4.3	11.2	3.2	12.0
Av.	4.76	15.82	5:24	15.14	4.26	13.10	4.84	15.60

In Tables IV and V, there is not a single instance in which the percent of V.E. for duration is not greater than the corresponding measure for extent. However, in computing these measures for the second experiment, where the *S* attended to durations and only incidentally reproduced extents, the results are different. In the individual results, the percent of V.E. is sometime lower for the extents, and sometimes lower for the durations. However, the average results for all *S*s show a constantly lower V.E. for the durations as compared with the extents, although only slightly lower. In Table VI we give these average results for all *S*s.

TABLE VI

Percent of V.E. for Extents and Durations when Extents are 'Incidentally' Executed: Averages for 8 *S*s

50 Grm.		100 Grm.		150 Grm.		200 Grm.		250 Grm.	
D	T	D	T	D	T	D	T	D	T
14.2	13.0	14.0	12.0	12.2	11.0	14.6	11.4	13.7	11.8

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[8] R. S. Woodworth, *Le Mouvement*, Ch. iv, 1900.

[9] H. L. Hollingworth, The inaccuracy of movement, *Arch. Psychol.*, No. 13, 1909.

But the fact remains that our *S*s (see Tables I and II) tend to reproduce the time of the standard movement, even when the comparison movement is unloaded. The V.E., after all, exhibits only the minor variations of extents and durations; but the 'bulk' of the time taken for the standard movements tends to reappear in the comparison movements, except for the disturbing effect of load. Hollingworth too obtained positive correlations between the times and the extents of movement;[10] a fact in harmony with Leuba's finding that when an *S* calls two distances 'equal,' he has executed them in the same time.[11]

We are inclined to think that the criteria which determine the quantity of the comparison movement are many, and that the time of the standard movement is one of them. Our experiments show that kinaesthetic strain or 'effort' is another important factor, as is also the velocity of the standard movement. In fact, Louba mentions velocity as an apparent criterion, and notes, in an experiment involving muscle resistance, that there is a gradual reduction of velocity with increased resistance. This is completely verified by our results. If we divide the average time of the standard movements by their average extents, we get velocities whose constancy is greater throughout the entire experiment than that of any other figure. But even velocities show systematic errors. Increase of load invariably reduces velocity; and there is also another loss of velocity which is characteristic of the comparison movements, irrespective of load. Perhaps *S* reproduces *work*, judging the extent of a distance by the work required in traversing it.[12] If so, then the products of load times distance should be constant. Inspection of the crude data leads us to expect this, since as the load becomes greater the distances become smaller, so that the products of the two should approach constancy. Unhappily, this constant fails to appear. I do not think that my data are an adequate test either of the hypothesis that *S* reproduces *work* or of the hypothesis that she reproduces *power* (work divided by time). The 'set up' of my apparatus renders it uncertain whether the energy required to move the carriage is proportional to the weight used. This is because of the curved trajectory, because of lateral friction, and of the general crudeness of the carriage. So far as the factors which determine the judgments of space and time are concerned, I am sure that they are not wholly sensory, but that we must recognize the fundamental principle insisted upon by McDougall, that a definite 'perceptual thinking' is involved in all judgments of space and time.[13]

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[10] H. L. Hollingworth, Experimental studies in judgment, *Arch. Psychol.*, No. 29, 1913.
[11] *Op. cit.*, 377.
[12] C. O. Weber, The psycho-genesis of space, *Monist*, 32, 1922, 449 ff.
[13] W. McDougall, *Outlines of Psychology*, 1923, Ch. VIII.

Conclusions

Perhaps our general results will gain in significance if we orient them with reference to the problem of a phenomenal geometry. The field of psychology, we believe, must fundamentally remain the field of *phenomenology* from the subjective point of view. Initially, the data of the physicist and the psychologist are to a great extent identical: it is the point of view which differentiates them. The psychology here referred to, fundamentally Wundtian (and perhaps neo-Realistic as well), finds scientific 'objectivity,' not in the popular sense of an object independent of mind, which exists irrespective of its qualities. To mimic physics, the 'mother of sciences,' by finding a psychological object like a physical object is the basic error of behaviorism. Psychological data are here regarded as objective, not through being independent of all observers but through being independent of any particular observer.

Our experiments suggest the possibility of discovering a phenomenal geometry whose qualitative characteristics are objective in the above sense as independent of a particular observer, *i.e.* are the same for all observers in the same situation. The interest of the physicists themselves in such a geometry is growing.[14] We may enumerate the chief properties of such a geometry, so far as kinaesthetic space is concerned, as follows:

(1) The phenomenal geometry of the kinaesthetic sense is a geometry of space-time.

(2) Its space and time are radically altered by the presence of 'fields of force' or load. (a) A given distance under load is phenomenally equivalent to a greater distance under less load. (b) A given time interval under load is phenomenally equivalent to a smaller time interval (as measured by clock-time) under less load.

(3) The above effects of load are directly proportional to the load used; but at a critical point, increase in load tends to reverse the space errors.

(4) There are tentative results indicating that load equally effects the perception of area, and the phenomenally straight line, and therefore perhaps also degree of angle.

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[14] There is the well-known interest of Henri Poincaré, of Whitehead, Broad, and other English realists. Note Jean Nicod's *La geometrie dans le monde sensible*, Paris, 1924, especially the introduction to it by Bertrand Russell. Cf. H. Poincaré, *Dernières pensées*, 1924, ch. 2.

THE PLEASANTNESS OF BRIGHTNESS COMBINATIONS*

By JOHN T. METCALF, University of Vermont

I. PROBLEM

The pleasantness of color combinations has been made the subject of considerable experimental study. Relatively little attention, however, has been given to the question of possible differences in the affective character of different brightness combinations. The investigation to be reported here is a study of aesthetic reactions to certain colorless stimulus-objects, each one consisting of two shades of different intensity.

Historical. As far as the writer has been able to determine the only direct experimental attack upon this problem hitherto has been made by Cohn.[1] In the course of a comprehensive study of the pleasantness of single colors, brightnesses, and their combinations, this investigator devoted a section of his work to a study of the pleasantness of brightness combinations. Two color-mixers were used, on each of which were mounted two pairs of black and white disks, a large pair and a small. The disks were set each time to give the desired brightnesses in each of the two combinations which the subject was to compare with each other. The method of paired comparisons was used, 6 *S*s taking part in one series and 4 in another. Cohn reaches the general conclusion that the pleasantness of a combination varies with the difference in brightness between the components. The combination of black and white was preferred the greatest number of times, and other combinations were preferred in proportion to the difference in brightness between their component shades. The single exception to this rule appeared in the case of one *S* who was color-blind. When single shades were compared with each other the only clear result was a preference for white.

While Cohn's investigation was in progress Major was working on a somewhat similar problem.[2] Cohn's article appeared first, and Major therefore makes, in his published report, a careful comparison between his own results and those of Cohn. In so far as they bear upon our present problem the results of the two investigators differ widely. Major's stimuli consisted of square pieces of Bradley papers, exposed singly against each of two backgrounds, black and white. Every *S* rated the affective value of each stimulus on a scale of 7 steps. Essentially, then, this is a study of the pleasantness of single shades. However, as each shade appears against a background of a different shade (black or white), and as it is found that the brightness of the background affects the judgment, the results should throw light on the question of the pleasantness of combinations. As regards the pleasantness of shades and their combinations, Major's results differ from Cohn's in two chief respects. First, there is no trace of the preference of white to gray, except an equivocal one in the results obtained from one *S*. Second, black is not pleasanter on the white background than any of the grays, except that two *S*s rate 'green-gray' lower than black.

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*Read in part before the American Psychological Association at the Madison, Wisconsin, meeting, December, 1923.

[1] J. Cohn, Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten und ihrer Combinationen, *Philos. Stud.*, 10, 1894, 562-603.

[2] D. R. Major, On the affective tone of simple sense-impressions, this JOURNAL, 7, 1895, 57-77.

II. Material and Method

In our own experiments the stimulus-objects were made up of square pieces of white, gray, and black paper. Every one consisted of a 3-in. square of one shade, in the center of which was mounted a 1-in. square of a different shade. For convenience in handling, the larger square was mounted on a cardboard square of exactly equal size. Five intensities of the brightness scale were used: white, black, and 3 grays, light, medium, and dark. All possible combinations of these 5 shades were used, each shade serving once as center and once as background. This gave a set of 20 stimulus-objects. The first set made up proved to be unsatisfactory for several reasons. It was made in the spring of 1923 from black, gray, and white papers then on hand in the laboratory. The various shades differed from each other in the texture of the paper; some of them were tinged with color, and the three grays were far from the intensities desired. The results of the preliminary experiments made with these stimuli are therefore not included in this report.

The second set of stimuli was made up from black, white, and gray Hering papers obtained from C. H. Stoelting Co. Grays from the 9.5 x 18 cm. sample books were used, as, in the case of the ight gray and the dark, these were found to be more satisfactory than the similarly numbered shades in the large sheets. Titchener has called attention to the defects of this series of shades, pointing out that some of the grays are tinged with color, and that some vary from standard brightness.[3] The three grays used in this experiment (Nos. 3, 9, and 36) were fairly colorless. None of the *S*s mentioned the presence of any color as some of the *S*s in the preliminary series with the first set of stimuli had done.

The brightness of the grays was determined by matching them with rotating black and white discs cut from the black and white papers that were used, along with the three grays, in making up the stimulus-objects. It was desired to obtain an equal-step series of brightnesses from black (B) to white (W), with the medium gray (M) just half-way between the extremes, the dark gray (D) just half-way between B and M, and the light gray (L) just half-way between M and W. The three grays were chosen to meet these requirements by two *O*s working under the same conditions of illumination as obtained in the experiment itself. The field against which they were chosen was white. After they had been so chosen the brightness of the grays was determined. In terms of degrees of white in the color-wheel, the measurements are as follows: L=230°, M=105°, D=40°. The brightnesses of the stimulus-objects were measured again after the experiment had been completed to make sure that the grays had not faded or otherwise changed in the course of the investigation.

In the preliminary series the experiments were carried on by diffuse daylight. This, however, varied so much in intensity, even within a single experimental session, that it was decided to work by artificial light, shutting out all daylight from the room and using a 'daylight' lamp. A 100-watt National C-2 Mazda bulb was placed 1.85 m. directly above the horizontal field on which the stimuli were shown. Rich, on the basis of color equations worked out by this form of illumination and also by natural daylight, concluded "that the Mazda lamp, although dependable through the middle range of the spectrum, gives a light which is markedly deficient in blue, rays as compared with daylight."[4] Since in our experiment we are concerned with color only in the effort to prevent its appearance in the stimuli this deficiency in blue rays is not a serious matter.

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[3] E. B. Titchener, The Hering grey papers, this Journal, 25, 1914, 297-298.

[4] G. J. Rich, The daylight Mazda lamp in the psychological laboratory, this Journal, 30, 1919, 315.

Before beginning an experiment the experimenter always allowed about 5 minutes for the *S*'s eyes to become adapted to the illumination.

The field against which the stimulus-cards were displayed was white for Series I, medium gray for Series II, and black for Series III. These fields consisted of large sheets of paper 15 x 20 in., the same as that used for the corresponding shades in the stimuli themselves. A vertical screen, matching the field, hid the stimulus-cards which were not being displayed, and *E*'s records from the *S*'s view.

Throughout this report the large sheet of paper on which the stimulus-cards were shown will be called 'field.' The term 'background' will refer to the 3-in. square which forms the background of the stimulus-card. The term 'center' will refer to the 1-in. square which forms the center of the stimulus-card. For convenience the stimulus-cards were numbered, after being arranged in order from light to dark. First they were arranged into five groups according to the brightness of the background, and then the four cards of each group were arranged according to the brightness of the center. The arrangement and numbering is shown in Table I which gives the numbers of the cards and the shade of background and center in each case.

TABLE I

Showing for every Card the Shade of the Background and the Shade of the Center

Card No.	Backgr.	Center	Card No.	Backgr.	Center	Card No.	Backgr.	Center
1	W	L	9	M	W	17	B	W
2	W	M	10	M	L	18	B	L
3	W	D	11	M	D	19	B	M
4	W	B	12	M	B	20	B	D
5	L	W	13	D	W			
6	L	M	14	D	L			
7	L	D	15	D	M			
8	L	B	16	D	B			

The method employed was that of paired comparisons. Every card in the series of 20 was treated as a single color is treated in the usual color-preference experiment. *E* placed the two cards to be compared side by side, about an inch apart, in the middle of the horizontal field, and removed them as soon as *S* had given his response. Sometimes two sessions were required to compare every card with every other one (190 judgments). More often, however, it was possible to complete the experiment in one session. The *S* was always questioned in the course of an experimental session as to his state of fatigue, and, if his reply indicated that he was at all tired, he was allowed a brief period of rest. The writer acted as *E* throughout.

The *S*s were told that they would be shown a number of cards, two at a time, and that each time they were to indicate which of the two was pleasanter. In case both were unpleasant, they were to indicate the less unpleasant of the two. In case there was no difference in pleasantness between the two, they were to call them equal. They were cautioned against making the judgment on other grounds than the pleasantness or unpleasantness of the two cards actually before them. These instructions were not given *S* in a set formula, but *E* explained to each *S* what he was to do, and satisfied himself that *S* understood before beginning the experiment.

It was natural that some 'equal' judgments should appear. The differences in the affective quality of brightness combinations are probably much smaller than similar differences in the field of color combinations. Experience showed, however, that *S* was usually able to make his judgment readily and easily in favor of one card or the other. Though all of the 24 *S*s were instructed to respond with 'equal' in case there was no difference in the pleasantness or unpleasantness of the two cards, only 9 *S*s gave 'equal' judgments. The total number of such judgments, 76, is less than 2% of the 4560 judgments made in the experiment as a whole. When 'equal' judgments did appear, they were treated in the usual way by crediting a half point to each of the cards involved.

In Series I 12 *S*s took part. Ten of these were students in the writer's classes at the University of Vermont. The other two were both college graduates. One of the student *S*s was red-green color-blind. Six *S*s took part in Series II, and six in Series III, all of them students. In each of the three series half of the *S*s were men and half were women. *E* noted down any comments made by the *S*s which had a bearing upon the preferences they had expressed, but did not try to elicit such comments by questions until the end of the experiment. After the 190 comparisons had been completed, however, *E* picked out certain of the cards which had been preferred a large or a small number of times and asked *S* to look at them and state, if possible, why he found them pleasant or unpleasant. *S* was also encouraged at this time to make any general statements or comments that he could. For the most part the *S*s were not sufficiently trained in psychological methods to be able to give very accurate introspective reports, though some of their comments taken in connection with the objective records of their preferences proved illuminating.

III. RESULTS

Table II gives the complete results for the three series. The numbers in the vertical columns of this table show how many times a given *S* preferred each one of the different cards. Totals and averages are given for each series separately and for the three series combined. The averages are also presented in graphic form in Figs. 1 and 2. In these figures the numbers of the cards appear along the base-line, and the height of a curve above a card's number indicates the average number of preferences for that card. In Fig. 1 the continuous line represents the averages for the three series combined, the averages being obtained by dividing the total number of preferences for a given card by 24—the number of *S*s. Since Series I includes 12 subjects and Series II and III but 6 each, Series I (W field) has double weight. If the averages for the separate series are averaged, Series I has no more weight than the other two. This procedure was also followed, and the resulting averages are represented in Fig. 1 by the dotted line. It will be observed that the two curves follow each other fairly closely, but that the dotted line lies slightly above the other for the first half of its course, and below it for the second half.

In Fig. 2 the averages for the three series are represented by the three lines. Series I (W field) is represented by the dotted line, Series II (M field) by the dashed line, and Series III (B field) by the continuous line. The three curves may be said to follow roughly the same general course. Their differences reveal an influence of field upon the pleasantness of the different cards. It will be remembered that the cards are grouped according to brightness of background, cards 1–4 having W backgrounds, cards 5–8 having L backgrounds, and so on. If the curves of Fig. 2 are studied, it appears that there is a general tendency for a card to be less preferred when its background matches the field, and more preferred when its background differs from the field. Thus, at the start where cards 1–4 have white backgrounds, the curve for the white field is lowest, and those for the gray and black fields higher. At the finish, where cards 17–20 have black backgrounds, the opposite arrangement of curves is found. This

TABLE II

Showing for every *S* the Number of Preferences for every Stimulus-Card

Stimulus Cards	Series I														Series II								Series III								Series I, II, III	
	SUBJECTS												Total	Av.	SUBJECTS						To-tal	Av.	SUBJECTS						To-tal	Av.	Total	Av.
	1	2	3	4	5	6	7	8	9	10	11	12			13	14	15	16	17	18			19	20	21	22	23	24				
1	5	4	4	12	11	13	6	2	1	2	1	15.5	76.5	6.4	6	8	12	9	16	19	70.0	11.7	19	4	14	12	1	5	55.0	9.2	201.5	8.4
2	9	6	9	9.5	11	5	2	12	4	7	5	13	92.5	7.7	9	13	13	6	16.5	16	73.5	12.3	15	3.5	13.5	14	13	12	71.0	11.9	237.0	9.9
3	11	6	7	9.5	7	4	4	12	6	7	7	5	85.5	7.1	9	8	11	4	16.5	6	54.5	9.1	14	2.5	14	9	16	9	64.5	10.8	204.5	8.5
4	13	3	9	6.5	2	1	5	11	4	2	1.5	.5	58.5	4.9	5	2	11.5	1	17	3	39.5	6.6	15	2	17	9.5	14	13	70.5	11.8	168.5	7.0
5	13	7	3	12.5	8	12	5	6	6	6	7	16.5	102.0	8.5	4	7	12.5	9	5.5	15	53.0	8.8	16	13.5	11	12	4	2	58.5	9.8	213.5	8.9
6	10	12	13	16.5	16	17	18	18	4	13	14	18	169.5	14.1	13	18	14.5	14	13	14	87.5	14.6	16	13.5	12	18	7	16	82.5	13.8	339.5	14.1
7	9	14	8	14.5	15	6	10	14	8	11	13	10	132.5	11.0	19	18	15	11	9.5	6	78.5	13.1	13	10.5	9.5	14	13	17	77.0	12.8	288.0	12.0
8	11	10	12	9.5	5	9	13	15	12	6	8.5	3	114.0	9.5	17	11	15	6	15	8	72.0	12.0	12	4.5	11.5	10.5	11	17	66.5	11.1	252.5	10.5
9	11	12	9	15	14	7	6	5	11	10	14	13.5	127.5	10.6	6	15	11	9	12	13	66.0	11.0	10	13.5	15	16	10	12	76.5	12.8	270.0	11.3
10	7	15	13	17	14	16	17	5	9	16	13	17	159.0	13.3	10	16	10.5	13	6.5	14	70.0	11.7	12	18.5	13.5	18	9	15	86.0	14.3	315.0	13.1
11	4	18	14	11.5	16	13	15	9	6.5	16	19	16	158.0	13.2	15	16	8	17	3	9	68.0	11.3	7	14.5	11.5	14	7	16	70.0	11.7	296.0	12.3
12	10	13	15	10.5	11	10	11	18	13	17	15	7.5	151.0	12.6	15	13	11	13	9	4	65.0	10.8	4	11.5	10.5	12	9	15	62.0	10.3	278.0	11.6
13	15	11	7	12	6	5	2	5	15	5	15	6	104.0	8.7	8	7	1	3	9.5	12	40.5	6.8	8	6.5	13	5	13	8	53.5	8.9	198.0	8.3
14	7	14	13	13	13	11	8	6	15	7	10	9	126.0	10.5	10	10	4	8	6.5	9	47.5	7.9	7	13.5	8	8	10	9	55.5	9.3	229.0	9.5
15	5	17	15	8.5	16	15	14	5	11	11	16	14	147.5	12.3	9	7	5	17	3.5	8	49.5	8.3	4	15	4	8	3	10	44.0	7.3	241.0	10.0
16	2	6	7	1	10	13	10	14	9	16	8	10	106.0	8.8	9	5	3	16	5	2	35.5	5.9	0	9.5	1	3	5	3	21.5	3.6	163.0	6.8
17	19	6	10	3	3	0	9	7	9	1	5.5	.5	73.0	6.8	0	3	3	2	15	10	33.0	5.5	6	1.5	6	3	19	2	37.5	6.3	143.5	6.0
18	14	6	8	4	1	5	10	6	18	9	6	2.5	89.5	7.6	6	6	10	5	7.5	11	45.5	7.6	8	10	3	1	15	4	41.0	6.8	176.0	7.3
19	11	6	8	3	7	14	14	12	15	13	9.5	4.5	117.0	9.7	13	6	8.5	11	6.5	6	51.0	8.5	3	10.5	1.5	3	9	3	30.0	5.0	198.0	8.2
20	4	4	6	1	4	14	11	8	13.5	15	2	8	90.5	7.5	6	1	10.5	16	1.5	5	40.0	6.7	1	11.5	.5	0	2	2	17.0	2.8	147.5	6.1

explains the course of the two curves in Fig. 1. The curve representing the averages in which white field has doubled weight lies below the other where the backgrounds are lighter, and above it where the backgrounds are darker.

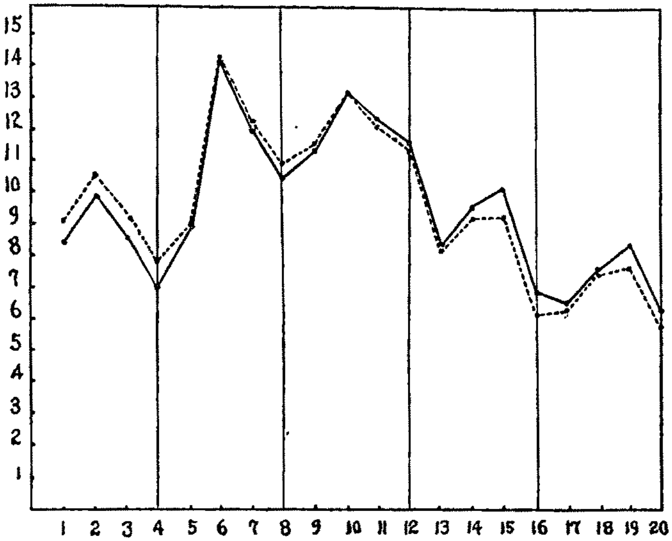

FIGURE 1

TABLE III

Showing the Comparative Pleasantness of the Different Cards

Rank	Card	No.	Shade of		Order in Series		
Order	No.	Prefs.	Backgr.	Center	I	II	III
1	6	339.5	L	M	1	1	2
2	10	315.0	M	L	2	5.5	1
3	11	296.0	M	D	3	7	7
4	7	288.0	L	D	6	2	3.5
5	12	278.0	M	B	4	9	10
6	9	270.0	M	W	7	8	3.5
7	8	252.5	L	B	10	4	8
8	15	241.0	D	M	5	13	15
9	2	237.0	W	M	14	3	5
10	14	229.0	D	L	8	14	12
11	5	213.5	L	W	13	11	11
12	3	204.5	W	D	17	10	9
13	1	201.5	W	L	19	5.5	13
14.5	13	198.0	D	W	12	16	14
14.5	19	198.0	B	M	9	12	18
16	18	176.0	B	L	15	15	16
17	4	168.5	W	B	20	18	6
18	16	163.0	D	B	11	19	19
19	20	147.5	B	D	16	17	20
20	17	143.5	B	W	18	20	17

For the purpose of studying the comparative pleasantness of the different cards, Table III has been made up from the results presented in Table II. Here the 20 cards are arranged in order from top to bottom according to the number of times each was preferred in the experiment as a whole. The card number is given in the second column, and the number of times it was preferred in the third. The next two columns indicate respectively the shade of the background and of the center of each card, and the remaining three columns give every card's rank order in the separate series.

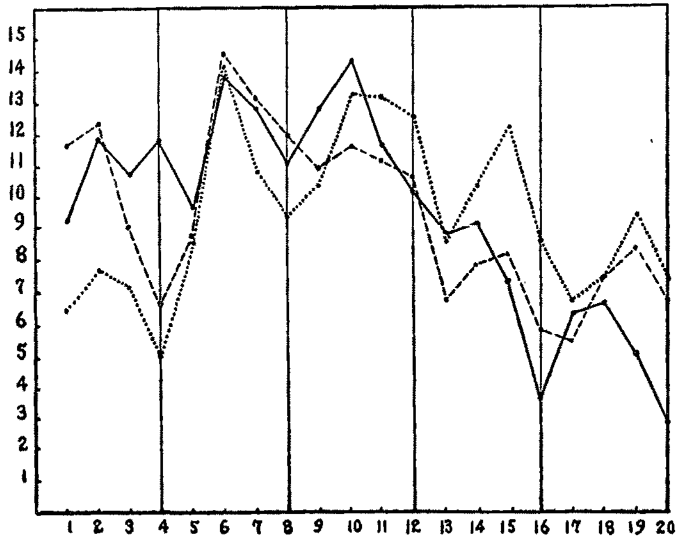

FIGURE 2

IV. Analysis and Discussion of Results

The card most frequently preferred, No. 6, was chosen 2.4 times as often as the one preferred the smallest number of times, No. 17. If each card had been preferred an equal number of times, or if every judgment had been an 'equal' judgment, each one of the cards would have 4560/20 = 228 preferences. Our card No. 14, which ranks 10th in the series of 20, was actually preferred 229 times.

When the three series are compared with each other, the degree of difference that is revealed between the pleasantness of the different combinations is about the same for each series. The curves of Fig. 2 show this, for they follow in general much the same course, and while they reach their high and low points in somewhat different places, the degrees of difference represented are quite comparable. The cards having the highest and lowest average number of preferences for the three series differ from each other as follows:

	Highest	Lowest
Series I	14.1	4.9
" II	14.6	5.5
" III	14.3	2.8

The low points show greater differences between the separate series than the high, the chief variation being in the low point for Series III. Here card No. 20 (D on B) receives on the average only 2.8 preferences. This card rates low in each of the other series too (Ser. I, 7.5, Ser. II, 6.7), but in Series III it reaches a point lower than that reached by any card in either of the other two. Black was in general the shade least preferred, and it seemed to be especially unpleasant when combined with dark gray. Table III shows that the two cards made up of B and D rate respectively 18th and 19th in the experiment as a whole, while Table II shows that card No. 16 (B an D) has an average of only 3.6 preferences in Series III. A further factor in determining the extreme unpleasantness of card No. 20 in Series III is the fact that its background matches the black field used in this series, and, as was shown above, cards tend to rate low when their backgrounds match the field.

An examination of the fourth column of Table III, giving the shades of background and center for the 20 cards arranged in order of preference, shows that the cards with gray backgrounds tend to find their places near the top. Of the first 10 cards, all but one have gray backgrounds, while of the last 10 all but three have backgrounds of black or white. With centers there is the same tendency, though not as clearly marked. Of the first 10 seven have centers of one or another of the three shades of gray, while of the last 10 five have gray centers. Six of the 20 cards consist of a gray background combined with a gray center of a different shade. All six of these are to be found among the first 10, four of them being the first four cards in the column.

This preference for gray might be due, at least in part, to the fact that in the experiment as a whole three-fourths of the results were obtained with white or black fields, and only one-fourth with a gray field. It is necessary, therefore, to see whether such a preference holds in Series II, in which the medium gray field was used. Table III shows that of the first 10 cards in order of preference for that series, seven have gray backgrounds and seven have gray centers, while of the last 10 cards, five have gray backgrounds and five have gray centers. Of the six cards which are combinations of two shades of gray, four fall in the first 10 (two of these being first and second in order), and two fall in the second 10. Thus it appears that the preference for cards having gray backgrounds or centers or both holds in Series II with the gray field, though not to as great a degree as in the experiment as a whole. The cards which consist of combinations of black and white rate very low, both in Series II, and in the experiment as a whole. There are two such cards, No. 4 (B on W) and No. 17 (W on B). Table III shows that in the experiment as a whole No. 4 rates 17th, and No. 17 rates 20th. No. 17 rates very low in each of the three series. No. 4 rates low in Series I and II, but in Series III (B field) it rates 6th.

(*a*) *The effect of field.* Table II and Fig. 2 show the differences in the number of times the 20 cards were preferred in Series I, II, and III respectively. In Fig. 3 the curves of Fig. 2 are condensed by combining the results for cards having backgrounds of the same shade. Thus, there are five points on the base-line, the first one, W, for the general average of the four cards having W backgrounds (Nos. 1–4), the second one, L, for the cards having L backgrounds (Nos. 5–8), and so on. In this figure as in Fig. 2 the dotted line represents the results of Series I (W field), the dashed line the results of Series II (M field), and the continuous line those of Series III (B field).

It has already been noted that a field which contrasts with its background enhances the pleasantness of a card. This appears clearly in Fig. 3. The greater the difference in brightness between a given background shade and the field on which it appears, the higher is the average number of preferences, represented by the height of the curve. Where the background shade is equally different in brightness from each of two fields the curves almost coincide. This situation occurs in three places: (1) For background L (cards 5–8), which is equally different in brightness from field W and field M; (2) for background M (cards 9–12), equally different from W and B; and (3) for background D, equally different from M and B. The rule does not hold in the first of these three instances, for if it did the curves for Series I (W field) and Series II (M field) should coincide at a point below that reached by the curve for Series III (B field). The curve for Series I is low, but that for Series II is high, even a little above that for Series III. The reason for this exception to the rule, resulting in a high point in the curve representing cards with a L background on a field of M, seems to be a very special liking for the combination of L and M. Table III shows that the card most often preferred is No. 6 which has L for background and M for center, and that the one second in order is No. 10 which has M for background and L for center. In the other two instances the rule holds perfectly.

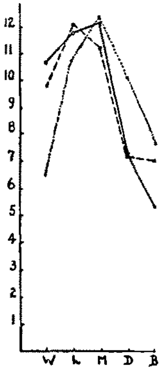

FIG. 3

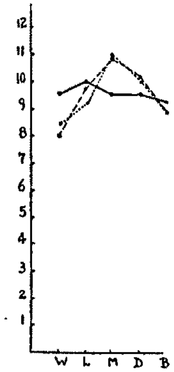

FIG. 4

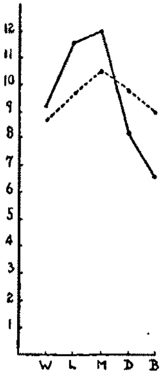

FIG. 5

The effect of the brightness of the field in determining the pleasantness of the card is not large as compared with the effect of the relationship between the two shades which form the background and center of the card itself. This may be seen from the curves of Fig. 2. These curves show less variation from each other in height at any given point than each one shows in its own height throughout its course. The widest variations of the curves from each other occur toward the ends where the white and black backgrounds are represented and where, consequently, the widest contrasts between background and field occur.

In general, then, it appears that the pleasantness of the combination depends to some extent upon the brightness relationship between its background and the field against which it is shown. The pleasantness of the combination tends to vary with the degree of difference in brightness between background and field.

Fig. 4 combines the results for cards having centers of the same shade as Fig. 3 does for those having identical backgrounds. It will be seen at once, when these two figures are compared, that the centers play a much smaller part in determining the pleasantness of the different cards than do the backgrounds, for the curves of Fig. 4 vary much less in their course than do those of Fig. 3. This is shown more clearly in Fig. 5 in which the two curves combine respectively the three curves of Fig. 3 and the three of Fig. 4. The curves for the white and gray fields in Fig. 4 follow almost identical courses, while the black curve differs from them by running almost level. Thus there is no effect between field and center comparable to that between field and background. The black field seems to have a tendency to minimize the effect of the shade of the center and to emphasize the effect of the shade of the background. In Fig. 4 the black curve runs almost level, while in Fig. 3 it covers a greater vertical distance than either of the others.

(*b*) *Effect of relationship between center and background.* Some of the cards present combinations of shades one step apart in our brightness scale (*e.g.* W+L, D+B, M+D, etc.), others present combinations of shades two steps apart (*e.g.* W+M, L+D, etc.), still others are combinations of shades which lie three steps apart (*e.g.* W+D, L+B, etc.), and finally two of the combinations (W+B, B+W) have component shades which lie four steps apart. The cards presenting these different combinations are the following:

1-step combinations	1, 5, 6, 10, 11, 15, 16, 20	(8)
2-step "	2, 7, 9, 12, 14, 19	(6)
3-step "	3, 8, 13, 18	(4)
4-step "	4, 17	(2)

The difference in the number of cards in which each of the different intervals is represented is, of course, due to the way in which the series was made up. One-step combinations can be made all along the line; 4-step combinations can be made only by combining the two extreme shades of white and black. In a series of 20 cards, therefore, 1-step combinations appear 8 times, and 4-step combinations only twice. This difference in number should be kept in mind in comparing the results for the different step intervals. Table IV gives the average number of preferences in the experiment as a whole for the different cards arranged according to interval. This table is derived from the last column of Table II.

TABLE IV

Showing Average Number of Preferences for the Different 'Steps'

Card No.	1-step	Card No.	2-step	Card No.	3-step	Card No.	4-step
1	8.4	2	9.9	3	8.5	4	7.0
5	8.9	7	12.0	8	10.5	17	6.0
6	14.1	9	11.3	13	8.3		
10	13.1	12	11.6	18	7.3		
11	12.3	14	9.5				
15	10.0	19	8.2				
16	6.8						
20	6.1						
Av.	10.0		10.4		8.7		6.5

The averages of this table would seem to indicate that 2-step and 1-step combinations are preferred in general to 3-step ones, and these in turn to 4-step ones. It will be seen, however, from an examination of the table that there is considerable variation in degree of preference for the different cards of a given step-group. Especially is this true in the case of the 1-step combinations. Card No. 6 has an average number of preferences of 14.1, while card No. 20 has only 6.1. Thus, within this step the range of variation is practically as great as that for the table as a whole, for 14.1 is the highest value, and the only one lower than 6.1 is the 6.0 of the 4-step column. In the 1-step column the first two and the last two values are low;

the other four are high. Cards 1 and 5, the first two, both have white in the combination; cards 16 and 20 both have black. The four remaining cards are all combinations of two shades of gray. The average for the four having white or black in the combination is 7.6, that for the four gray combinations 12.4. Thus it appears that 1-step combinations rate high in pleasantness when they occur toward the center of the brightness scale, but low when they occur toward the ends of this scale. In the 2-step column the values show much smaller differences from each other, and such differences as occur do not seem to be due to the presence or absence in the combination of black or white. Only two of the six cards are combinations made up of two grays, No. 7 (D on L) and No. 14 (L on D). The remaining four involve black or white. The average for the two all-gray combinations is 10.8, that for the four others 10.3. All of the 3-step combinations necessarily involve black or white, and the two 4-step ones are, of course, combinations of black with white. In general, then, it seems that the combinations most preferred are 1-step combinations consisting of two grays. These are followed in order by 2-step combinations, 3-step combinations, 1-step combinations in which black or white appears, and finally 4-step combinations.

The results for the separate series are given in Table V in order to determine what effect, if any, brightness of field had upon the pleasantness of the different step-groups.

TABLE V

Showing Effect of Field Brightness upon the Different 'Steps'

	1-step (Grey)	2-step (Grey)	2-step (B *or* W)	3-step	1-step (B *or* W)	4-step
Series I (Field W)	13.2	10.8	10.2	8.2	7.8	5.9
" II (" M)	11.5	10.5	10.7	8.9	8.3	6.1
" III (" B)	11.8	11.1	10.0	9.4	6.9	9.1
" I, II, III.	12.4	10.8	10.3	8.7	7.6	6.5

In Table V the 2-step combinations as well as the 1-step ones are divided into those which are combinations of two grays and those which have in them black or white. The table shows differences between the different series in a given step group, but these are for the most part small. In only two places do they result in a change in the regular order as between step groups. The first of these is in Series II, where the gray field results in the 2-step gray combinations falling slightly below the 2-step B or W ones. The second is in Series III, where the black field has raised the relative value of the 4-step combinations (because of the effect upon card No. 4 noted above, p. 614) so that these rank higher than the 1-step B or W. Otherwise the table shows the same order for the separate series as for the combined result.

(*c*) *Comparative pleasantness of light center on dark background and dark center on light background.* Of the 20 stimulus-cards 10 have centers which are darker than their backgrounds and 10 have centers which are lighter than their backgrounds, each of the 5 shades having been used once as center and once as background in combination with each of the 4 others. The question naturally arises as to whether there is any preference for one arrangement or the other. Table VI gives for each of the 24 *S*s the average number of preferences for cards in which the center is darker than its background and those in which the center is lighter than its background. The cards in which the first arrangement obtains are Nos. 1, 2, 3, 4, 6, 7, 8, 11, 12, and 16; and the cards which show the second arrangement are Nos. 5, 9, 10, 13, 14, 15, 17, 18, 19, and 20.

It will be seen from Table VI that in Series I with the white field 6 *S*s show a preference for lighter centers, 5 for darker ones, and the remaining *S* (*S* 10) shows no preference either way. The averages for the series as a whole show 9.5 preferences for each arrangement. In the case of all except 2 *S*s (*S*s 8 and 9) the differences are not very marked. The results of this series, therefore, show no preference for either arrangement over the other. In Series II and Series III a preference is shown for a darker center on a lighter background. In each series 5 of the 6 *S*s show such a preference, and the general averages are: Series II, darker center 10.8, lighter center 8.2; Series III, darker center 10.7, lighter center 8.3. The averages for all three series combined are: darker center 10.1, lighter center 8.9. In general, then, there appears to be a slight preference for darker centers on lighter backgrounds.

(*d*) *Pleasantness of combinations as related to pleasantness of single shades.* In a study of the affective tone of color-combinations, Geissler concludes that "the greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination."[5] In order to determine whether preferences for combinations of shades are related to any preferences *S* may have for the component single shades, a special experiment was carried out.

Every *S*, after the experiment with the 20 cards had been completed, was given five slips of paper each of which represented one of the shades used in making up the combinations, and was asked to arrange them in the order in which he preferred them. These slips were ½ x 1½ in. in size, and were cut from the five different papers used in making up the stimulus-cards. The conditions of lighting and field were the same as had obtained in the experiment with the combinations. Table VII gives the results of this experiment with single shades for the 24 *S*s of the three series. In the column under each *S*'s number, the number opposite the letter indicating the shade of the slip shows the position of that slip in that *S*'s arrangement. Thus, in the case of *S* 1, the black slip was placed first as most preferred of the five, the dark gray one was second, and so on. The rank orders are totalled and averaged for the separate series, and again for the three series combined.

In Series I, M is the shade most often placed first, and the average rank order is 1.8. L and D have average rank orders of 2.7 and 2.8 respectively, and W and B average rank orders of 3.9 and 3.8. Thus with these slips, representing shades chosen as 5 equal steps from W to B, M ranks first, L and D come with almost equal values next, and W and B with almost exactly equal values last. The darker fields of Series II and III result in an increase in the preference for W and L, and a decrease in that for D and B. The preference for M is very slightly reduced.

If the results just presented are compared with those given in Table III and with those represented by the different figures, it will be seen that the preference for M as a single shade is borne out by a preference for combinations in which M appears as one of the components. The columns of the table and the curves of the figures also reflect the intermediate positions of L and D, and the position at the bottom, as least preferred shades, of B and W. Thus, the pleasantness of one or both of the single shades of a combination would seem to be an important factor in determining the pleasantness of the combination.

An attempt was made to analyze this relationship more closely. Calculations were made separately for every one of the 24 *S*s in the following manner. The *S*'s arrangement of the five slips was taken, and from it was calculated the rank order of the 20 stimulus-cards as this would have been

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[5] L. R. Geissler, The affective tone of color-combinations, *Titchener Commemorative Volume*, 1917, 162.

TABLE VI
Showing the Average Number of Preferences for Cards with Centers Lighter and Darker than the Background

Centers in Relation to Background	SERIES I													SERIES II							SERIES III						
	SUBJECTS													SUBJECTS							SUBJECTS						
	1	2	3	4	5	6	7	8	9	10	11	12	Av.	13	14	15	16	17	18	Av.	19	20	21	22	23	24	Av.
Lighter	10.6	9.9	9.2	8.9	8.6	9.9	9.6	6.5	12.3	9.5	9.8	9.2	9.5	7.2	7.8	7.6	9.3	7.4	10.3	8.2	7.5	11.4	7.6	7.4	9.4	6.7	8.3
Darker	8.4	9.1	9.8	10.1	10.4	9.1	9.4	12.5	6.7	9.5	9.2	9.8	9.5	11.8	11.2	11.4	9.7	11.6	8.7	10.8	11.5	7.6	11.4	11.6	9.6	12.3	10.7

TABLE VII
Showing Order of Preference for the Different Shades Used in the Experiment

	SERIES I														SERIES II								SERIES III								SERIES I, II, III	
Shades	SUBJECTS												Total	Av.	SUBJECTS						Total	Av.	SUBJECTS						Total	Av.	Grand Total	Grand Av.
	1	2	3	4	5	6	7	8	9	10	11	12			13	14	15	16	17	18			19	20	21	22	23	24				
W	5	4	4	2	4	3	4	5	4	5	5	2	47	3.9	4	3	3	4	2	1	17	2.8	1	5	3	3	1	3	16	2.7	80	3.3
L	4	2	2	1	2	2	3	4	5	3	3	1	32	2.7	2	2	2	3	1	2	12	2.0	2	4	1	2	2	1	12	2.0	56	2.3
M	3	1	1	3	1	1	1	3	2	1	1	3	21	1.8	1	1	1	1	5	3	12	2.0	3	1	2	1	3	2	12	2.0	45	1.9
D	2	3	3	4	3	4	5	1	1	2	2	4	34	2.8	3	4	5	2	4	4	22	3.7	4	2	4	4	4	4	22	3.7	78	3.3
B	1	5	5	5	5	5	2	2	3	4	4	5	46	3.8	5	5	4	5	3	5	27	4.5	5	3	5	5	5	5	28	4.6	101	4.2

if it had followed the rank order of the separate slips, that is, if the combinations had been ranked according to the sum of the pleasantness of the separate shades which composed them. For example, *S* No. 2 arranged the slips as follows: M L D W B, this arrangement being the most typical one. M was therefore counted as 1, L as 2, and so on. Card No. 1 consists of a W background and L center, or W+L. Substituting for the letters their numerical rank orders as determined by the arrangement of the slips, we have $4+2=6$ for card No. 1. Card No. 2 is W+M, or $4+1=5$. In this way values for the other cards of the 20 were calculated, and the same procedure was followed with the results of each of the other *S*s. The rank orders of the cards according to this calculation were then correlated with their rank orders as determined by the number of preferences given by *S* to each in the experiment. The formula used was $r=1-6\Sigma d^2/n(n^2-1)$.

TABLE VIII

Correlation between Actual and Calculated Rank Orders

Series I				Series II		Series III	
S	r	*S*	r	*S*	r	*S*	r
1	−.26	7	.56	13	.53	19	.83
2	.77	8	.32	14	.76	20	.64
3	.51	9	.29	15	.45	21	.41
4	.81	10	.82	16	.60	22	.81
5	.90	11	.74	17	.42	23	.00
6	.45	12	.55	18	.82	24	.40
Av. = .54				Av. = .60		Av. = .52	

Av. r, for Series I, II, III, = .55

This was done in order to get an indication of how closely *S*'s preferences for the combinations followed his liking for the separate shades of the combination. It should be recalled in this connection that background plays a more important part than center in determining *S*'s preference for a combination. This appeared in the curves of Fig. 5, and was discussed above. In the calculations just described both shades of a combination were weighted equally regardless of the fact just mentioned. If more weight had been given to the background shade the correlation would doubtlessly have been more close, but this was not done because of the lack of an exact measure of how much more important the background was than the center in determining the value of the combination.

The results of the calculations just described are presented in Table VIII, which gives opposite the number of each subject under r the coefficient of correlation between the actual and the calculated rank orders.

These r's range all the way from −.26 to +.90, but all except 4 of the 24 are +.40 or above, and 11 are +.60 or above. In the cases in which r is −.26 and .00 the *S*s, Nos. 1 and 23, were determined in their preferences solely by the degree of contrast presented. The greater the contrast the pleasanter the combination. When it was a case of arranging the single slips in order of preference, the degree of contrast between the slip and the field determined its position. Thus there was no question of preference for single shades determining preference for combinations, since in the combinations it was the degree of contrast that determined preference, and the pleasantness of the single shades themselves was relative to the background on which they appeared. *S* 17 (r=.42) and *S* 21 (r=.41) also showed a strong liking for the sharp black and white contrasts, though degree of contrast is not the only factor in determining preference, since Table II shows that some of the other combinations rate high, and Table VII shows that degree of contrast did not wholly determine the arrangement by these *S*s of the single slips. Three other *S*s have r's of less than .45.

These are *S*s 8, 9, and 24. Table VI shows that these 3 *S*s differ from the others in that they show marked preferences for the arrangement light-center-on-dark-background or for the reverse arrangement. So in the case of these *S*s too, the brightness relationship between center and background plays more of a part in determining the preference for a combination than does the pleasantness of the single shades. With the other *S*s the results seem to show that the pleasantness of the single shades is an important factor in determining the pleasantness of the combination.

(*e*) *Comments of Ss on the pleasantness of combinations.* Although, as stated above, the *S*s were not sufficiently trained in psychological methods to be able to give accurate introspective reports, their comments and explanations are of some interest when taken in connection with the objective results.

As the results of Table V show, the type of combination most preferred is a 1-step combination consisting of two grays (M+L or M+D). The comments of the *S*s on these combinations indicate two chief reasons for their pleasantness: (1) The degree of contrast is great enough to make the two shades definitely and clearly different from each other, without being so sharp as to be unpleasant. (2) The components are pleasant as single shades.

As examples of comments indicating the first reason the following may be cited:

S 8, Card 6. "The contrast is not too sharp, but it makes the effect clear."

S 20, Card 10. "Small contrast between the two grays, yet it *is* a contrast."

S 24, Card 11. "Seem to go together without being glaring."

A number of *S*s were evidently trying to express this factor without being able to formulate it in exact terms. The word "blend" was frequently used in this connection.

Comments indicating the operation of the second factor were less frequent, but they showed that it was recognized. For example:

S 11, Card 15. "I like both shades, and they go well together."

S 10, Card 12 (a 2-step combination). "Has as background the most attractive shade of gray."

One-step combinations involving black or white rated in general low (Table V). Card No. 1 was designated by different *S*s as "uninteresting," "insipid," "sickish," "dingy," "bleary," "vague," "not organized." Most of these comments were made when the card was displayed on the white field which matched its background. Card 20 was designated as "dismal," "not definite," and "too black." On the other hand, one *S* found it "restful." Similar comments were made with regard to cards 5 and 16.

Thus, 1-step combinations, when they are taken toward the extremes of the brightness scale, seem to the *S*s to be unpleasantly weak and lacking in character, though when they are taken toward the center of the scale they are found highly pleasant. One *S* (*S* 15), in comparing card 1 with card 7 (both, in this case, pleasant) preferred 7, giving as the reason that it was "more central." This seems to indicate an absolute preference for the middle ranges of the brightness scale, and the results of the majority of the *S*s would, of course, bear out this assumption.

Most of the *S*s, preferring in general the smaller degrees of contrast, found the 4-step combinations of B and W unpleasant. These combinations are said to have too much contrast by 18 of the 24 *S*s. The effect is variously designated as "sharp," "harsh," "too bright," "glaring," "dazzling," "fighting," "jarring," "blinding." Of the remaining 6 *S*s one (*S* 7), was non-committal as far as the black and white combinations were concerned. The results of this *S* show, however, that cards 4 and 17 both rated lower than the average. The other 5 *S*s express a liking for these combinations. Three of the 5 *S*s (19, 21, and 23) worked with the B field of Series III, one (*S* 1) with the W field of Series I, and one (*S* 17) with the M field of Series II. Thus, it seems likely that the B field had a tendency to increase the pleasantness of the B and W combinations. The cases of four of these *S*s have already been considered in another connection. The other *S* (*S* 19) gives 15 preferences to card 4, but only 6 to card 17. *S* 21 also gives but 6 preferences to card 17, though giving 17 to card 4. Both state that they find pleasant the contrast of card 4 (B on W), but find card 17 (W on B) unpleasant because it is "dreary" (*S* 19), or "too black" (*S* 21). Thus these two *S*s show only a partial preference for the B and W combinations. This leaves only three *S*s (*S*s 1, 17, and 23), who show very marked preference for 4-step combinations. It was pointed out above that *S*s 1 and 23 make their choices throughout on the basis of the degree of contrast represented in the combination, but that *S* 17, while showing a preference for black-white combinations, is not determined in preference throughout by degree of contrast. In commenting on the black-white combinations, *S* 1 says that the shades "set each other off;" *S* 17 says that in contrast with each other the black and white are improved so that "each is best of its kind;" and *S* 23 says that the contrast results in a pleasing "definiteness."

The *S*s seem to form two groups with regard to the degree of contrast they prefer. The majority form a large group showing a preference for small degrees of contrast, especially for 1-step and 2-step combinations involving two grays. There is, however, a small group consisting of three, or possibly five, *S*s who show a preference for the sharp contrasts presented by black and white. There seems to be no intermediate group showing a preference for 3-step combinations, for there is only one *S* (*S* 9) whose results show a higher average number of preferences for the 3-step combinations than for those of any of the other step groups.

(*f*) *Sex differences.* As already stated, half of the *S*s in each series were women and half were men. The women were Nos. 6, 7, 8, 9, 10, 12, 15, 16, 17, 19, 20, and 22; the men, Nos. 1, 2, 3, 4, 5, 11, 13, 14, 21, 23, and 24. In the study of each one of the points taken up above, calculations were made for the men and women separately, in order to determine any possible sex differences which might be revealed. The only difference found which is large enough to be at all significant is a slightly greater tendency on the part of the men to prefer contrast. The two *S*s 1 and 23, who made their choices consistently on the basis of degree of contrast, are both men. Of the five *S*s who showed a preference for 4-step combinations, three are men and two are women. The difference appears also in a comparison of the average number of preferences for the combinations of the different step groups given by the men and women respectively. These are as follows: 1-step: Men 9, Women 10.9; 2-step: M. 11, W. 10; 3-step: M. 9.5, W. 7.9; 4-step: M. 7.3, W. 5.8. These differences, small and probably of little significance, were the only sex differences found.

V. Conclusion and Summary

The most important result of this study seems to the writer to be the fact that the majority of the *S*s are found to prefer relatively small degrees of contrast. Cohn's principle that combinations are pleasanter the greater the difference in brightness between the components is consistently borne out by the results of only 2 of our 24 *S*s. Only 5 of the 24 showed a preference for maximum contrasts. Cohn related this brightness principle to his results for color. Finding that combinations between complementary colors were preferred to other color-combinations, he formulated the general principle: "Auf dem Gebiete der Gesichtsempfindungen wirkt die Verschiedenheit benachbarter Empfindungen lusterregend um so mehr, je grösser sie ist."[6]

Cohn's conclusion with regard to complementaries has not been borne out by subsequent investigations. Baker, in each of two separate studies on color-combinations, found very little, if any, justification for it.[7] Barber, in an extended experimental study of binary combinations in which tints and shades were used as well as saturated colors, got results which tend to confirm Baker's as far as the pleasantness of complementaries is concerned.[8] Geissler found no evidence in his results for the supposed superior pleasantness of complementaries, or even of "near-complementaries."[9] It would seem from our results that a similar revision is required for the idea that two brightnesses in combination are more pleasant the greater their difference.

Cohn's color-blind *S*, a case of red-green color-blindness, was the single exception to his rule. The red-green color-blind *S* in our experiments (*S* 6) seems to be highly sensitive to contrast, and to show a more than average preference for 1-step combinations and aversion to 4-step ones. Her average is 14.1 for 1-step combinations as compared with 10.0 for all *S*s and 0.5 for 4-step ones as compared with 6.5 for all. One would naturally expect such results from a *S* who was, as color-blind persons presumably are, especially sensitive to brightness differences. However, similar results are found for *S* 12 (14.4 for 1-step, 0.5 for 4-step combinations) whose color-vision *E* had no reason to suspect of being abnormal. It seems doubtful, therefore, that the factor of color-blindness affected the result.

Briefly summarized, the chief results of our investigation are as follows:

(1) The *S*s in general preferred combinations of two grays which represented a relatively small degree of brightness difference. Combinations of gray with black or with white were in general less preferred, and combinations of black with white least preferred.

(2) Two of the 24 *S*s found brightness combinations pleasant in proportion to the degree of brightness difference between the components. In addition, 3 *S*s also showed a preference for the black-white combinations.

(3) With the stimuli used in our experiment, background played more of a part than center in determining the pleasantness of a combination.

(4) The brightness of the field on which the stimuli were shown had an effect upon their pleasantness, increasing it when field contrasted with background, decreasing it when field matched background.

(5) The arrangement in which a dark center appears upon a light background was slightly preferred to the reverse arrangement.

(6) When single shades were arranged in the order of their pleasantness, the medium gray was in general most preferred, and was followed in order by light gray, dark gray or white, and finally black.

(7) The pleasantness of the single component shades is an important —possibly the most important—factor in determining the pleasantness of a combination.

(8) No sex differences appeared, except that the tendency to prefer small to greater degrees of contrast was slightly more marked in the results of the women than in those of the men.

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[6] Cohn, *op. cit.*, 603.

[7] E. S. Baker, Experiments on the aesthetics of light and color: I. On combinations of two colors, *Univ. Toronto Studies, Psychol. Ser.*, 1, 1900, 65 ff. (241 ff.); II. Spectrally pure colors in binary combinations, *ibid.*, 2, 1907, 42 f.

[8] F. L. Barber, Color aesthetics: II. Combinations of colors, tints and shades, *ibid.*, 2, 1907, 45 (289).

[9] L. R. Geissler, *op. cit.*, 165.

THE FUSION OF NON-MUSICAL INTERVALS*

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Psychologically fusion is an equivocal term, and even when its meaning is carefully narrowed it corresponds to a very difficult *Aufgabe*.[1] There is also some question as to the exact meaning of musical interval.

I. QUESTIONS OF INTERVAL

There are two ways in which a musical interval can be defined: (1) an interval in which the vibration-frequencies are in simple mathematical ratio (the simple interval of the physicist); (2) an interval which has been used in music. If we adopt the physical definition, we are confronted with the fact that the intervals on the 'tempered' scale of to-day are not in exact ratios at all. On the other hand, there are some 154 different intervals which have been used, and which, therefore, could be called musical.[2]

Nature of Interval. The physical nature of the interval is worthy of consideration. In the first place, the fact that the partials of a tone are in simple vibration-ratio with the fundamental and with each other gives the 'simple interval' a basis in natural experience. Moreover the facts that beats occur between two tones a short distance apart on the scale and between mistuned simple intervals (as Krüger has shown),[3] and that extraneous difference tones occur elsewhere (generally where the ratio is other than simple) tend to make the simple interval comparatively a simple sound. Thus we have two factors, one experiential and the other immediate, determining qualitative aspects of the sound which are dependent on vibration-ratio.

Musical Scales. As musical expression developed and there came into use more intervals than the octave and the simplest intervals, it became desirable to place the tones along a scale so that a series of intervals would be found around each tone on the scale. The modern scale of twelve semitones within the octave has been developed in the following ways:

(1) The Pythagorean scale is constructed from a series of perfect fifths which, when reduced to the compass of an octave, gives the complete diatonic scale.

(2) The just scale ascends by fifths and thirds, thus giving the third an exact ratio which it does not have in the Pythagorean scale.

(3) The meantone scale gives the fifth the ratio 109: 163, and ascends by the Pythagorean method.

(4) The tempered scale divides the octave into twelve geometrically equal parts along a scale of 1200 'cents.'

This makes it evident that it has been found impossible to devise a scale in which all tones are in simple ratio to all the others. On the Pythagorean scale the third comes out 64:81; the meantone scale, to avoid this ratio, mistunes the fifth; the just scale tunes both the third and the fifth at the expense of the other intervals; while the tempered scale mistunes them all in order to approximate all and obtain a simpler series.

The intervals c:c## and c:d are alike only on the mistuned tempered scale. On the meantone scale the value of the first is 152 cents and of the latter 193 cents. Hence we should have a far from musical interval if we had c and c## in the same chord; but, since these occur in wholly unrelated keys, they would practically never be found together. Slight changes of pitch of this sort are met with in enharmonic musical changes; many instrumentalists and at least some choralists are sensitive to such changes.

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*Communicated by C. C. Pratt.

[1] C. C. Pratt, this JOURNAL, 32, 1921, 490-515.

[2] H. Helmholtz, *On the Sensations of Tone*, tr. A. J. Ellis, 4th ed., 1912, 516.

[3] F. Krüger, *Psychol. Stud.*, 5, 1910, 294-409.

Melodic intervals seem to be learned rather than natural. Those who have learned to sing to the piano use the tempered intervals; some *a capella* choralists sing the just intervals. The best instrumentalists, however, play the third as 4:5,[4] and unpracticed singers frequently use mean-tone intervals, presumably because the thirds and fifths and their inversions are pure. Artistic singers, however, vary greatly in pitch while singing a single note,[5] and naïve singers have been found to vary in the rendition of the tones of a folk-song from 76 cents (for the octave) up to 272 cents for the second.[6] So, after all, it is probably only in harmonic music that accurate intervals are used.

Non-Musical Intervals. We have seen how the transition from musical to non-musical intervals may take place in our present musical system. Other systems of music use definite 'non-musical' intervals. A few examples will suffice:

(a) Javanese and other oriental musics have a pentatonic scale in which the octave is divided into five parts. The divisions are equal; thus the intervals are 240, 480, 720, and 960 cents.[7]

(b) In the ninth century Zalzal in Arabia devised a three-quarter tone scale, employing intervals of 355 and 853 cents.[8] In the nineteenth century Meshaqah in Arabia used a quarter-tone scale giving many secondary (melodic) notes.[9]

(c) The Scotch bagpipe uses intervals of 341 and 853 cents.[10]

(d) An ultra-modern Hungarian, Mr. Hába,.has made a quarter-tone piano and composes for it.[11] He, as is not the case in the first three examples, frankly uses 'non-musical' intervals in harmony as well as in melodic line.

Conclusions. (1) No musical scale can be constructed which has all tones in simple ratio to all the others; some or all of the intervals must be approximated.

(2) There seems to be a phenomenological basis of recognition of pure intervals.

(3) A singer tends to sing melodically in pure intervals, but also in intervals on a scale which he has learned.

(4) He tends to sing harmonically in pure intervals, for in reproducing a tone with distraction he tends to sing tones which are harmonious with the distracting tone.[12]

(5) Non-musical intervals seem to have been used up to the present time only in homophonic music that lacks tonality or harmony.[13]

II. QUESTIONS OF FUSION

In general psychologists have been content to describe fusion not too definitely: as a product of an act of the perceptive apparatus or as a symptom of its posture. Recent accounts, especially those appearing in the *Gestalt* literature like Wertheimer's article on configurations in the visual field,[14] have tended to bring these matters down to the level of clear explanation used heretofore on the simple phenomena of sensation.

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[4] Helmholtz, *op. cit.*, 235.

[5] M. Schoen, *Mus. Quart.*, 12, 1926, 290 ff.

[6] O. Abraham, *Psychol. Forsch.*, 4, 1923, 1-22.

[7] Ellis, in Helmholtz, *op. cit.*, 518, 522.

[8] *Ibid.*, 516-520.

[9] *Ibid.*, 525.

[10] *Ibid.*, 515. E. Smith, *J. Amer. Or. Soc.*, 1, 1847, 73 f.

[11] A. Welleck, *Mus. Quart.*, 12, 1926, 231 f.

[12] E. H. Cameron, *Psychol. Monog.*, No. 34, 1907, 227-300.

[13] Ellis, *op. cit.*, 526 f.

Tonal Fusion. Simultaneous fusion of tones, as the simplest and most easily controlled case of fusion, has been the subject of numerous theories, and yet we can still appreciate the difficulty noted by Mach, when he said, "the lack of a positive factor in the explanation of consonance has been very generally felt."[15] It is true that every theory of tonal fusion has in general made fusion dependent upon one, and only one, 'positive factor.' Existing theories have been classified below according to their reference to these factors.

Theories.

I. There are theories which make fusion dependent on *vibration ratio.* Until very recently all theories postulated such a dependence and made the explanation of this dependence their basis.

(A) Fusion depends on *vibration ratio by means of extrinsic labels.*

(1) Fusion depends upon the partial tones—the original physical theory of Helmholtz.[16] That beating partials operate to reduce the degree of fusion is difficult to deny, but Stumpf and more recent critics have pointed out that the fact of mutual reinforcement of partial tones affords no positive explanation of the fact of fusion.

(2) Fusion depends upon the difference tones—Krüger's supplement to the Helmholtz theory.[17] This view is open to the same criticism as its predecessor, and furthermore Stumpf in refutation has shown how this theory would presuppose the fusion of a few intervals such as 8:11, 10:17![18]

(B) Fusion depends on *vibration ratio by means of immanent factors.*

(3) Fusion is a direct phenomenon of vibration ratio due to 'micropsychic' rhythmic coincidence of beats. This theory of Lipps,[19] elaborated from the speculations of Leibniz and Euler, has been refuted largely by subsequent experimental work.[20] It is incapable of explaining, for example, the good fusion of a slightly mistuned octave.

(4) Fusion depends upon a relation in the tonic (the common fundamental of two tones) or in the phonic (the common overtone). Von Oettingen made this relation a remembrance;[21] Mach placed it on the sensory level.[22] However, these theories treat of matters of quality rather than of qualitative fusion.

(5) Fusion depends upon a relational element of specific synergy in consciousness. Stumpf identified fusion with a unitariness,[23] which depends upon a similarity of a hypothetical sort not the same as similarity seen in sequence.[24] Revész found qualitative similarity only between tones in the octave relation.[25]

(6) Fusion depends upon a relational effect through association of tonal excitation. Ebbinghaus' theory is parallel to that of Stumpf, but makes the relation peripheral rather than central.[26] Stumpf considered this conception less adequate because it does not explain degrees of fusion in imagination.

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[14] M. Wertheimer, *Psychol. Forsch.*, 4, 1923, 301-350.

[15] E. Mach, *Analysis of Sensations*, tr. C. M. Williams, 1914.

[16] Helmholtz, *op. cit.*, 182 ff.

[17] Krüger, *loc. cit.*

[18] C. Stumpf, *Zsch. f. Psychol.*, 59, 1911, 161-175.

[19] T. Lipps, *Psychologische Studien*, 1905, 115 f.

[20] R. M. Ogden, *Hearing*, 1924, 133 f.

[21] A. v. Oettingen, *Harmoniesystem*, 30 ff.

[22] Mach, *op. cit.*, 224.

[23] C. Stumpf, *Abhandl. d. Münch. Akad.*, 1897.

[24] Stumpf, *Beiträge zur Akustik*, I, 1898, 50.

[25] G. Revész, *Zur Grundlegung der Tonpsychologie*, 1913.

[26] H. Ebbinghaus, *Grundzüge der Psychologie*, 1923, I, 350 f.

(7) Fusion depends upon a balance or symmetry in the excited organ. Watt based this conception upon his theory of tonal mass, and made fusion a matter of symmetrical coalescence of volumic masses.[27] There arises, however, the objection that geometrical symmetry on the basilar membrane does not evidently connote fusional phenomena, that the configurational relation is not apparent. Work on volume by Rich[28] has been used by Ogden[29] to show that the volumes of the partials cannot possibly have the proportional values demanded by Watt's theory.

II. There are theories which make fusion depend upon *frequency of association* or otherwise upon experiential data. A few more recent theories would make fusion wholly a matter of familiarity.

(8) Fusion depends upon familiarity established in the past experience of the individual. This view appears in some of the theories mentioned above, notably those of Von Oettingen and Ebbinghaus.

(9) Fusion depends upon familiarity in racial experience. Ogden[30] put forward frequency of association as a factor which was denied by Stumpf[31] but reaffirmed by the experimental work of Valentine[32] and Moore.[33] He admitted the additional influence of immanent and extrinsic factors.

(10) Fusion depends upon tradition and experience. In his criticism of Ogden, Peterson points out the danger of postulating an acquired somatic character which is transmitted, and appeals to tradition as the medium of transmission.[34]

As a transition to the final group of theories, it will be well to recall the work of Edmonds and Smith,[35] showing that each interval is recognizable by means of its phenomenal characteristics, described in terms of a number of factors.

III. There are theories which take into account *multiple factors*.

(11) Fusion in the sense of unitariness is one of a number of factors. Kemp found in the tonal complex a factor of fusion as an unalterable peculiarity of the sensory material, and also a number of other sensory and affective factors.[36]

(12) Fusion in the sense of unitariness is an integration of factors. Pratt, in the study of dimensional factors such as roughness, analyzability, disparateness, and richness, found that intervals have a different value in each dimension,[37] and seemed further to find that the results for these four factors average to the equivalent of results from unsophisticated judgments on unitariness, a conclusion which would make unitariness the integrative result of multiple factors.

III. Experimental Investigation

The experiments described in this paper were carried on during the two academic years 1924-1926. During the first year some preliminary work was completed for a set of non-musical intervals, which were presented on an Appunn tonometer, a reed instrument giving a rich set of overtones with each stimulus. The *O*s were: Mrs. E. C. Gilbert (G); Miss H. F. Moran (M); Mr. Herbert Gurnee (HG); and Mr. Morgan Upton (U).

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[27] H. J. Watt, *Psychology of Sound*, 1917, 3 ff.

[28] G. J. Rich, this Journal, 30, 1919, 121-164.

[29] Ogden, *op. cit.*, 133 f.

[30] *Ibid.*, 140-149.

[31] Stumpf, *Tonpsychologie*, II, 1890, 208.

[32] C. W. Valentine, *Brit. J. Psychol.*, 7, 1914, 118-135.

[33] H. T. Moore, *Psychol. Monog.*, 17, 1911, No. 73, 1-68.

[34] J. Peterson, *Psychol. Rev.*, 32, 1925, 17-34.

[35] E. M. Edmonds and M. E. Smith, this Journal, 34, 1923, 287-291.

[36] W. Kemp, *Arch. f. d. ges. Psychol.*, 29, 1913, 139-181.

[37] Pratt, *loc. cit.*

The main part of the experiment was performed during the following year; musical and non-musical intervals alike were presented, on a set of Stern variators.[38] These instruments give comparatively pure tones.

For this part of the experiment the *Os* were: Dr. M. B. Pratt (MP); Mr. A. G. Ekdahl (E); Dr. F. A. Pattie (P); and Mr. E. G. Wever (W), all members of the Harvard Psychological Laboratory.

For the purposes of the experiment we took as musical intervals the 12 intervals between the octave as found on the tempered scale (in general reduced to simple vibration-ratios), and used for non-musical intervals 12 intervals so chosen that each one, as a ratio, was the geometrical mean between two of the musical intervals, *i.e.* at intervals of 50, 150, 250 cents, etc.

The objects of this experiment were in general as follows: (1) to study musical and non-musical intervals alike with reference to their relative degrees of fusion; (2) to investigate the relation between fusion and simple vibration-ratio; (3) to investigate the relation between fusion and pleasantness; (4) to investigate the relation between fusion and familiarity; and (5) to study the relation between fusion and various phenomenal aspects of the interval.

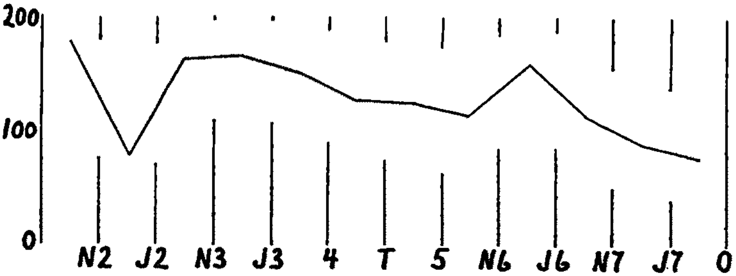

FIG. 1. NON-MUSICAL INTERVALS IN PAIRED COMPARISONS

The musical intervals of which these are the intermediates are shown as abscissae; the intervals grow larger reading from left to right. The numbers of judgments favoring each interval as more unitary is indicated as ordinates. This curve shows the combined totals for all observers. N = minor; J = major; T = tritone; O = octave.

Preliminary Series. In this series 12 intervals were used, and they were presented for judgment by the method of paired comparisons. At no time were any musical intervals used in this series. The order of presentation was reversed in successive series to avoid the possibility of stimulus-error through cadential effect (Kemp's factor of need of resolution).[39]

A first series was given in an attempt to fix a criterion of fusion for each *O*, preferably alike for all. They were instructed to try to form a good criterion based on the conception of the meaning of 'fusion.' Upon demanding introspections for these naïve judgments, it was quite evident that 'fusion' bore a wholly different meaning for every *O*. To one it meant *intimacy-separateness;* to another, *smoothness;* to another, *pleasantness;* while to the remaining *O* it was "too much of an intellectual conception" for easy judgments.

It was at once decided that a stricter criterion should be used, and Stumpfian 'fusion'—*unitariness*—was chosen. Six complete series of paired comparisons were given.

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[38] These variators were secured through a grant from the Milton Research Fund of Harvard University.

[39] Kemp, *loc. cit.*

It was possible to draw the following conclusions from the numerical results:

(1) Correlations between rank orders for the different *O*s show only one convincing correlation—a coefficient of 0.65 between HG and M. The others were all less than 2.5 times the probable error (see Table I).

(2) There was one very equivocal interval—the quarter tone. It was phenomenally a single tone with 8 regular oscillations of intensity (beats) per second. Two *O*s ranked it first, disregarding the beats; the others gave it positions 5 and 11.

TABLE I

I. Correlations between rank orders for each pair of observers: Unitariness series.*

HG–U	−0.06	Probable error, 0.124
U–M	0.17	
U–G	0.22	
HG–G	0.29	
G–M	0.31	
HG–M	0.65	

II. Correlations between rank orders for each pair of observers: Pleasantness series.*

M–U	−0.23	Probable error, 0.124
M–G	0.02	
M–HG	0.07	
G–HG	0.50	
U–HG	0.55	
U–G	0.66	

III. Correlations between rank orders for each observer, for the two series: Pleasantness and Unitariness.*

HG	−0.24	Probable error, 0.124
U	0.14	
G	0.58	
M	0.73	
Combined	0.73	

*All correlations by Spearman's formula.

(3) There was a general trend favoring 5 of the intervals as well fused: 4 intervals in the region between the major second and the tritone, and one between the sixths.

(4) The other intervals were all very rough, being near unison, the octave, or the fifth. The greatest disagreement among the *O*s appeared for these intervals.

(5) M mentioned in introspection the 'cadential effect' and gave frequent reversals of judgment on reversal of the orders of pairs in the series.

(6) The *O*s were fairly consistent with their own results; different series of the same *O* correlate highly.

(7) Introspections were demanded on the determinants of unitariness. No leading descriptive words were suggested; hence it is significant that some *O*s used nearly the same words. (a) HG and U used as the key-word "separability," apparently some active process. (b) G and M used "apartness," apparently a passive phenomenon stated in or translated into spatial terms. (c) Most significant of all was the fact that in no case where two *O*s used the same words in describing the process did they show

any sort of correlation of rank-orders. Hence the phenomenon of unitariness would seem to be complex below the level of introspective analysis which the *Os* were using.

The Affective Factor. To determine the part played by affection in the perception of fusion, a series of the same proportions and using the same stimuli was presented, with instructions to judge on pleasantness-unpleasantness, to forget the old *Aufgabe* as much as possible, and to assume a more passive attitude. We may summarize the results:

(1) All *Os* but U soon lost the old *Aufgabe;* U thought the two correlated. (No such correlation, however, appears in his results.)

(2) M gave zero correlations with the other *Os* (with respect to rank-orders). The other *Os* gave high rank-order correlations (between 0.50 and 0.66) with each other.

(3) For HG and U the pleasantness series showed no correlation with the unitariness series which could be considered significant; but G and M gave a high correlation.

The highest correlation of unitariness with pleasantness was given by M, whose results for pleasantness did not correlate at all with those of the other *Os*. This difference seems to justify the conclusion that pleasantness *meant* fusion to her, and that fusion was the immediate fact. Excluding this case, our observations lead to the following conclusions:

(1) Pleasantness, far from being dependent on fusion, seems to be a more univocal datum, for in respect of pleasantness the *Os* are in agreement.

(2) Pleasantness is frequently wholly unrelated to fusion.

Main Series. Stimuli. The preliminary series indicated that some of the non-musical intervals might be as well fused as certain of the musical intervals. Stumpf mentioned five grades of fusion with intervals of simple ratio: (1) the octave; (2) the fifth; (3) the fourth; (4) the thirds and sixths; (5) all other (dissonant) intervals.[40] It is evident that there are several grades within Stumpf's fifth class. Is it not possible that some of the non-musical intervals among the thirds and sixths may fall in the fourth class? The judgments of a set of naïve *Os* on a series of 12 musical and 12 non-musical intervals, recorded very roughly, seemed to lend justification to some such notion.

We continued to use the Stern variators in presenting the stimuli. Their tones are practically pure; only the first overtone is at all discernible; a mistuned octave beats, but a mistuned fifth beats only faintly and other slightly mistuned intervals not at all. In all there were four variators, with ranges of 200-400, 300-600, 400-800, and 500-1000 d. v., respectively. They had some disadvantages: they were not entirely accurate, and had to be adjusted at first and calibrated against each other afterwards; and among the lower tones there was a certain hissing or 'breathiness.'

Of the *Os*, MP was the only one that might be considered particularly musical. She recognizes intervals and has considerable technical knowledge of music. The others were essentially non-musical, and P ranked particularly low in the Seashore tests. MP tried to maintain a naïve psychological attitude.

After a preliminary work-out, three series were given, with stimuli and instructions changed for each series.

First Series. The entire series of 24 intervals, from a quarter-tone above unison up to the octave, was presented 10 times for absolute judgments on a scale of five points. Each interval was presented 5 times in a low range (from 250 to 550 d. v.) and 5 times in a high range (from 350 to 700 d. v.) to avoid any stimulus-error due to 'breathiness' in the lower range or sharpness in the higher. The *Aufgabe* was first given as 'unitariness or fusion,' but the latter word was subsequently struck out because, as in the preliminary series, it bore more than one connotation.

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[40] Stumpf, *op. cit.,* 135.

TABLE II

DEGREES OF FUSION FOR MUSICAL AND NON-MUSICAL INTERVALS. 4 OBSERVERS

First Series: average of ten trials

	Observers				
Interval	MP	E	W	P	Average
Quartertone	3.4	1.6	4.4	4.8	3.550
Min. 2d	2.5	1.0	3.5	3.8	2.700
+ Min. 2d	2.5	1.0	3.2	2.8	2.375
Maj. 2d	2.4	1.1	2.2	3.6	2.325
+ Maj. 2d	3.3	1.5	3.2	3.2	2.800
Min. 3d	3.9	2.4	3.6	3.0	3.225
+ Min. 3d	4.2	2.2	3.3	3.2	3.225
Maj. 3d	3.8	2.4	3.8	3.7	3.425
+ Maj. 3d	2.4	3.2	3.8	3.5	3.225
Fourth	3.0	3.0	3.6	3.6	3.300
+ Fourth	2.7	3.2	3.6	4.2	3.425
Tritone	3.3	3.7	3.1	3.5	3.400
+ Tritone	1.7	2.9	3.9	2.7	2.800
Fifth	1.9	3.4	3.5	3.9	3.175
+ Fifth	2.2	2.8	2.6	3.3	2.725
Min. 6th	3.1	3.7	3.6	3.4	3.425
+ Min. 6th	3.6	3.3	3.1	3.5	3.375
Maj. 6th	3.7	3.1	3.2	3.4	3.350
+ Maj. 6th	2.1	2.7	3.1	2.7	2.650
Min. 7th	2.6	3.3	3.3	2.8	3.000
+ Min. 7th	2.2	2.4	2.2	3.2	2.500
Maj. 7th	1.6	2.3	2.6	3.0	2.375
+ Maj. 7th	4.2	3.2	3.8	2.9	3.525
Octave	5.0	4.7	4.6	4.1	4.600
Second Series: average of six trials					
Min. 3d	3.7	3.0	2.8	1.8	2.833
+ Min. 3d	3.8	2.7	3.7	2.2	3.083
Maj. 3d	4.0	2.8	3.3	2.3	3.250
+ Maj. 3d	3.0	2.7	3.3	2.8	2.958
Fourth	2.2	4.2	3.7	2.5	3.125
+ Fourth	1.5	3.8	4.0	2.8	3.042
Tritone	2.2	4.0	3.5	3.3	3.250
+Tritone	1.7	3.5	3.3	3.5	3.000
Fifth	1.8	2.7	4.2	4.3	3.250
+ Fifth	1.7	3.8	3.7	3.5	3.167
Min. 6th	3.2	3.2	4.2	3.8	3.583
+ Min. 6th	3.7	3.7	3.0	3.7	3.500
Maj. 6th	3.7	2.7	3.2	3.7	3.292

Second Series. The same instructions were given, and 13 intervals from the more equivocal part of the scale were presented each 6 times. All the musical and non-musical intervals from the minor third up to the major sixth were used; the same precaution was taken to present half in the lower range and half in the upper. Occasional judgments of the four adjacent intervals were taken.

Third Series. Eight intervals, 4 musical and 4 non-musical, were chosen in the central part of the scale, from the perfect fourth up to a quarter-tone above the minor sixth. These were presented for paired comparisons, and four complete series of 28 comparisons were taken. The order of presentation of each separate pair was reversed with each trial, and after the second set all the high tones were reset to become low, and *vice versa.*

The musical intervals in the first and third series were in *simple vibration-ratio;* in the second series the *tempered scale* was used. The order of presentation in every case was entirely random.

TABLE III.

DEGREES OF FUSION FOR MUSICAL AND NON-MUSICAL INTERVALS

Final averages, first and second series (cf. Table II); all observers

Interval	Av.
Quartertone	3.550
Min. 2d	2.700
+ Min. 2d	2.375
Maj. 2d	2.325
+ Maj. 2d	2.800
Min. 3d	3.029
+ Min. 3d	3.154
Maj. 3d	3.338
+ Maj. 3d	3.092
Fourth	3.212
+Fourth	3.233
Tritone	3.325
+ Tritone	2.900
Fifth	3.212
+ Fifth	2.946
Min. 6th	3.504
+ Min. 6th	3.437
Maj. 6th	3.321
+ Maj. 6th	2.650
Min. 7th	3.000
+ Min. 7th	2.500
Maj. 7th	2.375
+ Maj. 7th	3.525
Octave	4.600

Average of non-musical intervals, 1st. Series	3.015
Average of musical intervals,* 1st. Series	3.044
Average of non-musical intervals, 2nd. Series	3.130
Average of musical intervals, 2nd. Series	3.226
Average of non-musical intervals, both Series	3.057
Average of musical intervals,* both Series	3.110

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*Excluding the octave.

Reports. First of all, a series of 12 musical intervals and about three times as many intermediate intervals, chosen at random, were presented with instructions for the *O* to judge each interval with regard to fusion on a positive scale of seven points. The results were not tabulated. As three of the *Os* made use of but four or five points of the scale, it was decided that a scale of five points would be sufficient, and the *Os* were given written instructions accordingly.

Here they were asked, as in the preliminary trials of the previous year, to judge upon unitariness or fusion. This was done in order to allow each

O to form a good criterion of judgment either from the meaning 'unitary' or from the meaning 'fused;' but again we found that 'fusion' was an unsuitable term: it was too general a term to W; it was determined by the active analyzing attitude for MP, and by the passive attitude for E and P. 'Unitariness' in general meant the passive attitude for all the *O*s wherever analysis and attentiveness were distinguished. The series was completed with the instructions expurgated so as to include only the word 'unitariness.' The judgment *five* was to be given for complexes "entirely unitary" and the judgment *one* for those "not at all unitary, like two separate things." The second limited series was given with the same instructions. The stimuli and results for these two series appear in Tables II and III.

During the second series, when the *Aufgabe* of unitariness seemed to be completely set, the *O*s were given a few random stimuli with instructions to "characterize the individual nature of each of the numbers used as response words." It seems worth while here to quote the report of W for its bearing on the matter of roughness. He distinguished the numbers as follows:

5—A clear single tone.
4—One clear tone; another, weaker, just standing out; *or* quality suggested as in dumb-bell effect.
3—Two tones standing out from each other.
2—More complex, with three outstanding tones; *or* two tones standing out distinctly.
1—Several tones of varying intensity; *or* two tones completely separated.

It is seen that this *O* made a *double classification* for non-unitary dyads as they have and have not difference tones; the former appear as a bunch of tones of different intensity, the latter as two tones becoming less and less intimate. He said, "roughness is well abstracted from but tends to reduce unitariness half a step."

Introspective Factors. (1) *Simple unitariness.* This seems to distinguish the best tonal fusions from all the rest. It is represented as *occlusion* of one tone by the other. It is not a uniform process: for P the higher tone predominates; for E it is the lower. Probably more than one process is at work.

(2) *Attitudinal factors.* (a) There may be the inability to analyze although complexity is perceived; E so described the typical judgment '4.' (b) A certain modicum of attention is necessary to, and brings with it, analysis. This attention must be on the complex, and yet apparently not on the more intense component *per se.* (c) Double meaning seems to occur with slow beats. MP stated that the quarter-tone may be judged '1' if it means oscillation back and forth from one tone to another, while it may be judged '5' if it means the oscillation of a single tone. (d) Other double meanings which are more complex and less easily described are frequently noted. These effects can be due to fluctuations in the center of attention, as in (b), or to fluctuations in meaning, as in (c).

(3) *The coherence factor.* In introspection this factor seems to work both ways: at times coherence connotes fusion; again disparity leads to fusional effect.

(4) *Roughness.* This is the most prominent factor and the most easily dealt with. It seems to be due entirely to fast beats or difference tones, and either enters frankly into the judgment, or is carefully abstracted from.

The fact that W maintained a double classification, one for those judgments with difference tones which stand out, and the other for those in

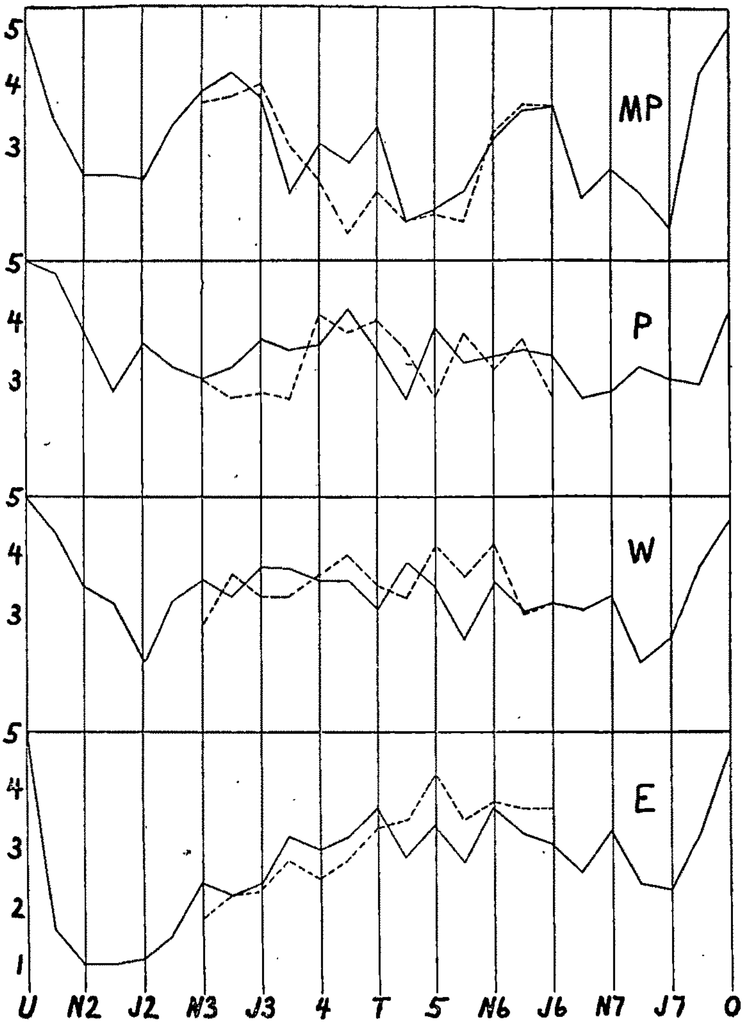

FIG. 2. DEGREE OF FUSION OF 24 INTERVALS AS JUDGED BY EACH OBSERVER

The musical intervals occur at the vertical lines; their intermediates are between the lines. The numbers on the ordinate scale indicate the average of the *O*'s judgments. The continuous line indicates the first series; the broken line is the second series. U = unison; N = minor; J = major; T = tritone; O = octave.

which both components are clear, tends to show that fusion depends upon much more than simple difference tones and the other superficial roughness factors which are at the root of Helmholtz's and Krüger's theories.

In the introspections, we find with surprising frequency the major third mentioned as being rough—more often in fact than most of the non-musical intervals.

(5) *Richness and brightness.* These factors are chiefly characteristic of the intervals in the middle of the octave.

(6) *Factors of association and familiarity.* Here we can only quote W's statement that " although fusion does not follow musical properties, they disturb judgment and adherence to the criterion."

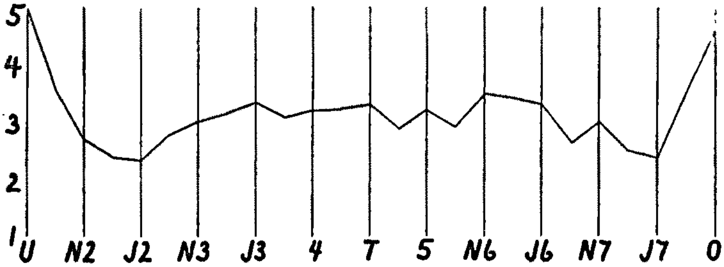

Fig. 3. Average of Curves in Fig. 2

Average of judgments on each interval by all *O*s. The musical intervals occur at the vertical lines; their intermediates are between the lines. Cf. Fig. 2.

Results. A first scrutiny of the general results as represented in Figs. 2 and 3 shows clearly not only that some non-musical intervals are as well fused as certain musical intervals, but also that (when the stimuli are comparatively pure tones, at least) all the intervals make a more or less smooth curve, with the degree of fusion changing constantly and smoothly as the interval is slowly altered to make it wider or narrower. If we examine the final curve or any of the others for any sort of Stumpfian hierarchy of fusions, we find nothing of the kind. It is true that the octave heads the list, but we find the fifth subordinated to no less than four of the 'dissonant' intervals!

This makes it seem clear that simple vibration-ratio is not the important determinant of fusion that it has been supposed to be. Nor does the fact of familiarity appear to tell the entire story. It seems at the first glance as if we had indeed a number of independent factors, each determining a different set of fluctuations. The meaning of the Stumpfian results will be discussed later, together with a further consideration of our first evident conclusions.

If we examine the curves for the individual *O*s, recalling that one of the superimposed curves represents the pure musical intervals and the other the tempered scale, we find no significant difference in the two sets of results. With two of the *O*s the tempered fourth is better fused than the pure fourth; with the other two the reverse is the case. Practically the same may be said of the other musical intervals.

We can study the question of increased fusion of a complex as it becomes more familiar. Taking averages of the judgments on 6 non-musical intervals in the first series (when they were absolutely unique) and in the second series, we get the following results:

	MP	P	E	W
First Series	2.8	3.4	2.9	3.4
Second Series	2.6	3.4	3.1	3.5

This lack of increase along the scale is not due to a downward movement of the whole scale, for it rose some two-tenths of a point.

Since some degree of habituation might have taken place earlier, the results of the most musical *O* were studied to determine whether there had been any growth of fusion of the unfamiliar intervals within the first series. Comparison of the first five judgments with the last five, showed that on the average the musical intervals became more unitary and the unfamiliar ones less unitary.

In general, the depressions in the curves correspond to those points on the bitonal scale where roughness occurs. This relation holds for the region of the seconds and sevenths, and explains the depressions on either side of the perfect fifth which do not, however, appear with every *O*. If we examine the tonometer curve in Fig. 1, we shall observe that the depressions are much more sudden. The isolated peak between the sixths on this curve is between the roughness area around the octave and that around the fifth.

It will be seen that the curve for W, who abstracted from roughness, shows these depressions in lesser degree than any of the others.

The peak at the minor seventh is hard to explain. It is noticeable in the results of all but P. A few introspective reports mention unitariness in the sense of 'occlusion,' but no further hint is given.

The general trend of the curve of Fig. 3 is up as the interval becomes wider, but if we examine the curve of Fig. 1 for the tonometer, we notice that there the general trend is down. Now if we look at the separate curves in the later series, we see that the general upward trend is due almost wholly to the influence of E, who built himself a trough of complete disunity around the seconds and in general tended to rate intervals higher as they were further apart. The opposite tendency, displayed in Fig. 1, is shown clearly by other *O*s. Thus the factor of coherence appears to be very questionable, for until we can describe two such processes we shall have to watch one process work at different times in opposite directions.

The experiment ended with a short series of relative judgments, which were taken to bring out any flagrant bivalency involved in the attitude in absolute judgment, whereby the scale used in judging intervals of simple ratio might differ from that used in judging the more complex intervals. A very limited part of the scale was used. Four series were taken, using eight intervals in complete series of paired comparisons.

MP and W both stated that in relative judgments such factors as roughness assumed more importance. In giving the pairs of intervals, one was always pitched much higher than the other. All the *O*s at once formed a prejudice in favor of either the higher or the lower complex.

It is hardly necessary to refer to the results more than to remark that the tritone ranks high, that the fifth occupies a singularly subordinate position, and that the non-musical intervals do not show any particular lack of unitariness.

Conclusions. The data thus briefly surveyed enable us to draw certain general conclusions with regard to the problem of tonal fusion.

(1) At least a great deal of what goes to make up fusion is not dependent on simplicity of vibration ratio. Krüger's difference tones enter into the perception in a striking degree, but all *O*s seem to be able to abstract from them and still to find many degrees of fusion. Roughness as a general phenomenon touches general parts of the field.

Immanent factors, too, seem to be ruled out. 'Micropsychic' rhythms no longer have aught to explain. Watt's nice adjustment of balance becomes rough and but massively symmetrical action of the *Gestalt* sort. The factor of similarity works well enough in the case of the octave, but, as Revész would expect, plays no part elsewhere.

With regard to Stumpf's results, we must note that in his work stimuli full of overtones were presented rapidly and judgments were given of 'one' or 'two' main tones. This sort of thing amounts to recognition of a given set of partials, and naturally the more like a more intense overtone the upper tone is, the more likely it is to be judged part of the lower tone ('one'-judgment).

TABLE IV

RELATIVE JUDGMENTS FOR MUSICAL AND NON-MUSICAL INTERVALS

Table shows number of judgments favoring each interval

		OBSERVERS			
Interval	MP	P	E	W	Total
Fourth	13.5	14.0	14.0	11.0	52.5
+ Fourth	10.0	14.5	10.5	15.0	50.0
Tritone	11.0	17.0	13.5	17.0	58.5
+ Tritone	13.5	11.0	9.5	12.5	46.5
Fifth	11.0	13.5	15.5	13.0	53.0
+ Fifth	12.5	16.5	14.0	16.0	59.0
Minor Sixth	21.5	12.0	17.0	15.0	65.5
+ Minor Sixth	19.0	19.0	18.0	12.5	63.0

The musical interval judged more unitary: 229 times
The non-musical interval judged more unitary: 219 times

(2) As to the question of growth in degree of fusion through familiarity, it appears, in the first place, that the degree of fusion of wholly unfamiliar intervals was initially quite high; and, in the second place, that no increase of fusion could be noted during the course of the experiment.

(3) Thus we are reduced to a consideration of fusion of the simplest sort as a multiplication of factors. We may presume that pleasantness is not one of these factors, and that to bring in some ultimate fact of fusion —a Kempian peculiarity of the sensory material—would be no more than a confession of ignorance. We no longer need any vague factors to explain the hanging together of certain scattered intervals throughout the bitonal range.

First among these factors stands roughness. This factor is plainly a determinant of fusion, but a very instable determinant.

Richness and brightness and like factors were not brought out any too well by the procedure in this experiment, but it may be presumed that certain preferences, like the interval just above the fifth to the fifth itself, may be due to this system of meanings; and individual introspections on this particular case seem to bear this statement out.

Coherence-disparity as a criterion, seems to have a double aspect. Proximity of pitch is well known to have an associative effect in melody.[41] It has a definite fusional effect with some *O*s, and is so noted in introspection.

Familiarity supposedly plays some small part in 'fusion.' Musical properties disturb the judgment. It may be true that *nothing* was familiar on the Stern variators. Against this possibility and also against the contention that non-musical intervals might subjectively, before judgment, be assimilated to adjacent musical intervals, we can oppose the wholly unsolicited reports of two *O*s that "about half of the intervals given seemed to be not musical."

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[41] O. Ortmann, *Psychol Monog.*, 35, 1926, no. 162.

The significant phenomena in our results appear as slight and constant fluctuations rather than as sharp and definite changes, and thus take us back for comparison to Wertheimer's factors of configuration in the visual field. In the tonal complexes the intimacy of grouping varies, and it varies with the interplay of a number of definable factors. Intervals would thus seem to be configurations of exactly the same sort as groupings in the visual field.

IV. SUMMARY

(1) If we recognize the fact that there are *degrees* of fusion, we find that the most non-musical intervals are as well fused as the most musical intervals, including the perfect fifth.

(2) The simple intervals and those on the tempered scale are equally well fused.

(3) Fusion is not all dependent upon simplicity of vibration-ratio.

(4) Pleasantness is not a determinant of fusion, although fusion may, of course, determine pleasantness.

(5) Familiarity has little bearing on fusion. Similarity of the two components leads to some effect, but this takes place only in the case of the octave.

(6) A fusion is a configuration, and as such is the resultant of the interplay of various aspects of the interval.

MUTUAL INTERFERENCE IN THE EVOLUTION OF CONCEPTS

By J. A. Gengerelli, University of Wisconsin

Long ago, James, with his usual clearness, described an act of conception as resulting "from our attention singling out some part of the mass of matter for thought which the world presents, and holding fast to it without confusion."[1] Stated in behavioristic terms, we would probably omit the factor of 'attention' as a causal agent, and say simply that in conception the organism, as a result of a process of conditioning, responds to a stimulus pattern which constitutes a part of the total object as it exists in the external world. Of James' description of the process of evolution by which we obtain the concepts we use in everyday life, everyone knows. "What is associated now with one thing and now with another tends to become dissociated from either," he writes, "and to grow into an object of abstract contemplation by the mind."[2] "One might," he continues, "call this the *law of dissociation by varying concomitants.*"

Regardless of how the above description has been accepted by modern psychologists, no one, to our knowledge, had undertaken an adequate experimental investigation of the evolution of concepts until recently. Hull, however, using a modification of Chinese symbols, carried on a rigorous piece of research which corroborates in the essentials the evolutionary process as described by James;[3] but such an investigation of the problem as Hull's at once suggests the problem most closely associated with it—that of the mutual *interference* of concepts in the process of their evolution.

Here again, it is James who most clearly and convincingly states the facts. The following passage is classic. "Every reality has an infinity of aspects or properties. Even so simple a fact as a line which you trace in the air may be considered in respect to its form, its length, its direction, and its location. When we reach more complex facts, the number of ways in which we may regard them is literally endless. Vermillion is not only a mercury-compound, it is vividly red, heavy, and expensive, it comes from China, and so on, *ad infinitum.*"[4]

Thus at any given moment we react only to certain given parts or aspects of an object and ignore the rest. In fact, with respect to some of the things with which we come in contact daily, we have become so accustomed to conceiving of them or reacting to them in such definite stereotyped ways that it is only with difficulty that we can be brought to have a different 'slant' on them. As our author quaintly remarks, "we are so stuck in our prejudices, so petrified intellectually, that to our vulgarest names, with their suggestions, we ascribe an eternal and exclusive worth."[5]

It is at this point that the problem arises. Is it really true that if we have learned to conceive a thing in a certain way, it will be relatively more difficult to conceive that same thing in a second and different way? More specifically, would more effort be required to conceive the thing in the second way, after having first learned to react to it the first way, than if the first way had not been learned at all.

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[1] W. James, *Principles of Psychology*, I, 461.

[2] *Ibid.*, p. 506.

[3] C. L. Hull, Quantitative aspects of the evolution of concepts, in *Psychol. Monog.*, 28, 1920, 1-85.

[4] James, *op. cit.*, II, 332.

[5] *Ibid.*, 334.

METHOD

Would it, therefore, be more difficult for a group of *S*s to evolve the 9 'concepts' or common elements contained in the material shown in Fig. 1, taken horizontally, after having evolved nine other 'concepts' or common elements from the *same* material, taken vertically?[6] To take account of the practice effect involved, a control group of *S*s was made to evolve, successively, the 'concepts' contained in the material shown in Figs. 3 and 4.[7] By subtracting the average amount of labor required by the control group to evolve 'concepts' or common elements contained in the material in Fig. 4 from the average amount required to evolve those contained in Fig. 3, we get a quantitative expression of the practice-effect alone. If we likewise subtract the average amount of effort required by the test group to evolve the set of 'concepts' contained in the material in Fig. 1, taken horizontally, from the average amount required to evolve the set of 'concepts' from the material, taken vertically, and subtract this difference from the difference obtained in the case of the control group,[8] we get a result expressive of the amount of interference involved, if the factor of interference is really present.

The cardboard squares on which the symbols were drawn, as shown in Figures 1, 3, and 4 were cut into 81 2-centimeter squares, which were then posted on a long narrow band of oilcloth, about 2½ centimeters apart. Care was taken, however, that no two symbols having the same common elements should follow each other consecutively. The exposure apparatus is that described in Hull's work.[9]

S was seated comfortably before the apparatus, the aperture at a level with his eyes. He was then handed the following typewritten directions. "I have here in this apparatus a number of figures of different designs. I will expose every figure for 5 sec., and shortly after it is exposed I will pronounce a letter of the alphabet, which you will pronounce aloud after me. The purpose of this experiment is to see how long it takes you to associate certain letters of the alphabet with certain figures which will be shown you. If, after you've observed a figure a few times you think you know the letter, which is associated with it, then speak the letter aloud before I do, so that I can tell whether you know it or not. If you are not quite sure whether a certain letter is associated with a certain figure, then guess. This experiment is not very hard, and, besides, you will be given a short rest every few minutes.[10] During these intervals you are urged to keep your mind off of the material."

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[6] It will be observed that every symbol has two possible common elements. As will be seen from Fig. 2, each significant element or character is to be associated with a certain letter of the alphabet. Taken vertically, therefore, all the symbols contained in the first left hand column of Fig. 1 were to be associated with the letter 'C;' those in the second column with the letter 'I'; those in the third column, with 'R' and so on to those in the ninth column, which were to be associated with 'E.' Taken horizontally, all the letters in the top row were to be associated with 'K;' those in the next row, with 'Y;' those in the third row with 'N,' and so on. The *S*s, both test and control evolved, consequently, two series of 'concepts' successively; these we shall designate as the *initial* and *secondary* series, respectively.

[7] The possibly unequal difficulty of the two series of common elements to be evolved was met, by having one-half of the test *S*s evolve series A—the series drawn horizontally across the square in Fig. 1— as the initial series; and series B— the series drawn vertically,—as the secondary series; and the other half to proceed the other way about. The same, of course, was observed for the control group—one-half of the *S*s using A_1 as the initial series; the other half B_1.

In order to make the material used by the control group as much like that used by the test group as possible, 81 small figures, each different from every other, were drawn to take the place of one of the series of common elements. By consulting Fig. 1, it will be observed that the symbol in the upper right-hand corner, for example, consists essentially of a parallelogram with a 'three-leaf' in the center and an 'X' on the outside. The same symbol in the corresponding position in Fig. 3 shows the 'three-leaf' on the inside in the same position, but on the outside, instead of the 'X,' a new figure. In Fig. 4, it is the 'three-leaf' which is absent, its place being taken by the figure, and the 'X' is on the outside as in the test material.

[8] We assume, of course, that the two groups of *S*s are comparable in ability.

[9] *Op. cit.*

Every *S*'s record showed the number of 'prompts' required to evolve a given series of 'concepts.' As every symbol appeared in the aperture of the exposure apparatus, *E* pronounced the letter to be associated with the significant element contained in that symbol, and this counted as a prompt. Whenever *S* responded correctly to the symbol exposed, *E* would say 'right,' and, of course, no prompt was given. If an incorrect letter was given, *E* supplied the correct one, and charged the *S* with a prompt.

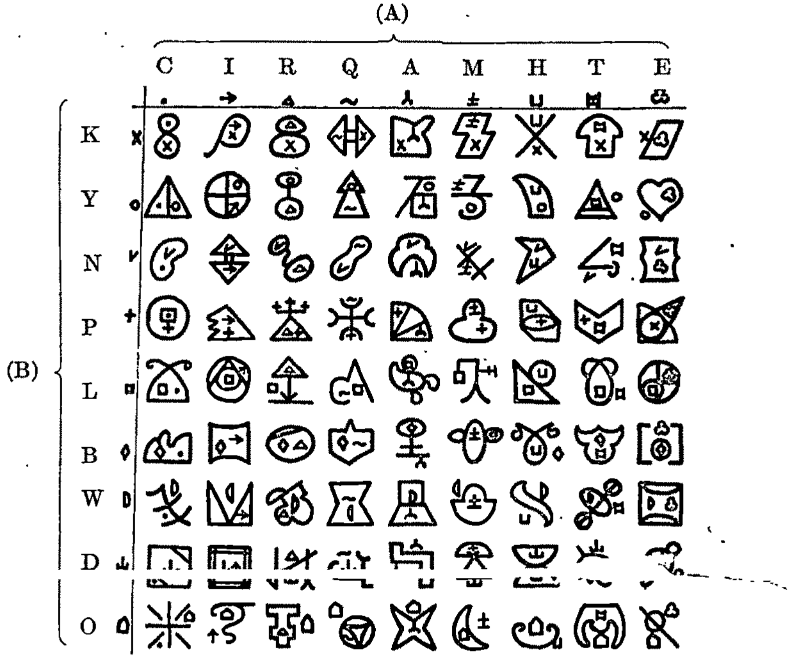

FIG. 1

The criterion adopted as indicative of complete learning, or rather, the successful evolution of the given series of 'concepts,' was that the *S* go straight through the list and respond correctly to every one of the 81 symbols.[11]

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[10] After the 27th and 54th symbol, the exposure apparatus was stopped for a period between 15 and 20 sec., and the *S* asked to rest his eyes by letting them wander about the room. At the end of the series of 81 symbols, that is, before the same series was repeated, *S* was given a rest between 45 and 60 sec.

[11] The question may be asked at this point as to how assurance was obtained that the *Ss* responded to the 'dynamic core,' so to speak, in the symbol-complex rather than to the symbol-complex as a whole. It was ascertained that, as a matter of fact, the *Ss* found it impossible to comply with the criterion by 'brute memory' in the time allowed. There were several *Ss* who tried this and succeeded to a meager extent, but they soon gave it up. Whenever an *S* was responding to the total symbol-complex, his responses were always hesitant and his errors very erratic. Furthermore, when the experiment had been finished, the *Ss* invariably made some such remark as "I had an awful easy time singling out that arrow at first," or "It's easy after you get on to the idea;" which leaves no doubt as to the nature of the process.

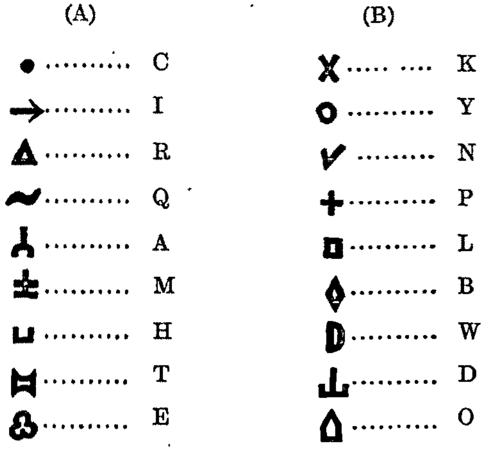

FIG. 2. The two series of 'concepts' (common elements) and the letters to be associated with each.

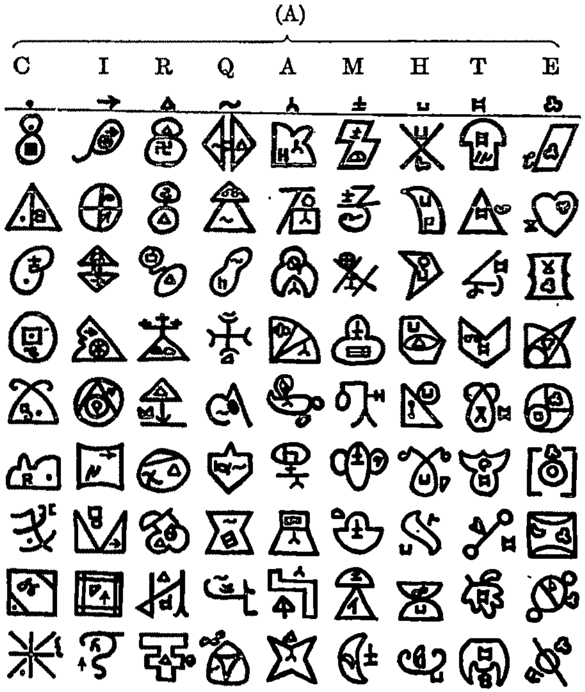

FIG. 3

Results

The results of the experiment are given in Table I. It will be observed that 24 *S*s constituted the test group and 24, the control group. The table gives the scores of every *S* individually for both the initial and secondary series of 'concepts.'

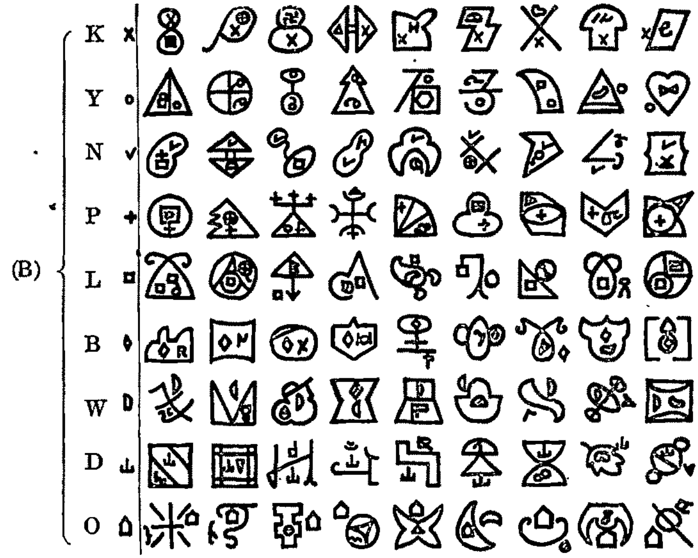

Fig. 4

It is obvious, that the average number of prompts required by the test group to evolve the secondary series of 'concepts' must necessarily be much smaller than the average number required for the initial series. For, after having evolved the initial series, the *S*s would no longer stare at each symbol exposed in its entirety, but would seek out the significant element. In the words of one of the *S*s, they would know that "it isn't the whole thing that counts, but just a part of it." This is true, of course, for the control group as well. The control *S*s evolved the same 'concepts' from the same material, but the material, in their case, was so prepared as not to give rise to any interference; so that if our set-up was adequate and the factor of interference really present in the case of the test *S*s, we should expect that the average number of prompts required by that group to evolve the secondary series of 'concepts' would be distinctly larger than the mean number required by the control *S*s to evolve the secondary series. Table I shows this to be the case. The average number of prompts for the test group being 86, and for the control group, 57. There is a difference of 29 prompts, with a probable error of 7.9. The difference is 3.67 times greater than its probable error.

A second and better way of computing the results, is to compute for every *S* the percent reduction in the number of prompts required to evolve the secondary series over the number required for the initial series. We may thus obtain the mean percent reduction for each group of *S*s. Here, if the factor of interference really exists in the process under consideration, we should expect the mean percentage decrease for the control group to be distinctly larger than the mean percent decrease for the test group. Reference to Table I shows, in fact, the mean percent decrease for the control group to be 75, that for the test group 57. A difference of 18 points, with a probable error of 3.86, the difference being 4.66 times greater than its probable error.

TABLE I

Test Group Concepts from material in Fig. 1				Control Group Concepts from material in Figs. 3 and 4			
	No. prompts required		%		No. prompts required		%
	Initial Series.	Secondary Series	Decrease* in Secondary		Initial Series.	Secondary Series	decrease* in Secondary
Evolving series A as initial series; B as secondary	61	38	38	Evolving A_1 (Fig. 3) as initial series; B_1 (Fig. 4) as secondary	152	55	64
	153	140	8		277	113	59
	291	167	43		124	51	59
	79	29	63		138	79	43
	118	61	48		442	88	80
	138	70	49		317	44	86
	254	100	61		143	81	43
	374	125	67		117	37	68
	271	80	70		453	130	71
	177	122	31		206	40	81
	197	81	59		370	110	70
	219	219	0		62	21	66
Evolving series B as initial series; A as secondary	159	46	71	Evolving B_1 (Fig. 4) as initial series; A_1 (Fig. 3) as secondary	243	56	77
	263	88	66		89	21	76
	160	41	74		386	37	90
	348	74	79		417	40	90
	343	77	77		324	31	90
	263	84	68		345	73	79
	171	38	75		459	60	87
	208	36	82		450	31	93
	515	27	95		301	68	77
	96	69	28		95	31	67
	298	195	35		280	27	90
	384	55	85		235	35	85
Ave.	231	86	57	Ave.	268	57	75
m.v.	88.75	39	19.5	m.v.	112	24.4	11
P.E.$_n$	15.3	6.7	3.36	P.E.$_n$	19.3	4.2	1.9

*In computing the decrease, anything over .5 was counted as 1.

To the question, then, as to whether it is more difficult to conceive a thing in a second way after having first learned to conceive it in another way, than if the first way had not been learned at all, the results of this experiment would lead us to answer in the affirmative.

Although the experimenter made no systematic effort at securing introspections from the *S*s as to the nature of the interference or conflict they experienced, a number of *S*s were questioned on this point after the secondary series of 'concepts' had been evolved. All admitted being confused and at loss for at least the first 18 or 20 syllables. They expressed themselves as still responding to the original common elements during this time, and not knowing what to make of the fact that the common element which in the previous series had been designated by one letter was now apparently designated by another.

It will also be noticed from Table I that the initial series for the control group has a slightly larger mean than the initial series of the test group. There is a difference of 37, but the probable error, 24.6, is so high, that the difference is hardly due to any difference in the difficulty of the two sets of material used. But even if the two sets of material used by the two groups were of unequal difficulty, if we assume that the two groups were comparable in ability—and we feel that there is every reason for this assumption, as the *S*s were selected from the introductory course in psychology and divided into two groups haphazardly—the validity of the results expressed in terms of the average percentage decrease would not be disturbed, since these express relative and not absolute values.

TABLE II

	The number of prompts required		The number of prompts required
Evolving as initial series the common elements (A) from the material in Fig. 1	61	Evolving as initial series the common elements (B) from the material in Fig. 1	159
	153		263
	291		162
	79		348
	118		343
	138		263
	254		171
	374		208
	271		515
	177		96
	197		298
	219		384
Evolving as initial series the common elements (A_1) from the material in Fig. 3	152	Evolving as initial series the common elements (B_1) from the material in Fig. 4	243
	277		89
	124		386
	138		417
	442		324
	317		345
	143		459
	117		301
	453		95
	206		280
	370		235
	62		450
Ave.	214	Ave.	285
m.v.	94.3	m.v.	92
P.E._n	16.2	P.E._n	15.8

$$285 - 214 = 71 \text{ difference}$$
$$\text{P.E.}_D = 22.6$$

Table II contains results that justify the technique used with the intent purpose of equalizing difficulty of the two series of common characters or 'concepts' used. The first column gives the results for those *S*s, regardless of the group to which they belonged, who evolved as the initial series the common characters (A) or (A_1) from the material in Figs. 1 and 3 respectively; the second column, for those evolving as the initial series, the common characters (B) or (B_1) from the material in Figs. 1 and 4, respectively. As will be seen, there is a decided difference in the degree of difficulty of the two series of common characters. A difference of 71, with a probable error of 22.6—the difference is 3.14 times greater than its probable error. This is exactly what could have been predicted from the behavior of the *S*s during the experiment. Nearly all the *S*s had greater difficulty with the series of common elements drawn vertically in Fig. 1. They invariably confused at first the character 'X' (K) with the character '+' (P), and the character '◇' (B) with the character '□' (L).

A question that suggested itself again and again during the course of the experiment was whether the individual who obtained a high score in the evolution series—*i.e.*, who required a small number of prompts—also secured a high score in the conflict series. The coefficient of correlation was therefore calculated between the initial and secondary series for the test as well as the control group.

The r between the initial and conflict series for the test group of *S*s was + .151 ± .134; for the control group, + .3644 ± .119. The difference between the two 'rs' is .2134, and the probable error of the difference is ± .178. There seems, then, to be a very low correlation between the initial and conflict series for the control group of *S*s, whereas, for the test group no relation seems to exist; that is, there is no possibility of predicting what any given individual will do in the conflict series by what he has done in the evolutionary series.

It might be of interest and significance to find out whether over-learning would influence the amount of interference occurring in the secondary series of concepts one way or the other. For in actual life, with respect to our everyday concepts, because of the vast amount of repetition which they undergo, "we are so stuck in our prejudices, so petrified intellectually" that we can't observe anything but that one side of the complex object which we have been taught to recognize. Now, if in having the *S*s evolve the initial series of 'concepts,' we were to use as the criterion of the satisfactory evolution of the series, not that the *S*s go straight through the list of 81 symbols *once* without a single prompt or error, but that they go, say, ten, twenty or thirty times without a single prompt or error, would the amount of interference be greater or smaller? In such an experiment a greater number of *S*s would very probably be needed to insure a low probable error.

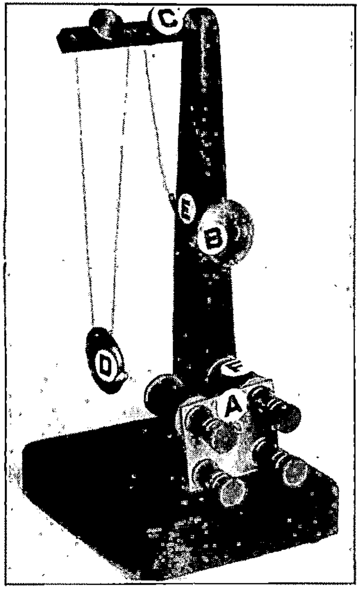

Fig. 1

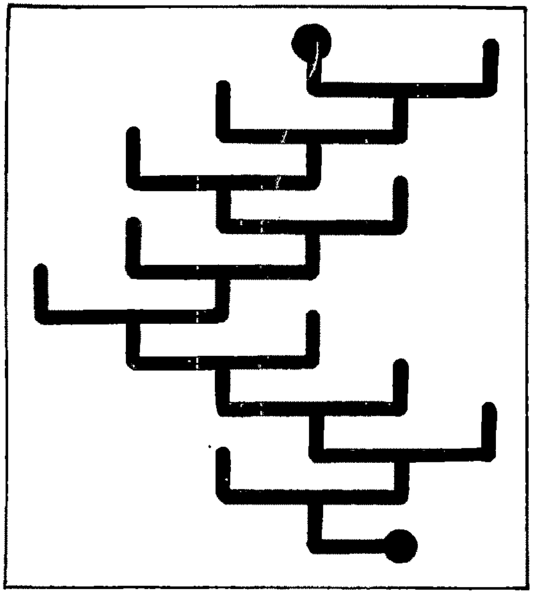

Fig. 2

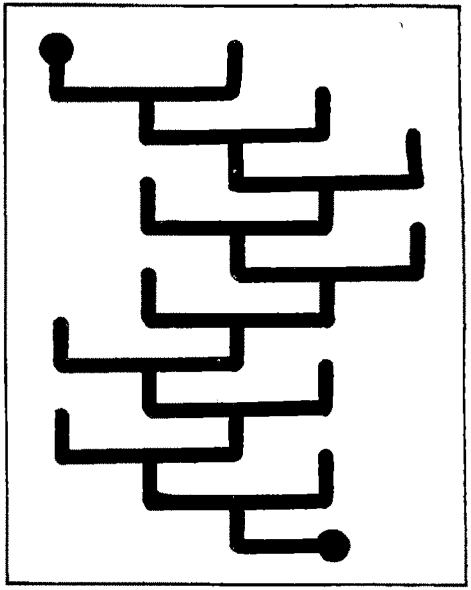

Fig. 3

APPARATUS

SOME SUGGESTIONS IN LABORATORY APPARATUS

By CHRISTIAN A. RUCKMICK, University of Iowa

I. SANFORD CHRONOSCOPE WITH ELECTRICAL RELEASES

The writer carried on some correspondence with the late Dr. E. C. Sanford in regard to an electrical release for his well-known reaction chronoscope. Sanford was very enthusiastic about the proposal and stated that when he first devised the apparatus he had an electrical release in place of the usual mechanical key. The reason why it was discontinued, he said, lay in the fact that at that time the mechanical key was more suitable for the psychological laboratories of that day. Since then, with the improvement of laboratory technique, there has been more need for an electrical release, especially when the observer is removed from the room in which the control apparatus is situated. But even under the ordinary conditions when *E* and *O* work in the same room, the release can be made more quiet and quick through electrical keys than in the mechanical type. Accordingly, at the psychological laboratory of the University of Iowa, we are using only chronoscopes with electrical releases.

These chronoscopes were made by the departmental machinist in accordance with suggestions from the writer and include a number of improvements. The illustration, Fig. 1, shows most of the essential features. At A are located the two electro-magnets with binding posts mounted on insulated material. One of the magnets is marked F. Just below A is an adjusting screw which regulates the relative position of the magnets since the whole square may be slightly rotated in order to bring the magnets into the optimal position with relation to the bobs. These bobs (B and D) are made of brass with iron rivet-head inserts opposite the cores of the magnets. The length of the threads attached to the bobs is regulated by two thumb screws one of which is illustrated at C. The silk threads pass through holes in the horizontal brass bar suitably beveled to prevent cutting the threads and are wound around the ends of the thumb screws in opposite directions so that a single turn of the screw winds up both strings equally for each bob.[1] When the instrument is carried from place to place the bobs may be hung up on hooks indicated at E on the vertical pillar. The construction throughout is rugged and is finished with the new crystalline type of japanning. Three small rubber feet protect the tops of tables on which the instrument may be used. Obviously, the release operates on a break circuit, but through proper instructions to the observer the current can be economically used. One dry cell in good condition is ordinarily sufficient to operate the magnets. Of course, specially treated iron cores must be used to prevent permanent magnetization.

II. MODIFIED FORMS OF THE STYLUS MAZE

Some years ago Warden[2] suggested a form of stylus-maze which he used in connection with a special study in learning. The form which he used is illustrated in Fig. 2. It was suggested to us that this type of apparatus might be suitable for laboratory exercises in the field of motor learning. In the Iowa laboratory, since many students in the college of education enroll for the purpose of acquiring a more intimate knowledge of psychological laboratory technique, we have frequent occasion to stress experiments in learning. For this purpose we have made two types of maze—one illustrated in Fig. 2 following closely the dimensions specified in Warden's article. The maze is made of ⅛ in. sheet brass and measures 12¾ x 11¾ in. The width of the path is ¼ in. throughout. The circles at the beginning and end are ¾ in. in diam. This type of maze has the advantage of presenting practically two discreet problems when both sides are used. To vary the procedure we have devised another similar maze illustrated in Fig. 3.

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[1] This instrument was perfected several years ago. Since then a note on a somewhat similar improvement in the adjusting screw has been published by S. W. Fernberger, (this JOURNAL, 37, 1926, 154-155). The attachment of the threads to the thumb-screw is almost identical with Fernberger's arrangement. In both cases the idea is to adjust the length of the bob without giving it a cant.

[2] C. J. Warden, The relative economy of various modes of attack in the mastery of a stylus maze, *J. Exper. Psychol.*, 7, 1924, 243-275.

The maze may be used for a variety of procedures. Two, which we commonly use in the laboratory, are to time the number of errors and the time of running the maze with a stylus or pencil without preliminary visualization as compared with learning to run the maze with a 5-sec. previous visual exposure. It is not necessary to use glass as a writing surface as is suggested in the article cited. A smooth surfaced table with a sheet of paper under the maze will give accurate results for our purposes. Instructions are preferably typewritten in either case.

We have also used the apparatus in connection with experiments on emotion when the effect of reward or punishment was used in connection with the number of wrong tracings—that is, into the blind alleys. Of course his could be a separate experiment in learning but our purpose was to create some fear of punishment in the form of electrical shock or scolding in order to get a description of the emotional state. Variations may include trying the test from the right and from the left in passing from entrance to exit as well as from bottom to top, and often top to bottom. In these cases the different types of learning will be brought out in so far as motor images conflict with visualization or in so far as one of the other types of imagery is used predominantly in the learning series. In all cases it is a good device for getting qualitative reports on the learning consciousness as well as the quantitative data.

AN IMPROVED HEAT GRILL

By H. G. Bishop, Wittenberg College

All the earlier forms[1] of the heat grill have certain limitations to escape which the form described here and shown in Fig. 1 was devised. The dimensions, material, number of coils, etc. are not specified because such matters must be left to the needs of the specific experiments.

An inspection of the accompanying drawing, Fig. 1, shows three main units: the coils, the supporting stand, and the capstan. The coils are supported by silk fishing line running from the capstan spool through holes in the stand top to the coils themselves. The capstan is held up by a coiled spring which is sufficiently strong to support the load of the temperature coils. A pin through the shaft of the capstan limits the upward excursion.

The capstan is capable of two kinds of movement: it may be depressed and raised; or it may be rotated in either direction. If it is depressed from the neutral position shown in Fig. 1, both systems of coils will be lowered without altering their alignment in the horizontal plane, and both systems will simultaneously come to rest upon the arm of the observer as it lies beneath the supporting stand. If the capstan is rotated the position of the two temperature systems is altered reciprocally in respect to one another. Counterclockwise rotation, which is indicated, lifts one system—the clear—and lowers the other—the shaded. A slight turn is sufficient. When the capstan has been rotated through the proper angle and in the right direction, the system of coils desired will be handing freely below the other. If the capstan is then pushed downward the lower system alone will touch the observer. Clockwise rotation will, obviously, bring the systems back to their original positions ready for simultaneous stimulation by both coils or, if the rotation has been beyond the original setting of the capstan spool, by the hitherto unused system.

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[1] F. Cutolo, Jr., A preliminary study of the psychology of heat, this Journal, 29, 1918, 442-448.
E. B. Twitmyer and S. W. Fernberger, A demonstrational heat grille, this Journal, 38, 1927, 119.
N. C. Burnett and K. M. Dallenbach, The experience of heat, this Journal, 38, 1927, 418-431.

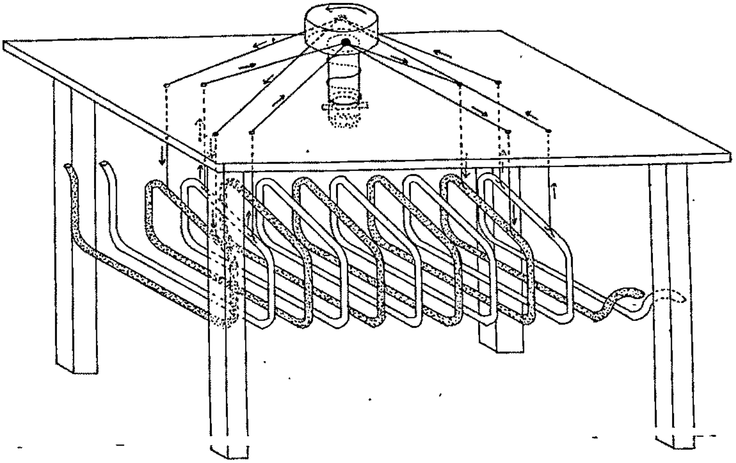

FIG. 1

The following are advantages of this instrument over the earlier forms. (1) Stimulation may be rapidly altered between the warm alone, the cold alone, or by both temperatures simultaneously. (2) The temperature changes may be effected without the observer changing the position of his arm, which is not only advantageous in itself but which also permits repeated stimulation of the same part of the skin. (3) Since the coils, if the capstan is turned until the supporting strings are slack, will bear down with the amount of their own weight the stimulation will always be at the same pressure. Variations of the pressure of the arm upon the grill, such as Dallenbach noted,[2] are obviated by this arrangement.

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[2] Burnett and Dallenbach, *op. cit.*, 425.

SOME SUGGESTIONS REGARDING THE CONSTRUCTION OF APPARATUS AND TESTS

By W. C. Wideman, C. H. Stoelting Co.

The economical manufacture of apparatus and laboratory supplies is of interest to all experimental psychologists. I therefore wish to bring to the attention of all those who devise psychological apparatus and tests, some of the unnecessary expenses that are frequently met by the manufacturer.

One of the most frequent sources of trouble and expense are the toys, pictures, and household specialties so often used in mental tests. A psychologist goes into some general or department store and selects some materials that seem to meet his requirements and then proceeds to devise his series of tests. In the natural course of events, if he has produced something of real value, it is not long before other psychologists wish to use similar material. About that time every one interested discovers to his astonishment and dismay that many of the articles are off the market. The particular novelty or specialty has had its day and is superseded by something else. Often the item that takes its place is quite different, and as more often happens, the item has been a commercial failure and the plates or tools have been 'junked.' Under these circumstances the apparatus manufacturer may have to go to the expense of producing new tools, or plates, and, as happens in some cases, pay the original producer a heavy royalty for the use of copyrighted plates or patented mechanical devices. When a thing is taken from the market the original producer can rarely be induced to supply it at anything like a reasonable price in the comparatively small quantities used for psychological purposes.

The same facts apply to supplies for demonstrational and experimental purposes. Electrical and spring motors, etc. have their hour and day and sooner or later are superseded by something more practical from a commercial point of view, but often not so well adapted for use as a part of a special psychological apparatus. Under the conditions of a changing commercial supply it would be more economical in the end if greater care were exercised in the selection of the materials.

So as to keep the cost of production down to a minimum, it would be advisable in devising new tests and apparatus, to determine in advance how long the item one contemplates using will probably be on the market in its present form, or else to make some arrangement with the original manufacturer for a continuance of the supply. If neither of these courses is possible, it would in many instances be economy in the long run to produce at the start the exact type of picture, device, or other material required.

BOOK REVIEWS

Psychological Types of the Psychology of Individuation. By C. G. JUNG. Translated by H. G. Baynes. New York, Harcourt, Brace & Co., 1923, pp. xxii, 645.

The author finds in his "pratical medical work with nervous patients" (p. 9) some typical varieties, of which two are specially clear: *introversion* and *extraversion.* Description is embarrassed in that no individual is a pure type, each tending to compensate for his own one-sidedness; there is a merely relative predominance of one mechanism (13). The two phases indeed alternate with a kind of natural rhythm (systole and diastole); but one of them gradually dominates the conscious attitude while the other recedes into the uncounscious (11). No type is completely known until both aspects (conscious and unconscious) have been surveyed. These types are due to native differences and appear at random in all strata of society (413). In general the extravert finds satisfaction in the object, his social and physical milieu, which alone has significance for him; whereas the introvert is concerned with his own self and psychic processes, the environing world being incidental and secondary. The former adapts by fitting himself to the world, the other by protecting himself from the world (414); the former is the genius of civilization and practical achievement, the latter of culture and abstract invention (352).

(*A*) *The extraverted type.* The extraverted type consciously adopts the prevalent attitudes and code of his fellows, and represses contrary egoistic tendencies; but these break through from time to time and confuse the picture. Hysteria is the typical neurosis, with its exaggerated rapport (416 ff.).

(1) The *extraverted thinking* type tries to bring the whole of life into harmony with what he conceives to be world-law, as revealed in objective data; he seeks to regulate his activity by intellectual formulae, which leave no room for affective (irrational) tendencies; these outlawed feelings take revenge by undermining his conscious convictions, and he in turn overcompensates the doubts by fanatical belief and sacrifices (434 ff.).

(2) The *extraverted feeling* type is held in spell by objective standards of some sort; a thing is felt to be beautiful because that is the 'correct' way to feel about it. This attitude, found mostly among women, conduces to harmony in social life. Such a woman marries a man who is 'suitable;' this demands no simulation of love on her part because she just naturally falls in love with a suitable man. Thinking remains infantile, negative, and in all cases the servant of feeling (488 ff.).

(3) The *extraverted sensation* type is the most realistic of all; he lives in the concrete experience of the moment, and is ever seeking new sensations, new thrills. He has no purpose but 'carpe diem,' is likely to become credulous and captious, and condemns the restraints of reflection (457 ff.).

(4) The *extraverted intuitive* type depends upon sensation only as a guide for intuitive penetration. He is not satisfied with the sensory realities of a situation, as is the preceding type, but looks ever to its future possibilities; when a thing's novelty is exhausted he throws it aside; he is the adventurer, promoter, who is cramped and stifled in static conditions of life (464 ff.).

(*B*) *The introverted type.* The introverted type is controlled by subjective factors; the object is of concern only because of what it constellates or invokes in himself. It is quite unfair, however, to call him autoerotic or egoistic; because in every psychic function the subject is just as indispensable as the object. The assumed superiority to the milieu is compensated by an unconscious fear of it, which thwarts his inner freedom; he is terrified because the world moves without his permission or control. The prevailing philosophy of the West being extraversive, he feels oppressed and so works ever harder to protect himself against the pressure of society; his typical neurosis is psychasthenia (471 ff.).

(1) The *introverted thinking* type is concerned less with new facts than with new theories or insight; he looks upon facts merely as evidence for his theories. He is scrupulous and painstaking in his work, stubborn in pursuit of his own ideals; he cares little about influencing others, as in teaching, and gives little heed to their judgments. He thus appears inaccessible and is likely to be misunderstood. Kant belongs here and Darwin belongs to the corresponding extraversive group (484 ff.).

(2) The *introverted feeling* type is represented largely by women, who are slient and melancholic; they find it even harder to communicate than does the introversive thinker. They are consciously indifferent and placid, but may compensate with overweening vanity and cruel ambition. The extreme cases are neurasthenic (492 ff.).

(3) The *introverted sensation* type never stops with the pure sensory effect of stimulation, but suffuses the percept with subjective additions; two artists of this type would paint the same landscape very differently. He tries to see a thing *sub specie aeternitatis* instead of with its direct, momentary aspect; he is likely to misinterpret what he perceives and to move in a mythological world (500 ff.).

(4) The *introverted intuitive* type perceives the background-processes of consciousness much as, in ordinary sensation, we perceive external objects. His own unconscious images become objects of perception to him and may seem so detached that he is quite unaware of their source in his own being. Here belong the prophets of Israel, the mystics and dreamers and seers, who live in the exuberant profusion of their phantasies. The outer expression is likely to be feeble and unimpressive to his contemporaries (a voice crying in the wilderness), but his posthumous influence may be very great (508 ff.).

In the early part of his book, Jung surveys many fields of life and thought seeking evidence to support his theory of opposing types. Thus, the strife of Tertullian and Origen in the early church arose because the introverted Tertullian, after a *sacrificium intellectus* (*certum est quia impossible est*), finds reality in feeling; whereas the extraversive Origen, who escapes from self by a *sacrificium phalli*, finds truth by intellectual apprehension of God (20 ff.). The realists who maintain that universals exist *ante rem* are introverted, the nominalists who derive them *post rem* are extraverted (37 ff.). The extraverted Freud finds adjustment in life by using external objects to satisfy instinctive (sexual) needs, the introversive Adler seeks to make man superior to his environment (78 ff.). Schiller contends that modern culture subordinates man to the object, reducing him to a mere instrument of collective advance. This extraversive emphasis in modern society leads to dissociation of thought and feeling in the subject and disturbs the balance and unity of his life. To Jung, however, this lack of balance is due not primarily to external institutions or social pressure, but to our own nature (87 ff.). Nietzsche treats of the same opposition in his contrast of Apollonian and Dionysian; the former has to do with an inner world of phantasy, whereas the other yields himself, with unbridled dynamis, to the instincts which bind him to the group (Seid umschlungen, Millionen) (172-9). In Spitteler's "Prometheus und Epimetheus" the same contrasts appear. Prometheus is the introvert toiling amid his own ideas, while Epimetheus is the conventional man of action with 'correct' ideas, at peace with the world, righteous and self-assured. This extraversive attitude however stifles the highest values; because what once was good does not remain good forever. Changes in racial attitude (*e.g.* religion) are slow and painful; the Epimethean influence persists and cumulates until a true Prometheus comes to kindle for the world a new flame (207-29). In Hindu theology the strife between opposing tendencies is resolved in Brahman

(245); in Chinese philosophy Tao is the middle path which frees man of the struggle with himself (264).

In all these cases, how is deliverance to be gained from the clash of introversive and extraversive forces, of conscious with unconscious? Various methods have been used. The Indian technique is to withdraw attention (libido) from conscious contents; whereupon a kind of hypnotic state ensues wherein the primordial and repressed tendencies of the unconscious are activated and released (250). In western religions deliverance is often symbolized by a divine birth, a god-renewal, a messiah who breaks the shell of collective morality and brings a new way of life (241). Jung finds the way of salvation in *phantasy* or play. Man is only completely man when playing (135). Phantasy is a kind of serious play, wherein the extravert's unconscious introversive tendencies are given release, and likewise the introvert's outlawed extraversive tendencies. From the unconscious springs creative phantasy and religion as well. There is always, between religion and man's actual mode of life, a compensatory relation (174); that is, religion voices the repressed forces which secure no adequate release in ordinary life. Salvation then always begins with the unconscious, that despised region to which is exiled all that is consciously regarded as ugly, immoral, useless; but the dominant conscious forces so abominate these exiled ideas that they crucify the very savior who brings deliverance. "He (the Savior) hath no form nor comeliness—He is despised and rejected of men" (323).

Otto Gross proposes two psychological types, based upon the concept of primary and secondary functions. By a primary function is meant an actual performance of the cells (*e.g.* a representation) which of course dissipates energy; the secondary function is a process of assimilative renewal or restoration. A brief period of restitution implies large capacity for deviation, rapid and superficial adaptability, a flitting from idea to idea; whereas a long secondary function causes the subject to perseverate and form complexes; he becomes intently absorbed with his inner world and may develop excessively valued ideas. The extravert is hampered in unifying his own personality because it is determined by the flux of outer events, while the introvert is hindered by his tendency to form complexes, which break his life into fragments. Intensity of the primary function is the decisive factor for Jung: the introvert has a strong primary response and a correspondingly long period of restitution: the extravert has a weak primary and brief secondary function (337 ff.).

In the field of esthetic appreciation, the extravert finds that the world gains life and value only through his empathic identification with it; whereas the introvert, overpowered by the confusion of the world, seeks esthetic satisfaction in abstract forms (358 ff.).

What James calls the "tender-minded" and "tough-minded" philosophers (372 ff.), and Ostwald's well-known classic and romantic types among men of science (401 ff.), both reduce to forms of introversive and extraversive types.

No critical appraisal of Jung's book will here be attempted. His major ideas are coming into general knowledge and are being reduced to experimental form. The present work is a fruit of much clinical experience and is full of keen observation and illuminating comment. His survey brings into review whole fields of literature which the ordinary psychologist would never touch. Two comments may suffice:

(1) The elaborate symbolism in many parts of the book was much against my palate and little more intelligible than the poems and philosophies which he seeks to elucidate. I am the last to damn a piece of work merely because the writer uses a terminology differing from my own; if a man has something to say I care not whether he puts it in terms of 'libido', or 'gestalt', or 'subvocal speech responses', or 'conscious function,' so long as

I can apprehend what he is trying to say and convert it into my own ways of thinking. Yet I confess being wearied and repelled with page after page of stuff like this: "Just as Prometheus allows all his passion, his whole libido to flow inwards to the soul, to his innermost depths, in complete dedication to his soul's service, his God also pursues his course round and round the pivot of the world, thus spending himself like Prometheus, whose whole being comes near to extinction" (220). It seems too bad that a sentence like that is typical of a book which purports to give scientific information about human nature.

(2) It seems clear that such a dichotomy (introversion and extraversion) has but limited and temporary value, just as would a division of humans into tall and short, bright and dull; a preliminary orientation of this kind must be followed by more careful analysis and exact measures. What with the dominant 'conscious' and the opposed 'unconscious,' in which are confined all the other elements of human nature in repressed or infantile form, the picture of any one type is often pretty confused. Now and then it seems that his theory would fit equally well, were the facts exactly opposed.

These faults however do not crucially impair the value of what his translator calls "Jung's crowning work."

University of Illinois ELMER CULLER

Mental Growth and Decline. By H. L. HOLLINGWORTH. New York, D. Appleton and Co., 1927, pp. xii, 396.

The design of this book is ambitiously inclusive. The author sought to provide the college student with a survey text for the whole field of developmental psychology. To bring within the compass of less than four hundred pages the substance of the theoretical and experimental literature on mental growth, the stages of development, hereditary influences, normal variations, abnormalities of growth, characteristics of infancy, of childhood, of adolescence, of mature years, of senescence, and the influence which past generations have left behind them in social institutions was as difficult as it was a commendable undertaking. Psychology is now as much in need of such survey texts in certain fields as the older sciences. All who have tried to teach genetic psychology know full well the problems confronted in attempting to give a class the general scope, the background, and the relationships of the available material on the different stages of the human life cycle. The author has faced these problems and in this volume presents his solutions.

The style is often attractively thought provoking. Instances abound of that kind of sentence which a teacher may use as the basis for developing an instructive class discussion. The chapter on the "post-mortem age" (the survival of institutions) is clever and illuminating. The discussion of maturity as a phase of genetic psychology is a notable and worthy departure from the tendency, far too long current, to think of middle life as a tableland. The last chapter, in which the many laws of development are brought together in clean-cut fashion, is an especially welcome contribution.

If the reader expects to find in this volume an encyclopedic compendium of the material of genetic psychology he will be disappointed. It is not an impartial presentation of the substance of all schools of genetic psychological research. The author has quite obviously selected his material and distributed the emphasis according to his own standard of values. This applies to the factual as well as to the theoretical material. There is a generous use of biological data and of the results obtained by the so-called objective or test methods of trait study. The contributions made by other schools of thought and study receive far less consideration. One looks in vain for the contributions of the *Gestalt* movement to genetic psychology. Köhler's work is omitted and Koffka's is but briefly mentioned. If the term 'eidetic' appears in the book the reviewer has missed it. The extensive discussions of genetic psychological problems in the literature of the psychoanalytic school receive but a disproportionate and disparaging consideration. A little of G. Stanley Hall's theorizing appears and some of his clever phrasing, but the vast collections of data which he inspired are scarcely mentioned.

The reviewer believes that many readers will be disappointed in finding no bibilographies, and footnote references are all too few. The teacher who handles this text must himself know the literature very well in order to guide the student to the sources. Fascinating ideas are frequently presented without any reference to lead the interested student into the literature from which they were drawn. But these defects should deter neither the student nor the trained teacher from much use of the book. As an introduction and a conspectus it has no rival in the field of genetic psychology.

University of Oregon EDMUND S. CONKLIN

An Introduction to Social Psychology. By L. L. BERNARD. New York, Henry Holt & Co., 1926, pp. x, 651.

Of the recent books in social psychology this volume attempts the most thorough-going systematization of the field which has yet appeared in English. The treatment is divided into four parts: "Introduction;" "The Foundations of Collective Behavior;" "The Integration of the Personality in the Psycho-Social Environment;" and the "Psycho-Social Environment and the Organization of Collective Behavior." The amount of material in this book is so extensive, the treatment so detailed in many places, that the limitations of a review hardly suffice to give more than a passing notion of its contents.

After a preliminary statement of the nature and function of science, especially as it touches the social sciences, the author reviews briefly the field of social psychology, as presented by McDougall, Allport, Williams, Ellwood, Bogardus, Ross, Gault and Dunlap. A working definition of his own is then given:

"Social psychology studies the behavior of individuals in a psycho-social situation. This behavior is valid subject matter for social psychology whether it conditions or is conditioned by other social behavior or responses. It is also concerned with all collective responses, that is, responses of individuals which mutually and reciprocally condition each other and those which are uniform throughout the group, regardless of what environment they arise from."

After this introductory discussion the writer turns to his first task, that of revealing the foundations of social behavior in the individual organism. Taking his cue from the work of C. J. Herrick and C. M. Child, he gives a very able analysis of the mechanisms of man as the highest form of animal life. While he recognizes the part which heredity plays in behavior, he emphasizes, following Child, that hereditary traits are greatly modified and expanded under the omnipresent environment. In the case of man this environment is not merely physical but, all-importantly, it is 'psycho-social,' to use Bernard's own term. The difficulty with the earlier instinct school of social psychologists is uncovered. In this discussion the author draws largely on his own brilliant analysis of the whole theory of instinct in his earlier work. The principal superiority of man lies in the development of the more complex levels of habit formation and of the higher thought processes. The latter he calls the neuro-psychic behavior which finds its higest formulations in the rise of language and symbolic thinking, those all-significant carriers of social behavior. There is also some recognition of the part which so-called unconscious processes play—especially in reference to repression and the control of deeper patterns of behavior and thought by socially determined consciousness. Thus are the activities of the so-called 'censor' explained. There is some attention to the recent work on intelligence differences, as applied to social classes, nationalities and races. After a rather inadequate review of the current literature on this matter, little further reference is made to this phase of applied psychology.

The next division of the volume takes the individual as an organism and carries it over into the social world in which it becomes a personality. There is a pertinent discussion of the importance of suggestion and imitation as these concern the development of the personality. Imitation, in fact, becomes an important concept for the author. This form of interaction, if it may be called such, ranges from a simple, direct sort of person to person, to the indirect form of imitation of ideal personalities and social standards which are found in the concepts of social values.

The final section treats in detail the nature of the psycho-social environment in which the individual lives, moves and has his being. Primary and derivative (secondary) groups and the types of attitudes and ideals which belong to each sort of group are discussed. There is a competent handling of the problem of social contact and communication. This is followed by a discussion of rational and non-rational groupings. The former are the more deliberate, the more stable forms of organization, the latter of the touch-and-go variety seen in crowds, mobs and temporary organizations of the more spontaneous sort. The relationship of collective behavior to leadership is discussed, showing the place of prestige, propaganda, and other factors. The qualities of leaders, both in primary and in derivative groupings, are analyzed. There is also a chapter on non-institutional controls such as fads, fashions, crazes and transient conventions and beliefs. This is followed by consideration of the place of institutional forms in social control, such items as customs, traditions, conventions, codes and rituals being discussed. The author indicates the growing objectification of institutions and attempts finally to indicate the place which science may play in the modification of institutions, especially in the way of making them more sensible and flexible.

At the close of the volume a splendid and thorough-going bibliography is appended.

What may one say in comment on this very exhaustive treatise? The dual approach of the author is sound. We can not construct a social psychology on the biological behavior of the organism alone: be it in terms of endocrine glands, as with Berman, or in terms of instincts as with McDougall, or be it in terms of individual differences in intelligence as with most present-day educational psychologists. This is only one side of the picture. Bernard has rightly emphasized the selective and all-important place of the environment, especially the social and cultural environment. Some persons may object to the author's invention of new terms, such as 'psycho-social' to describe this environment, and 'neuro-psychic technique' and 'neuro-psychic behavior' to describe the place of cortical functioning. Especially, also, some may object to the extended use of a rather threadbare concept like 'imitation,' especially by a man who wrote a 550-page book to expose the loose use and ill-starred fate of such a term as 'instinct' in the social sciences. Professor Bernard, however, rather carefully tries to avoid the terminology of the elder psychology without laying himself open to the criticisms of either current behaviorism or psychoanalysis. So, too, he attempts to circumscribe the meaning of 'imitation' putting it squarely, as do Humphrey and Allport, in line with the mechanism of conditioned responses. The only danger, as might also be said about the use of the term 'instinct,' is that the usual reader so easily forgets the specific meaning laid down by the author, and holds in mind the older, looser interpretation of these as sacred entities in the easy explanation of man's social behavior.

All new and uncharted sciences like social psychology or sociology suffer for want of clear, concise terms, largely because the basic concepts in which to describe the phenomena are themselves loose and undetermined. The older phases of both psychology and the social sciences must have patience with the latest arrival in their fields until more adequate observational and experimental data have been acquired. Only then can an adequate, logical vocabulary be established.

University of Wisconsin KIMBALL YOUNG

Das Recht der Dschagga. By BRUNO GUTMANN. Munich, C. H. Beck'sche Verlagsbuchhandlung, 1926, pp. 778.

The book represents the seventh publication in the *Arbeiten zur Entwicklungspsychologie* edited by Felix Krueger. Bruno Gutmann, who is also the author of the sixth number (*Amulette und Talismane bei den Dschagganegern am Kilimandscharo*), has already demonstrated his intimate acquaintance with Eastern Africa by several ethnographical and sociological studies. As a missionary he gained in a period of about 20 years, a thorough knowledge of the languages and the history of various ethnological groups in the former German East Africa.

The present work deals with the law of the Dschagga. The Dschagga, a Bantu tribe of about 100,000 people inhabit the southern and southeastern slope of the Kilimanjaro. There are 31 "officially recognized chieftainships" which consist of about 400 independent sibs. In the 69 chapters of his study Gutmann discusses the various legal forms in existence. He devotes the main part of the book to a description of the "norms concerning blood relationships" and the "norms concerning landed property." In the first 17 chapters, 'laws' related to topics of the following kind are discussed: marriage and marriage customs, betrothal, obstacles to marriage, divorce, illegitimate children, minors, responsibility of children, exclusion from the sib, revenge for homicide, and 'blood unions.' In the next 20 chapters there is a very detailed account of legal norms concerning taxes, loans, debts, securities, lost property, markets, canals, and compulsory service. Six chapters are devoted to 'stratification norms,' *i.e.* the rights of the chieftains and of delegates, and the norms regarding plundering expeditions. An additional 6 chapters deal with injuries inflicted on a person's honor, intentional and unintentional killing, theft, and arson. The last part of the book contains an accurate account of the various legal procedures and ordeals.

The present publication is supposed to be a contribution to the *Entwicklungspsychologie des Rechts.* Its lack of methodological, sociological, and psychological discussions is striking. An overwhelming number of facts is recorded with utter care. Nevertheless, Gutmann's study means more than a minute representation of legal norms. Here where the danger of a mere enumeration of 'objective' norms is very great, we find that the author has done more than to present a 'system of law' which in its objectivity is detached from other cultural manifestations. He shows the organic connection of law with religious, social, political, and economical conditions. By talking into account such conditions it becomes possible to trace the genesis of legal norms psychologically. The author does not work with logically simplified 'objectivations' like 'religion,' 'political organization,' 'social structure,' and 'economical life.' He shows in concrete cases in what way these various factors constitute special features in the *Gestalt* of the historically developed life of a group. Thus, an understanding of the genesis and content of legal norms is arrived at by seeing them in relation to other cultural forms. This 'seeing' is not a comparison of inductively ascertained facts; it is in the case of legal norms nothing but faithful observation of *one* aspect of the *Kulturganze*. The phenomenology of the *Kulturganze* is, however, a prerequisite for the appreciation of the one aspect. We see how legal forms develop by seeing the Dschagga in his relation to disease and death, plants and animals, family and sib. We get a vivid picture of the interplay of the various factors influencing the Dschagga people, but as to the question of 'primary' and 'secondary' factors no philosophical and theological abstractions are made. The legal aspect is represented as one among others. At the same time it becomes clear that the isolated 'individual' to whom legal theories ascribe rights and duties does not exist. Nor does an abstract 'society' exist, but a *Lebensganzheit* with superindividual legal norms of an irrational character.

Undoubtedly monographic studies such as that of Gutmann will be basic for *Entwicklungspsychologie*. We do not get, however, any suggestions from the author as to how the psychological analysis should proceed on such a phenomenological basis. The book itself sufficiently demonstrates the fact that a fruitful analysis has not to start from the 'psychic experiences' of primitives since the individual is not *subjectum agens* of society. In fact, the primitive 'individual' can only be of rather slight importance to the *Entwicklungspsychologie* of law and religion, etc. The fundamental question is, what are the constituents of primitive mentality as they can be singled out in various cultural (legal, mythical, and religious etc.) forms? Detailed analyses—which have, of course, nothing to do with an analysis of the 'psyche' of primitives—have to bring out the typical ways of primitives to 'structurate' the world mentally; analyses which have to show how, in the primitive *Weltbild*, 'objects' are defined and 'attributes' of objects enter various relations, how interrelations between objects are established by the various forms of the causal nexus, and how the proper place of objects is determined by characteristically primitive modes of measuring, numbering, and relating things spatially and temporally. Such detailed psychological (not intrapsychic) analyses on a broad descriptive basis are a desideratum. They are highly valuable for the understanding of the particular character and development of legal, religious, and other forms.

The *Nachwort* by Felix Krueger (pp. 44, which may be obtained separately from the publisher) points out the inadequacy of Wundt's approach in *Das Recht* and stresses the 'organic' point of view in the interpretation of cultural manifestations.

Columbia University HEINRICH KLÜVER

Lehrbuch der Sprachheilkunde. By EMIL FRÖSCHELS. Leipzig and Wien, Franz Deuticke, 1925. Revised edition, pp. xxi, 530. 114 figures, 5 plates.

The new edition of Fröschel's book on the art of correcting speech defects is a fuller exposition of his excellent work published in 1912.

It shows a scholarly and exhaustive understanding of the subject and is much needed in a field where there is such a paucity of real scientific writers.

Fröschel treats in general the various classes of speech disorders and is particularly illuminating in explaining the different devices for measuring the musculatures of speech.

His treatment of the disorder commonly known as stammering and stuttering varies from the methods employed by other German writers—particularly Gutzman. He does not go to the extreme in psychology as do the Freudians, nor hold a pathological view of 'nervousness' as is done by many specialists. In discussing the etiology of the subject, he holds that the disorder, being both psychogenetic and psychophysical in its reflexes, it works in a vicious circle of symptoms. Heredity is blamed only for various anomalies of the cortical speech mechanism. The nucleus of the trouble lies in the psychic condition of the patient, who becomes conscious of the ataxically disturbed speech movements. In the majority of cases there is a disproportion between speaking and thinking. This causes a constant upset, hence a weakening of the syllabary coördination apparatus; in other words, a coördination neurosis.

The generation of sthenic emotions gradually upset the entire speech mechanism and hysterical syndroms appear.

The author suggests three different periods of the disorder. The first stage is that of the single repetition of sounds and syllables between the ages of four and seven. The second period is one of exaggerated, conscious motions of the organs associated with speech, between the ages of six and ten. This stage gives birth to the anxiety neurosis which, of itself, may lead to compulsory movements of the articulatory organs. The third period in the development of the defect occurs any time after the previous stage and may be described as tonic cramp of the articulatory organs and other associated muscles. It is admitted that the neuropathically predisposed child may be more easily subject to such an ataxia and, that, in his case, it will last much longer when further exciting ages arise to upset his speech.

Ithaca, N. Y. FREDERICK MARTIN

Experimental Psychology: Loose Leaf Laboratory Manual. By A. T. POFFENBERGER. Chicago, C. H. Stoelting Co., 1926, 48 experiments.

Professor Poffenberger's loose-leaf laboratory manual for experimental psychology is designed for the elementary level. Forty-eight experiments are presented. The author emphasizes experiments that bear upon the learning process. Motor aspects are stressed at every possible opportunity. Observation of behavior assumes first place, although introspective data are not ignored. The order of experiments is left quite elastic so far as any one using the manual is concerned. Among the initial experiments are those on habit formation, practice in puzzle solution, learning to recognize objects, transfer of practice and interference caused by practice. Memory is taken up next. Then come such types of experiments as those on association, imagery, reaction-time and perception. Experiments are included on aesthetic judgment of form and color harmony, judgment of human character from an examination of the features, judgment of emotional states from facial expression, and muscular fatigue.

This manual appears to have been written with the needs of students of educational psychology primarily in mind. No experiments are included on such topics as perimetry of vision, after-sensations, or color mixture, for example. The manual contains a number of experiments that serve as desirable supplements to the classical experiments in psychology.

University of Vermont PAUL C. SQUIRES

NOTES

Some Notes on Behaviorism

The quotations in the present note are taken from Dr. J. B. Watson's article on Behaviorism in Vol. I, of the new volumes of the 13th ed. of the *Encyclopaedia Britannica*, recently published.[1]

To one of the questions proposed by Dr. Watson—"Do we think only in terms of words?"—the present writer feels that he can return a definite answer. He clearly remembers that when he was only about 6 months old he had a certain thought in his mind which he could not put into words at all at the time, but could only translate into action—which he promptly did.[2] The possibility of this recollection being correct and of the episode having occurred objectively at this early age is confirmed by the writer's subsequent observation of the behavior of his little niece. This child had occasionally been given small quantities of honey and at the age of 5½ months she could identify the jar which contained the honey on the table. Whenever it was raised she cried out and displayed very definite and marked signs of wanting some of the contents. This did not apply at all when any other somewhat similar looking object on the table was raised. It seems only reasonable to suppose that the child had some thought in her mind which would have been expressed as "I want some of the stuff inside that object" if she could only have used words at the time. Why should the possibility of thought step in suddenly, and quite adventitiously as it were, just when words are invented, or are capable of being used, and how and why did the first spoken word come to be invented and uttered if the person concerned had had no thought whatever before inventing and using this word? However we need not bother any further about such a fantastic possibility for the writer's clear recollection of his own early thought before he could speak, referred to above, finally disproves it and also finally and completely disproves the behaviorist view that "thinking is merely talking."

Of course it is possible that the use of words to represent things and ideas in a greatly shortened and more concise form may enormously facilitate thought, and extend its range, in the same way that the use of a system of shortened symbols enormously facilitates and extends the range of mathematics. This possibility, however, is not by any means certain although it appears to be so at first sight to adult civilised consciousness. It is quite possible that a savage in a primitive tribe which had not yet invented the use of words might be able to think and act very rapidly and extensively indeed, on certain subjects, without using words at all; and that the same may apply to dogs for instance at the present day.

As long as an organism has a clear idea of the objects involved, words may very likely not be *nearly* so necessary to very rapid, effectual, and complete thought as we speaking adults may often suppose. Of course the utility of words for communicating ideas to other individuals is an entirely different matter. Much adult thinking certainly appears to be in this talking form, and a very great deal of this particular form of thinking is, doubtless, really talking concealed, or repressed, by the operation of the intrapsychial 'censor.'[3]

It can only be lack of knowledge, or the presence of very heavy repressions, which can lead any speaking adult to suppose for a moment that "thinking is merely talking, but talking with concealed musculature." Dr. Watson's idea is so fantastic that the present writer wonders how anybody can hold it even for a moment—it has so clearly placed the cart before the horse. Where does the *talking* come from then? Surely it comes from thought, but admittedly it would appear that it does not always come from clear thought amongst the behaviorists. In their case it would appear that the production of a large amount of talking, and presumably of writing and literature as well, is not preceded by the slightest amount of thought for their own statement of their position quoted above is to this effect. Can it be wondered at, then, that they produce some curious literature?

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[1] *Op. cit.*, 345 ff.

[2] Cf. E. P. Farrow, Eine Kindheitserinnerung aus dem 6. Lebensmonat, *Int. Zsch. f. Psychoanal.*, 12, 1926, 81 ff.

[3] E. P. Farrow, A method of self-analysis, *Brit. J. Med. Psychol.*, 5, 1925, 118.

It would be very interesting to know exactly how and why the behaviorists come to ignore the striking phenomena of thought and consciousness in the world. "The Behaviorist takes the position at the outset that the total behavior of man from infancy to death is the subject matter of (human) psychology. Behavior can be observed like the phenomena of all other natural sciences, *e.g.* chemistry, physics, physiology, or biology. The same general types of methods used in the natural sciences can be used in behavior psychology. So far in his objective study of man no behaviorist has observed anything that he can call consciousness, sensation, perception, imagery or will. Not finding these so-called mental processes in his observations, he has reached the conclusion that all such terms can be dropped out of the description of man's activity."

Doubtless "the behavior of man is the subject matter of (human) psychology," but this is a very different matter from saying that it is the *whole* of the subject matter of human psychology. Dr. Watson seems to imply the latter proposition; but he apparently dare not say so explicitly for if he did it is clear that nobody would agree with him. It is obvious that something very important would be excluded. It is doubtless true that "so far, in his objective study of man, no behaviorist has observed anything that he can call consciousness, sensation, perception, imagery or will." That is not to say, however, that these latter things do not exist and can therefore be ignored. While we cannot directly observe another person's consciousness, he can inform us of his various states of consciousness, and of his various thoughts; and we can observe (*i.e.* record) all his various statements about these. Such statements are objective scientific phenomena in relation to a behaviorist observer. Why should they, therefore, be ignored?

There seems to be a craze for applying the methods of one objective science in the department of another. Admittedly a biologist who is used to describing, and endeavoring to understand, objects which he can see and often actually handle is apt to be very suspicious of the electrons and protons of the physicists, for example, which nobody has actually seen, and whose existence is merely inferred from certain effects on evidence more or less incomplete. The biologist may have a shrewd and unwelcome suspicion that, one day, much of the present electron theory may possibly go by the board in much the same way that the former necessity for a luminiferous ether has suffered badly under the theory of relativity. Even in that event, the present stage of atomic theory would doubtless have served a useful purpose in leading to the discovery of something else.

Admitting all this, and admitting that consciousness in other beings cannot be directly observed as a biologist would like to do, yet he is directly aware of his own consciousness and thinking powers, and the objective behavior of other people corresponds in certain respects so very closely with his own behavior that he is absolutely forced to attribute to them a similar consciousness with similar, or greater, thinking powers. While the biologist is directly aware of his own consciousness and power of thought which are not inferences at all but are, on the other hand, the instruments with which he infers, the attribution of consciousness to other beings is admittedly pure inference. It is, however, so much the simplest explanation of which the biologist can think to account for the observed phenomena that, while it is admittedly an inference, it is nevertheless an inference to which even a biologist is reluctantly completely forced by the overwhelming weight of evidence.

This ignoring of the striking phenomena of consciousness and thought in the world is so remarkable that one can only think that the condition of mind which can do this must be of a pathological nature. It would be of value and interest to know exactly how such a particular pathological attitude of mind can arise and something about the structure and aetiology of the pathology. The matter is of great practical importance as well, for great harm may be done by the behaviorists if they attempt to put their theories into practice without realising the consciousness and capacity for suffering which other human beings and animals possess.

An injurious effect of this kind may often be produced by many of the behaviorists' experiments on small children. Suppose a child is given a 'conditioned reflex' of fear of a fish-bowl by showing it the fish-bowl and then striking a steel bar behind its head. It does not by any means follow that when the child's 'fear reflex' of the fish-bowl is 'deconditioned' by bringing the fish-bowl gradually up to the child while it is eating that therefore the harm caused to the child by frightening it with the steel bar in the first place is thereby removed from its mind, in the way Dr. Watson seems to imagine. Some of these particular experiments with small children may do far more harm than the experimenters realise. It is far too readily assumed that every so-called 'reflex' in a small child is an 'unconscious' phenomenon.

The serious and fundamental error or omission associated with the behaviorist views may also be of great practical importance from a national point of view if they ever came to be applied to the construction of an army, or to the conduct of industrial affairs. Fortunately, however, there is probably extremely little likelihood of this. There is good reason to suppose that an army constructed and organised internally solely on the inhuman and artificial behaviorist conception of humanity would immediately break up and disintegrate completely on receiving severe blows from an enemy. On the other hand, in the case of an army organised largely on the conscious and affective phenomena of patriotism and mutual affection, blows from an enemy only serve to weld it into a more compact and homogenous whole.

In industrial affairs too, the ignoring of the phenomena of consciousness and suffering would doubtless produce great disaffection and probably strikes as well. The latter, however, would probably be regarded as 'behavior' and might lead to a revision of the theory.

With regard, however, to the aetiology of the pathology of the peculiar behaviorist view in the minds of the behaviorists the present writer has no very definite evidence. It would seem to him that perhaps a narcissistic phenomenon (a blind believed following of 'a formal method of other sciences') may play a large part in the pathological structure. This, however, would hardly account for the extraordinary blinding to the wonderful fact of consciousness. For an explanation of this striking phenomenon one would expect to find something far more powerful in the minds of the behaviorists.

Nevertheless we cannot hope to know anything very definite about the pathology of their peculiar views until several behaviorists have been analysed. It is thus greatly to be hoped, in the interests of science and the future happiness of humanity, that some rabid member of the creed may in the near future have his mind investigated, straightened out, and 'put right,' by means of the penetrating analytic process.

A form of wish fulfillment may often enter—namely a strong "wish that human psychology could be so greatly simplified in this way." We certainly should not, however, allow such forms of wish-fulfillment processes to blind us to the otherwise obvious phenomena of external reality.

Apart from some of their scientifically very interesting observations and experiments upon the 'conditioned reflex' and other subjects which must, after all—although they may modestly deny it—have required a considerable amount of thought for their preparation, one must give the behaviorists credit for having realised that a very large amount of the ordinary adult form of thinking is really concealed talking. Many other people indeed, including the present writer, had, however, realised this a long time ago by observational work upon their own minds. Dr. Watson seems also to have realised by direct observation the important fact that 'taps' in infancy have a very important bearing indeed upon this concealing process, a fact which most psycho-analysts do not yet seem to have realised at all by means of following the analytic process.[4] The present writer realised the very great strength and importance of this particular mechanism in the case of his own mind two years ago by means of self-analysis.[5] Dr. Watson, however, does not seem to have a sufficiently detailed knowledge of the precise nature of the psychological and instinctive mechanisms at work in this particular repressive process, and he probably does not realise at all how extensively and profoundly the taps which he was himself given in infancy affect him at the present day, in the same way that such taps similarly affect every other individual who has not been deeply analysed. Clearly one thing which is the matter with the behaviorists is that they do not realise, from direct knowledge, what happens in the individual human mind before the individual is able to talk, and that they have no knowledge as to how the concealing process of this early talking, and of the thoughts which underlie and produce this early talking, is brought about. If they had this knowledge, they could not possibly hold the behaviorist view. The early simple form of thinking is enormously more primitive than the later acquirement of mere talking.

With regard to the question "Does thinking itself offer any problem?" the present writer can only say that, to him, at least, this question is absurd for it is obvious that it contains many problems which may possibly not be completely solved for thousands of years and perhaps, indeed, never. The early thoughts of a human individual, before he is able to speak, when viewed by means of subsequent psychoanalysis, are apparently largely concerned with such things as a desire to fulfill certain deep-seated longings, and the avoidance of those circumstances which produce fear. For example, the writer's early thought referred to above was concerned with violent opposition towards a certain interference with the feeding process. Psychoanalysis shows that the influences of these repressed early thoughts enter as factors or components into practically all the forms of later adult thinking. There appears to be no discontinuity about the process, and indeed one would not expect any such discontinuity in a biological matter of this kind. Adult forms of thinking are thus closely bound up with the satisfaction of instinctive desires in infancy. In their turn, the instinctive desires, as we understand them, doubtless go back a very long way in the vertebrate scale. Man is not to be separated off sharply from the rest of the animal kingdom in the way the egocentric behaviorists would have us believe by supposing that thinking does not offer any problem and that it is "merely talking." The results of certain experiments indicate that even frogs can think, and are possibly conscious, in a very simple way under certain circumstances—that is to say that this is much the simplest explanation of the frog's very peculiar behavior in the course of the experiments. In this connection, it may be pointed out that the essence of sound science is simplicity. The simplest explanations which can be found in our endeavor to account for all the observed phenomena are the ones which should naturally always be selected.

Spalding, England E. PICKWORTH FARROW

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[4] Cf. E. P. Farrow, On the psychological importance of blows and taps in early infancy, *Psychoanal. Rev.*

[5] *Op. cit., Brit. J. Med. Psychol.*, 5, 1925, 118.

THE PRESENT STATUS OF PSYCHOLOGY IN CHINA

Psychology as a natural science in China gives much promise.[1] At present, it is inseparable from education and in that connection its achievement is marvelous. "The coming of Dr. McCall to China as the director of Psychological Research in the National Association for the Advancement of Education in 1923 has given to the movement of tests and measurements in China a center of impulse and guidance that has already yielded unexpected results."[2] The famous "TBCF Scale System" and some useful inventions or discoveries that came out incidental to the scaling and standardization are the results.

"This scale system is a sort of eclectic one, representing as it does an attempt to embody as many as possible of the best features of the other leading systems, namely, grade scale, percentile scale, and age scale."[3] The four test units were named in honor of those whose contribution to scientific mental measurement has been of the most fundamental significance. T (a unit for measuring the total amount of a pupil's ability in some trait) is for Thorndike, the originator and teacher of scientific educational measurement and author of the first College Entrance Intelligence Tests, and for Terman, the author of the Stanford Revision of the Binet-Simon Scale and leading exponent of the age-scale system. B (a unit for measuring a pupil's rate of progress and hence his *brightness* in the trait in question) is for Binet, the creator, with Simon, of the first Intelligence Scale, and for Buckingham, the creator of the grade-scale system. C (a unit which automatically indicates a pupil's *classification* in a standard school in China) is for Courtis, an early pioneer in educational measurement and originator of practice tests; for Cattell, who, with Fullerton, laid the foundation built upon by Hillegas in constructing the first statistically satisfactory judgment scale; and in remembrance of CHINA where this unit was first devised and used as such. F (a unit for measuring *effort* or how much the pupil has learned in proportion to his native intelligence or capacity to learn) is for Franzen, Pintner, and Monroe, all of whom published, at about the same time, a practical mechanism for measuring achievement as related to capacity to achieve. This unit is used only when both an intelligence and an educational test have been given. All intelligence and educational tests were constructed, scaled, and standardized according to this system.

Some of the useful inventions and discoveries incidental to the scaling and standardization of the tests are: (1) a simple, generalized formula allowing for chance regardless of the number of alternatives; (2) a generalized formula for the inclusion of elements in alternatives such that full knowledge will be required before correct answers can be given; (3) a new method for constructing product scales such as composition and handwriting scales; (4) a method for the construction of a C scale for an automatic reading of a pupil's classification in a standard school; (5) a straight line equation for all C scales; (6) a simple and identical logarithmic equation for all B scales; (7) a new method for the construction of intelligence tests; and (8) a new and unusually simple formula for measuring the efficiency of a school and of a school system.

Articles appearing in *The Chinese Journal of Psychology*, *TheChinese Educational Review*, *Chung Hua Educational Review*, *Elementary Education*, *Secondary Education*, *The New Education*, *The Eastern Miscellany*, and *Science* are almost exclusively on tests, measurements and statistical treatments, although some original articles on child study are not infrequently met. There are a few experimental works on reading. *The Chinese Journal of Psychology,* which has four volumes now, is owned by the Chinese Psychological Society.[4] Professor Y. C. Chang is the president of the Society and, at the same time, the editor of the Journal. The National Association for the Advancement of Education has a section on Eductional Psychology, from which all achievements in that field come.

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[1] At the request of Professor Washburn, the author writes this note at the expense of competency. It is his wish that news of the work of Chinese psychologists may henceforth be furnished directly from China.

[2] W. A. McCall, Scientific measurement and related studies in Chinese education, *Bull. Chinese Nat. Assn. Advancement Educ.*, 7, 1923, 2 ff. Foreword by P. C. Chang.

[3] *Ibid.*, 6. Cf. also McCall's *How to Experiment in Education*, 1923.

Psychology departments are found in Yen-Chin University, National University of Peking, Peking National Normal University, Peking Women's College in Peking; Nankai University in Tientsin; Teachers College in Wuchang; National Southeastern University in Nanking; Fu-Tan University in Shanghai; Amoy University in Amoy; and National University of Kwangtung in Canton. Although most, if not all, of them have some apparatus for demonstration and elementary laboratory courses, none has yet a regular laboratory building for research in experimental or animal work. Fu-Tan University in Shanghai is said to have built a Psychology Laboratory for Mr. Zing Yang Kuo, formerly of the University of California, but it is doubtful whether any research is carried on there.

To be a psychologist in China now is to be an all-round psychologist. While giving courses in general, educational, experimental, social, child, abnormal and comparative psychology, one is still entitled and required to devise tests, both 'intelligence' and 'educational.' Some of the earlier psychologists are T. C. Chen, Y. Tang (both of Cornell), T. T. Lew, Y. C. Chang, and H. C. Chen, all of Columbia. Mr. H. C. Chen and Mr. Ping Lin (the latter formerly of Stanford) are the only specialists on Child Psychology. Mr. Kong Wu was a student of William James. Mr. T. L. Wong did some important animal work in Chicago but has now quitted that field. Mr. C. W. Luh's research on "The Conditions of Retention" was considered by Professor Carr to be one of the best Ph.D. dissertations from the Chicago Laboratory. Mr. Luh is now recognized as one of the foremost, if not the foremost psychologist in China. Mr. Z. Y. Kuo started explicitly the anti-instinct movement in this country by his sensational article, "Giving Up Instincts in Psychology." Mr. T. C. Tu did work on vertical and horizontal reading of Chinese in the University of Iowa, and Mr. Hua I, formerly of George Washington University, is now performing some experiments on reading, using prisoners as subjects. Mr. Eugene Shen, a statistician, logician, and psychologist, who did important experimental work on the reading of Chinese by photographically recording eye movements at Stanford, is still in this country.

There appeared last year in the *Eastern Miscellany* two articles introducing the *Gestalt* theory. Some of the books translated into Chinese during the last ten or fifteen years and some of the original treatises on psychology published comparatively recently are as follows:

(1) *General Psychology*: Dewey, How We Think; Hart, Psychology of Insanity; Höffding, Outlines of Psychology; Poffenberger, Applied Psychology; Watson, Psychology from the Standpoint of a Behaviorist; Woodworth, Dynamic Psychology; Wundt, Introduction to Psychology.

(2) *Child Psychology*: Gaupp, Psychologie des Kindes; Kirkpatrick, Fundamentals of Child Study; Tracy, Psychology of Adolescence.

(3) *Educational Psychology*: Colvin, The Learning Process, Human Behavior; Edwards, The Fundamental Principles of Learning and Study; James, Talks to Teachers on Psychology; McCall, How to Experiment in Education, How to Measure in Education; Starch, Experiments in Educational Psychology; Thorndike, Educational Psychology (Briefer Course); Whipple, How to Study Effectively.

(4) *Social Psychology*: Ellwood, An Introduction to Social Psychology; LeBon, The Crowd, La Psychologie politique et la defense sociale,

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[4] Cf. this JOURNAL, 38, 1927, 487 f.

Les opinions et les croyances, Psychology of Revolution; McDougall, An Introduction to Social Psychology; Marbe, Grundzüge der forensischen Psychologie; Wallas, The Great Society, Human Nature in Politics.

(5) *Original Treatises*: Cheng, T. C., Outlines of Psychology (probably the oldest text); Kuo, Z. Y., Human Behavior, Vol. I (extreme behavioristic treatment); Luh, C. M., Psychology (the only textbook for the Senior Middle School), A Treatise on Social Psychology; Liao, S.C., Educational Psychology.

Stanford University SIEGEN K. CHOU

The Accuracy of the Method of Constant Stimuli

Culler[1] in his recent note on the method of constant stimuli has found that, in the fractionated data of Urban's[2] lifted weight experiments, there is a correlation between c and h, and on the basis of this concludes that these two variables are not independent as Urban stated, but are dependent. All of Culler's correlations are worked out in terms of ρ (Spearman's rank-correlation). If these had been calculated in terms of r (Pearson's product-moment correlation) there would have been considerable difference from the values which he has given. Table I gives some values of r and ρ calculated from various experiments where the method of constant stimuli was used. The first seven of these are from lifted-weight experiments and the remainder are in the visual-perceptual field. In the case of lifted-weights there is in general a high positive correlation between

TABLE I

LIFTED-WEIGHT EXPERIMENT

S_1		S_2		
r	ρ	r	ρ	
.998	1.000	.999	.943	Fernberger[3]
.973	1.000	1.000	.943	Fernberger[4]
.987	.996	.996	.996	Fernberger[5]
.984	.996	.991	.991	Fernberger[6]
.997	.750	.850	1.000	Fernberger[7]
−.352	−.620	.828	.250	Fernberger[8]
.999	1.000	.999	1.000	Fernberger[9]

VISUAL PERCEPTUAL EXPERIMENTS

S			S		
r	ρ		r	ρ	
.912	1.000	Fernberger[10]	.362	.500	Oberly[12]
.184	.500		.243	.900	
.419	.500		.164	.000	
.996	1.000	Fernberger[11]	.902	.900	Oberly[13]
.073	.500		.957	1.000	
.986	1.000		.600	.900	

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[1] Elmer Culler, The accuracy of the method of constant stimuli, this JOURNAL, 38, 1927, 307-312.

[2] F. M. Urban, Der Einfluss der Übung bei Gewichtsversuchen, *Arch. f. d. ges. Psychol.*, 29, 1913, 271-311.

[3] S. W. Fernberger, Table I, The influence of mental and physical work in the formation of judgments in lifted weight experiments, *J. Exper. Psychol.*, 1, 1916, 515.

[4] Table II, *ibid.*

[5] Table XI, On the relation of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 14, 1913, no. 61, 39.

[6] Table XII, *ibid.*, 40.

[7] Table I, An experimental study of the 'stimulus error,' *J. Exper. Psychol.*, 4, 1921, 71.

[8] Table II, *ibid.*

[9] Table III, *ibid.*

[10] Table VI, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 129.

[11] Table VII, *ibid.*

[12] H. S. Oberly, Table VII, The range of visual attention, cognition and apprehension, *ibid.*, 35, 1924, 342.

[13] Table VIII, *ibid.*

c and h although in one case there is a low negative one. In the visual-perceptual field there is a decided tendency towards a low correlation. Where there is a low correlation the value of ρ tends to be greater than r, in one case a difference of 0.657.

From the statistical theory of sampling we should be led to expect that c and h would remain fairly constant for a given observer, unless practice entered or there was a change of attitude on the part of the observer, but theoretically it is not clear what a high correlation between these would indicate, perhaps homogeniety of material or relationship between the variables.

Further if, as Culler contends, c and h are not independent but dependent, then the method of Least Squares breaks down, for our observation equations would take on the form

$$\begin{aligned} hx_1 - F(h) - \gamma_1 &= O \\ hx_2 - F(h) - \gamma_2 &= O \\ \cdots\cdots\cdots \\ hx_n - F(h) - \gamma_n &= O \end{aligned}$$

where

$$F(h) = c$$

or we would have

$$\begin{aligned} f(h, x_1, \gamma_1) &= O \\ f(h, x_2, \gamma_2) &= O \\ \cdots\cdots\cdots \\ f(h, x_n, \gamma_n) &= O \end{aligned}$$

and

$$\begin{aligned} h &= G(x_1, \gamma_1) \\ h &= G(x_2, \gamma_2) \\ \cdots\cdots \\ h &= G(x_n, \gamma_n) \end{aligned}$$

so that the most probable value of h might be either the arithmetic or the geometric mean.

Since the $\Phi(\gamma)$-hypothesis is directly dependent on the method of Least Squares it would seem that if there is dependence between c and h the whole method would have to be rejected. Further if c and h are dependent then has Culler not used quantities which were obtained in an improbable manner to establish this relationship?

University of Pennsylvania HAROLD M. LUFKIN

Two Pronounced Cases of Verbal Imagery

A number of years ago (1914) I discovered, among my students in a class in experimental psychology at the University of Oregon, a girl (M. M.) whose imagery seemed to be limited to the verbal type. During the past year, I discovered a man (W.A.), among my students in elementary psychology, whose imagery type seemed similarly limited. These two cases are so marked and unusual that I think it advisable to make them a matter of record.

M.M. was of the pure verbal-motor type.[1] The heard word had meaning to her only as she repeated it. She never took notes during a lecture, for she found it impossible to write and at the same time to repeat the lecturer's words. She had to devote her entire effort to the repetition of the words as they fell from the lecturer's lips. She even had to repeat the words spoken to her in a conversation if she wished to understand what was said. When she failed to pronounce a word, it was as if that word had been spoken in a foreign tongue.

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[1] For a description of another case of this type cf. S. W. Fernberger, Possible effects of the imaginal type of the subject on aphasic disturbances, this JOURNAL, 30, 1919, 330 f.

Tests that I gave her showed that she comprehended the spoken word, just as the average reader comprehends the written word, by way of throat kinaesthesis. In other words, she 'heard' as the average person 'reads.' In one of the tests I had her protrude her tongue and hold it between her teeth, so as to inhibit in a measure the verbal movements accompanying the heard word, and then I read sentences to her—which fell within the range of her memory span—at various rates of speed. At the moderate and slow rates she was able to reproduce the sentences, but at the rapid rate she failed. In another test I read a sentence to her while she was rapidly repeating a word aloud. Under these conditions she either failed to comprehend what was read or her rate of repetition slowed down noticeably.

As might have been expected, she read kinaesthetically; and for a motor reader she read at an extremely rapid rate. It is possible that the practice effect in 'motor hearing' carried over to the reading.

M.M. was sensitive regarding her 'defect,' as she looked upon it. She had long been aware that she differed from other people, having first realized it soon after she started to grammar school; but until she took psychology she did not know just how she differed.

As far as I was able to determine, she lacked visual and auditory imagery and was of the pure verbal-motor type. If she was under a handicap because of this, she was successful in overcoming it. Her university record showed no failures, and she was graduated with her class with marks above the average.

W.A.'s images were verbal like M.M.'s, but of the auditory type. Unless the 'heard' or 'read' words were accompanied by auditory imagery they were meaningless symbols to him. He comprehended conversations and lectures, or words read or written, through auditory verbal images. The 'word' to be understood, had to be imaginally 'heard.' Even W.A.'s thinking was done to a great extent in auditory-verbal images.

The knowledge that he differed from other people came to him, as to M.M., at an early age. He says, in a carefully prepared report on his type of imagery, that "I first noticed that my reading differed from the average person's when my cousin commented on the fact that she could read much faster to herself than out loud. As I had never noticed any difference in speed between oral and silent reading I tested it and found that there was no difference for me. I was about 9 yrs. old at the time."

W.A.'s imaginal type did not handicap him in grammar school because he had sufficient time to cover small assignments, but as he advanced to the higher grades, to high school, and to college, it became increasingly difficult for him to do the required work. He went through lectures, recitations, and hours of study under a constant surge of auditory-verbal images, which were at times as vivid as hallucinations. "The perpetual sounds of words in my head," he writes, "produced headaches, which became more and more severe and frequent as the number of lectures and the length of the class-room assignments increased."

In his senior year in high school W.A. made, at the suggestion of his teacher in classics who became interested in his case, a serious attempt to change, or at least to supplement, his type of imagery. He tried to read visually, but with no success. "Verbs, prepositions, adjectives, and adverbs," he reports, "meant absolutely nothing to me, and only a few of the nouns and pronouns carried meaning." If he was to graduate, he had to cover his assignments; consequently his attempt to modify his imagery was short-lived. The only effect of the attempt was to intensify his feeling of difference.

He found, when he came to Cornell, that the assignments were too long to be done by his usual methods of study, and so he appealed to his class advisor—a member of the department of psychology—for assistance. At the suggestion of his advisor he again attempted to alter his type of imagery. He counted aloud so as to drown the sounds of his auditory images, and at the same time he attempted to visualize as he read. It was of no use, however, for he could not understand what he read in this way. Though he made an earnest effort, the attempt to modify his imagery again failed.

The din of the auditory images which always accompanied his verbal perceptions became so intense that he seldom passed a day without a headache. The headaches became progressively more frequent and more severe until—just before he decided to leave college—he found it impossible to study for more than half an hour at a time.

W.A.'s images were almost exclusively of the auditory-verbal type. Visual images were rare in his experience, and when they did occur they were fleeting and unclear. I was unable to tell whether he had motor images; if he did, they did not bulk large in his mental life. He localized the auditory-verbal images within his head, and described them as high in pitch.

Cornell University KARL M. DALLENBACH

MR. GUNDLACH ON "SOME DIFFICULTIES WITH WEISS'S BEHAVIORISTIC POSTULATES"

I do not suppose that Mr. Gundlach expects me to meet his criticism of my postulates.[1] However, a general statement of my own reason for not doing so may interest the readers of this JOURNAL. If Mr. Gundlach's point of view is adequately expressed by his quotation from Bentley (IIC); it (psychology) "seeks to describe and to understand experience and the activities of the total organism in which experience plays an essential part," and if my point of view is expressed by 'psychology studies the development of those movements, from infancy to maturity, which make the individual a coöperative unit in a social organization' then the futility of our trying to reach any agreement seems obvious. For me it is immaterial whether or not (IIC) "psychology has its own definite problems and point of view which are not to be confused with those of other sciences." On the contrary I hope that some day psychology will be a part of Science with a capital S, as Max Meyer has recently expressed it. Nor do I see any more objection to making "psychology the handmaid of the social sciences" than the mathematicians object to having mathematics regarded as the handmaid of all Science. On the other hand, the traditional psychology which Mr. Gundlach represents does insist upon "its own definite problems and point of view" and does object to the 'handmaid' principle. We also differ widely on the principles of criticism. Mr. Gundlach avoids "subjecting himself" to what he calls "the criticism of internal inconsistency which less explicit authors happily escape." This is the atmosphere of clever debating, not that of science. As one of the "too explicit authors" I prefer not to engage in a debate. It seems to me that a most eloquent refutation of my postulates would be the formulation of an alternative set. This would leave the scientific reader free to chose between them.

Ohio State University A. P. WEISS

A REJOINDER

Through the courtesy of Professor Weiss and of the Editors of the JOURNAL I have been given an opportunity of appending a rejoinder to his 'reply,' but there is no need nor occasion for me to say anything as Professor Weiss does not touch upon my criticisms. He merely states why he does not reply. Criticisms, however, are not met by ignoring them, nor are internal inconsistencies justified by a refusal "to engage in a debate."

University of Illinois RALPH GUNDLACH

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[1] Ralph Gundlach, this JOURNAL, 38, 1927, 469-475.

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By Harry Helson, University of Kansas

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